# Historical Supplier Performance and Strategic Relationship Dissolution: Unintentional but Serious Supplier Error as a Moderator

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## INTRODUCTION

Manufacturers are frequently advised to build and maintain strategic relationships with critical-component suppliers (Sheth and Sharma 1997). These strategic buyer-supplier relationships are *long-term partnerships intended to yield benefits to both parties* (Monczka et al. 1998). The advice has merit, given the abundance of evidence attributing improved operational and financial benefits to such relationship arrangements (Dyer 1996; Carr and Pearson 1999).

For critical-component suppliers, performing well in supplying their customers over time, not just once, is expected to ensure relationship continuity (Shin et al. 2000; Abdul-Muhmin 2005). Yet, expecting these suppliers to operate error-free on a continual basis is not realistic (Hibbard, Kumar, and Stern 2001; Craighead et al. 2007). In complex supply networks, unintentional supplier errors are inevitable "normal accidents" (Perrow 1984), with some becoming costly supply failures (Primo, Dooley, and Rungtusanatham 2007), supply chain glitches (Hendricks and Singhal 2003), or supply chain disruptions (Craighead et al. 2007). An unintentional but serious supplier error, as such, is *an unplanned honest mistake whose chance occurrence results in negative operational and financial consequences for both the critical-component supplier and the manufacturer being supplied*. For example, Takata Corporation, a supplier of airbag inflators, initiated a design change in 2001 to replace a tetrazole-based airbag inflator propellent with an ammonium-nitrate based propellent; this design change had been initiated to allow smaller lighter inflators to be more safely manufactured and was not pursued with the intent to cause injuries or fatalities from airbag failures (Tabuchi 2014). This type of error (i.e., design change) differs from deliberate supplier misbehaviors (e.g., opportunism, unethical behavior) that intend to benefit the supplier at the expense of the manufacturer.

Our inquiry concerns unintentional but serious supplier errors and how they directly affect dissolution of strategic relationships, as well as moderate the association between prior supplier performance and strategic buyer-supplier relationship dissolution. While the association between *intentional* supplier misbehaviors and relationship termination has been established in the literature (Ganesan et al. 2010; Wang, Kayande, and Jap 2010), similar endeavors to investigate how and why *unintentional but serious* supplier error may or may not relate to relationship termination are rare. Research in services suggests that the effectiveness of mitigation schemes varies depending on the intentionality of the service failure (e.g., Iglesias, Varela-Neira, and Vázquez-Casielles 2015). Similarly, research in accounting suggests that bookkeeping errors versus deliberate misstatements elicit different public reactions (e.g., Hennes, Leone, and Miler 2008). As such, prior findings pertaining to the association between intentional supplier misbehaviors and relationship termination may or may not apply for unintentional but serious supplier errors.

To investigate the role that an unintentional but serious supplier error plays with respect to strategic buyer-supplier relationship dissolution, we conducted a scenario-based role-playing (SBRP) experiment in which a manufacturer sources a critical component from a strategic supplier. In this SBRP experiment, the supplier error (i.e., the unintentional shipment of defective units of a critical component with detrimental consequences) – the main experimental factor of interest – is dichotomous in nature; its manipulated level is unambiguous with respect to unintentionality or severity. Notably, its level is not the perceived outcome of other antecedent factors (e.g., bilateral trust), excluded from or controlled for in our SBRP experiment. The detected statistical effects of this factor are, therefore, attributed to its presence and not to its perceived levels.

Besides hypothesizing the direct effect of an unintentional but serious supplier error on strategic buyer-supplier relationship dissolution, we also engage a strong inference approach (Platt 1953) to hypothesize its moderation effect to be either negative or positive, depending on whether an *assimilation effect* or a *contrast effect* dominates. These two effects – *assimilation* versus *contrast* – are cognitive biases pertaining to how a new stimulus (in our case, an unintentional but serious supplier error) is evaluated relative to an established standard (in our case, historical supplier performance). An *assimilation effect* (or *contrast effect*) dominates when a new stimulus is deemed to be less discrepant (more discrepant) from an established anchor than it really is.

Data for our empirical test were collected from 256 sourcing professionals participating in our SBRP experiment in face-to-face sessions in nine US-based locations. After verifying the realistic nature of the SBRP experiment, ensuring that experimental factors are properly manipulated, and providing evidence as to the absence of Hawthorne and confounding effects, we fitted a general linear mixed-effects model to the data with appropriate controls. Our results indicate, as expected, that when a critical-component supplier with either stellar or marginally-acceptable historical performance errs, its likelihood of being terminated by the manufacturer it supplies increases. More importantly, this relationship is positively moderated by an unintentional but serious supplier error, in support of a *contrast effect*. For the same error, the supplier with stellar historical performance experiences a greater increase than its counterpart with marginally-acceptable historical performance. This harsher reaction, which we label the *positive supplier performance penalty effect*, is robust across conditions of prior supplier involvement in joint product development activities and the availability of attractive substitutes

in the supply market. These findings make two theoretical contributions to the literature on buyer-supplier relationship dissolution.

First, that an unintentional but serious supplier error adversely affects relationship continuity adds novel insight and effectively complements results regarding supplier opportunism and ethical violations (Ganesan et al. 2010; Wang, Kayande, and Jap 2010). To preserve a strategic relationship, a critical-component supplier must not only avoid opportunistic or unethical actions that exploit the manufacturer it supplies but should also minimize unintentional supply errors that harm the manufacturer. When an unintentional but serious supplier error occurs, the manufacturer's propensity to exit the strategic relationship with the supplier increases, regardless of prior supplier performance. This finding has not been previously reported in the literature.

Second, our detection of a robust *positive supplier performance penalty effect* suggests that suppliers with stellar historical performance should be even more attentive to minimizing unintentional but serious supplier errors. For these suppliers, a history of stellar performance is a necessary but insufficient condition for relationship continuity. Unintentional but serious supplier errors not only deteriorate the strategic relationships between suppliers and manufacturers, regardless of prior supplier performance, but they also weaken the association between stellar historical supplier performance and relationship continuity to a greater extent than for suppliers with a history of marginally-acceptable performance. In this respect, the robust *positive supplier performance penalty effect* detected in our analysis explains when, how, and why the deterrence to relationship dissolution typically engendered by stellar historical supplier performance does not hold. This finding qualifies conventional wisdom regarding the

positive association between supplier performance and strategic relationship continuity with customers being supplied.

Pragmatically, manufacturers should be aware of falling into a trap wherein their expectations of supply performance are subconsciously elevated over time; this escalation eventually biases them to prematurely terminate critical-component suppliers, regardless of prior supply performance. Suppliers, in general, should pay attention to escalating manufacturer expectations. Those with a history of stellar performance should be especially vigilant and proactive in managing escalating supply performance expectations that consequently expose them to greater chances of committing unintentional but serious errors. Equally important, when unintentional but serious mistakes do occur, critical-component suppliers, regardless of prior supply performance, should marshal mitigation resources quickly and visibly to minimize harm and return to normal operating conditions. Suppliers with stellar historical performance should, in parallel, aggressively frame these honest errors as one-time, not-to-be repeated aberrations.

The remainder of our paper is structured as follows. We begin by reviewing relevant literature from diverse disciplines on factors that influence relationship continuity. Next, we develop and discuss our hypotheses. We then describe the design and validation of the scenario-based role-playing experiment for collecting field data. We present our hypothesis testing results, as well as post-hoc analyses to establish robustness and to rule out alternative explanations, before discussing these results in terms of theoretical contributions and managerial implications. Finally, we identify future research opportunities and offer concluding remarks.

## BUYER-SUPPLIER RELATIONSHIP DISSOLUTION

In the mid- to late-1980s, manufacturers began pruning their supply base in order to benefit from building strategic relationships with a smaller set of critical-component suppliers. These efforts

altered how manufacturers managed and related to these suppliers. Instead of arms-length transactional relationships with critical-component suppliers, manufacturers sought longer-term, mutually-beneficial partnerships developed through conscious planning, investment, and commitment (Sriram and Mummalaneni 1990). Bolstering these efforts were reported research findings (e.g., Carr and Pearson 1999) associating improvements in operational and business performance to strategic buyer-supplier relationships.

As accumulated evidence removed doubts as to the value of strategic buyer-supplier relationships, research attention then expanded to focus on identifying factors that influence continuity of such relationship arrangements (Sriram and Mummaleneni 1990; Abdul-Muhmin 2005; Wang et al. 2010). Notably, interest on this topic extended beyond the supply chain management discipline (e.g., Chen, Dooley, and Rungtusanatham 2016), with substantive insights also generated by research from management (e.g., Broschak 2004) and marketing (e.g., Ganesan et al. 2010), as well as such sub-disciplines of marketing as marketing channels (e.g., Ping and Dwyer 1992), relationship marketing (e.g., Giller and Matear 2001), and services marketing (Beverland et al. 2004). Moreover, while some research focused on factors contributing to relationship continuity (e.g., Anderson and Weitz 1989), others delved into factors relating to relationship dissolution (e.g., Baker et al. 1998), despite the recognition that one relationship state is de facto the converse of the other.

Synthesizing across the diverse knowledge bases and across scientific inquiries into either relationship continuity or relationship dissolution, we discern three groupings of factors investigated as antecedents of relationship continuity/dissolution or moderators of the association between antecedents and relationship continuity/dissolution): *entity-centric factors*, *relationship-*

*centric factors*, and *environment-centric factors*. Table A1 in the Appendix describes each the groupings and highlights exemplary research under each grouping.

Entity-centric factors signal ability and willingness of one entity to engage in developing and sustaining a strategic relationship with the other entity in the buyer-supplier dyad. For example, in a buyer-supplier dyad, how satisfied or dissatisfied the buyer is with supplier performance is associated with relationship dissolution (Ping and Dwyer 1992). Moreover, when one entity is not satisfied with the net benefits (of cost) it derives compared to the other entity, the likelihood of relationship termination increases (Helm, Rolfes, and Gunter 2006). Conversely, calculative commitment, which reflects a positive cost-benefit economic justification, encourages the buyer to continue its strategic relationship with the supplier counterpart under normal supply performance and, equally important, may buffer the strategic relationship against ethical or opportunistic lapses by the supplier (Ganesan et al. 2010). Similarly, affective commitment, which reflects a positive emotional justification, significantly reduces the likelihood of relationship dissolution but may unduly amplify the negative impact of supplier opportunism on relationship dissolution (Abdul-Muhmin 2005; Ganesan et al. 2010). Not surprisingly, other unethical behaviors, besides opportunism, influence relationship dissolution (Ganesan et al. 2010; Wang et al. 2010).

Relationship-centric factors characterize the nature of the relationship between buyer and supplier. For example, the amount of time the buyer and the supplier has been in a relationship (i.e., relationship duration) exerts different influences on relationship dissolution across stages of relationship development and evolution (Fichman and Levinthal 1991). As relationship duration correlates positively with trust (Morgan and Hunt 1994), the risk of relationship dissolution decreases as a consequence of increased trust (Anderson and Weitz 1989). The amount of asset-

specific investments in the relationship strengthens bilateral bonds, which reduces relationship dissolution by discouraging opportunism directly and moderates the negative effects of opportunism on relationship continuity (Ganesan et al. 2010; Wang et al. 2010).

Last, environment-centric factors pertain to dynamics outside of a buyer-supplier relationship and are beyond the control of entities in the buyer-supplier dyad. Chen et al. (2016), for example, analyzed events tied to the Firestone tire design and manufacturing flaws and documented the influences of media attention and the U.S. government on the Ford-Firestone relationship dissolution. The availability of supply alternatives (Sriram and Mummalaneni 1990) and market demand characteristics (Gadde and Mattsson 1987) are other environmental dynamics shown to be associated with relationship continuity/dissolution.

Our research evaluates supplier behavior with no *a priori* intent to inflict harm on the exchange partner. By doing so, we add an unintentional but serious supplier error to the grouping of entity-centric factors and seek to determine whether or not this factor exhibits similar effects on relationship dissolution as deliberate supplier actions to exploit the buyer. Our methodological approach aligns with Ganesan et al. (2010), collecting and analyzing experimental rather than recall data to better understand how and why this factor not only affects relationship dissolution but also alters the association between stellar historical supplier performance and relationship dissolution.

## HYPOTHESES

Figure 1 depicts the research model underlying our inquiry. Below, we first articulate the baseline effect between historical supplier performance and the likelihood of strategic relationship dissolution. This baseline association, while intuitive and with theoretical and empirical support, deserves discussion since it is the foundation of our inquiry. We then

hypothesize the effect of an unintentional but serious supplier error on relationship dissolution before engaging a strong inference approach (Platt 1964) to offer competing arguments as to its potential moderating effects (negative versus positive) on the baseline relationship.

INSERT Figure 1 Here

**Historical Supplier Performance and Strategic Relationship Dissolution**

How well a critical-component supplier performs is routinely analyzed as an ongoing part of the post-supplier selection evaluation process (Narasimhan, Talluri, and Mendez 2001). This evaluation yields insights into the pattern of operational capabilities the incumbent supplier possesses with regards to unit cost, product quality, delivery reliability and punctuality, and responsiveness to customer-initiated changes (Prahinski and Benton 2004). Pre-supplier selection, if a manufacturer knows that a critical-component supplier is only able to perform in a marginally-acceptable manner relative to expectations, then the supplier under consideration is not likely to be selected at the onset (lest it is the only available option).

Post-supplier selection and once a strategic relationship has been established, two scenarios are possible. When the selected critical-component supplier habitually exceeds performance expectations, the manufacturer continually experiences a cognitive state of confirmation (Oliver 1977) that develops into a high level of competence-based trust in the suppler (Das and Teng 2001; Liu and Ngo 2004). Confidence in the ability of the supplier to meet performance obligations, in turn, allows the manufacturer to leverage such a partner to contribute to its own operational performance. The manufacturer, as such, has no reason to consider a change in the sourcing relationship, *ceteris paribus* (Shin et al. 2000). Alternatively, when the incumbent supplier meets performance expectations but in only a marginally-acceptable manner, the manufacturer develops an average level of competence-based trust in the

supplier (Das and Teng 2001; Liu and Ngo 2004). Over time, the manufacturer is likely to become dissatisfied and to consequently consider exiting the partnership (Abdul-Muhmin 2005).

Hence, post-supplier selection, when a manufacturer is in a strategic (i.e., long-term, partnership-like) relationship with its critical-component supplier, the *ceteris paribus* baseline effect is as follows:

H1: Historical supplier performance and the likelihood of strategic relationship dissolution are negatively associated (i.e., inversely related).

**Unintentional but Serious Supplier Error and Strategic Relationship Dissolution**

A critical-component supplier, in the course of supplying the manufacturer, cannot guarantee complete avoidance of unintentional but serious mistakes (Hibbard et al. 2001; Craighead et al. 2007). When such an error occurs, the strategic relationship becomes tense (Holmlun-Rytkonen and Strandvik 2005). The manufacturer experiences stress because the supply uncertainty affects its ability to meet its performance obligations to its customers. To reduce this uncertainty, the manufacturer has to find near-term solutions to proactively buffer the supplier error from impacting downstream customers, or provide remunerations reactively to appease customers affected by the supplier error (Primo et al. 2007). These added expenses alter the cost-benefit equation justifying the initial establishment of the strategic relationship. Moreover, in mitigating the unintentional but serious supplier error, the manufacturer also begins to evaluate its trust in the supplier, whether deliberately or subconsciously. Integrity-based trust in the supplier should remain unchanged since the error is unintentional, but competence-based trust in the supplier should decline (Das and Teng 2001; Liu and Ngo 2004; Connelly et al. In Press). The elevated stress, increased mitigation expenses, and lowered competence-based trust leads the manufacturer to question whether to stay in the strategic relationship. Hence, post-supplier

selection when a manufacturer is in a strategic relationship with its critical-component supplier, we hypothesize *ceteris paribus* that:

H2: An unintentional but serious supplier error increases the likelihood of strategic relationship dissolution.

**Unintentional but Serious Supplier Error as a Moderator**

Besides a main effect, we posit that an unintentional but serious supplier error also moderates the baseline effect of historical supplier performance on relationship dissolution. Assimilation-Contrast Theory (Sherif and Hovland 1961) suggests this moderation effect is either positive or negative, depending on which effect – *assimilation effect* or *contrast effect* – dominates when a new stimulus (i.e., an unintentional but serious supplier error) is juxtaposed against an established anchor (i.e., historical supplier performance). An *assimilation effect* is a cognitive bias that deems the new stimulus to be less discrepant from an established anchor than it really is; in our context, it manifests as a tempering of the increase in the likelihood of strategic relationship dissolution when an unintentional but serious supplier error occurs. A *contrast effect*, on the contrary, is a cognitive bias judging a new stimulus to be more discrepant from an established anchor than it really is and manifests, in our context, in a contrary manner. Whether an *assimilation* or a *contrast effect* dominates, therefore, reflects how manufacturer expectations about continuing supplier performance are systematically distorted when a supplier error is evaluated against its historical supplier performance (Herr, Serman, and Fazio 1983; Herr 1986).

This systematic distortion, however, does not apply when a critical-component supplier with marginally-acceptable historical supplier performance errs (see Figure 2). For such a supplier, neither an *assimilation* nor a *contrast effect* dominates, with the supplier mistake exerting only a main effect on relationship dissolution as hypothesized in H2. The absence of

systematic distortion is reasonable because a history of marginally-acceptable performance safeguards the critical-component supplier from the manufacturer's escalating performance expectations over time. These supplier performance expectations, over time, may even settle at levels that are conducive to the forgiving of unintentional errors. Hence, when a critical-component supplier errs, the manufacturer, already desensitized, deems the mistake to merely add evidence not inconsistent with the history of marginally-acceptable supply performance. The manufacturer, while sufficiently stressed by the supplier error to elevate consideration of relationship termination, makes no additional adjustments, positive or negative, to its reaction.

INSERT Figure 2 Here

For a critical-component supplier with stellar historical performance, two scenarios are possible regarding systematic distortion of manufacturer expectations about continuing supplier performance. On one hand, when an unintentional but serious supplier error occurs, the manufacturer deems the mistake to be an isolated, temporary, and non-recurring anomaly (Ganesan et al. 2010) and effectively discounts its negative impact (Ganesh, Arnold, and Reynolds 2000). By doing so, the manufacturer cognitively judges the error to be less discrepant from the anchor of stellar historical performance than it actually is (Sherif and Hovland 1961). This consequent assimilation of the error reduces its main effect, which manifests as a lesser-than expected change in the likelihood of strategic relationship dissolution (see Figure 2: Panel A). Because of this smaller increase compared to the increase experienced by an incumbent supplier with marginally-acceptable historical performance experiences, the slope of the association between historical supplier performance and likelihood of strategic relationship dissolution becomes more negative. Hence, when an *assimilation effect* dominates, we hypothesize *ceteris paribus* that:

H3a: An unintentional but serious supplier error negatively moderates (i.e., accentuates) the inverse relationship between historical supplier performance and the likelihood of strategic relationship dissolution.

Alternatively, the manufacturer, observing the contradiction between the new stimulus and prior positive appraisals of the critical-component supplier, experiences incoherence because "what is observed does not make sense" (Wang et al. 2010). Juxtaposed against escalating performance expectations, the error-triggered contrast becomes magnified, with the mistake appearing to deviate in a more pronounced manner from the anchor of stellar historical performance than it actually is (Sherif and Hovland 1961; Ganesan et al. 2010; Wang et al. 2010). This contrast consequently produces a greater than expected increase in the likelihood of relationship dissolution. Because of this greater increase compared to that experienced by an incumbent supplier with marginally-acceptable historical performance, the slope of the association between historical supplier performance and likelihood of strategic relationship dissolution becomes less negative. Hence, when a *contrast effect* dominates, we hypothesize *ceteris paribus* that:

H3b: An unintentional but serious supplier error positively moderates (i.e., attenuates) the inverse relationship between historical supplier performance and the likelihood of strategic relationship dissolution.

## SCENARIO-BASED ROLE-PLAYING EXPERIMENT

Data to test the hypotheses were collected using an SBRP experiment with a repeated-measures design. An SBRP experiment deploys varying versions of a descriptive vignette to convey scripted information to human subjects about manipulated levels of one or more factors of interest and about factors to be controlled for (Alexander and Becker 1978). Subjects assume a

designated role within the descriptive vignette and react to the scripted information by recording their responses to questions that map to the experimental factors. These reactions are then analyzed to reveal how and to what extent subject responses vary with the manipulated levels of the factors of interest. The method is ideally suited for studying human judgments, preferences, and decisions within complex phenomena (e.g., relationship dissolution) that are difficult to observe in real time (Rungtusanatham, Wallin, and Eckerd 2011).

An SBRP experiment offers two specific methodological strengths over survey and case-based research designs given our underlying research questions and setting. First, the confidential and negative ramifications typically associated with strategic relationship dissolution present challenges in terms of either identifying a sufficient number of cases for case-based research or obtaining a sufficient number of survey responses for statistical analyses. By using an SBRP experiment, we circumvent having to obtain confidential firm-level information, while being able to collect a sufficiently large sample to permit statistical inferences. Second, relative to other research designs, experiments enable precise isolation and assessment of causal effects attributed to factors of interest (Campbell and Stanley 1963). By using a descriptive vignette for our SBRP experiment, we can manipulate the experimental factors that are hypothesized to relate to strategic buyer-supplier relationship dissolution while controlling for other potential explanations.

**Design Matrix**

Table 1 depicts the design matrix for our "four-factors, repeated-on-two-factors" SBRP experiment in which subject responses are recorded thrice (hence, Response 1, Response 2, and Response 3). This mixed design combines two between-subject factors and two within-subject factors into a single SBRP experiment. With each factor manipulated at two levels, our SBRP

experiment, therefore, involves only eight versions of a descriptive vignette (i.e., $I_{GS}$, $I_{SG}$, $J_{GS}$, $J_{SG}$, $K_{GS}$, $K_{SG}$, $L_{GS}$, and $L_{SG}$) and, yet, allows for statistical modelling of all possible main and interaction effects of the four factors.

INSERT Table 1 Here

We label the eight versions as $I_{GS}$ through $L_{SG}$ to avoid signaling a socially-preferred or ordering effect between the experimental factors of Unintentional but Serious Supplier Error (USE) and Supplier Substitution Availability (SSA). The subscripts, S and G, denote the sequence in which the two repeated factors of USE and SSA are presented. For SG-subscripted versions, USE is introduced first followed by SSA. Conversely, for GS-subscripted versions, the sequence between USE and SSA is reversed. To ensure the absence of an ordering effect, we compare the means of the response variable from the SG-subscripted versions ($I_{SG}$, $J_{SG}$, $K_{SG}$, and $L_{SG}$) to those from the GS-subscripted versions ($I_{GS}$, $J_{GS}$, $K_{GS}$, and $L_{GS}$) for Treatments 1, 2, and 3 and find no significant differences ($p = .62$, $p = .16$, $p = .69$, respectively) to indicate a sequencing effect with respect to USE and SSA.

Note that we did not implement a $2^4$ full-factorial, complete between-subjects design nor a complete repeated measures, within-subjects design for legitimate reasons. First, with four factors, a complete between-subjects design requires the creation of $2\times2\times2\times2 = 16$ versions of the descriptive vignette. A complete within-subjects design that counterbalances against order effects (i.e., guards against the order in which subjects are exposed to the levels of the four factors) requires creating 40,320 (i.e., 8! permutations) versions of the descriptive vignette. The mixed design we implemented, by comparison, requires only eight versions, each representing a different sequence of the four factors. Second, a within-subjects design has a sample size advantage over a between-subjects design and is better able to control for individual differences

in isolating the effects of experimental factors on the response variable (Greendwald 1976; Maxwell and Delaney 2004). In terms of sample size, our "four-factors, repeated-on-two-factors" mixed design, therefore, requires more subjects than a complete within-subjects design but fewer subjects than a complete between-subjects design. Third, for a given number of experimental factors, a within-subjects design requires greater time commitment than a between-subjects design. In the case of the latter, subjects receive one treatment, regardless of the number of experimental factors. In the former, subjects receive as many treatments as the number of factors multiplied by the number of levels of each factor, which lengthens the time to complete the experiment. Moreover, as the duration of the experiment increases, subject attrition due to fatigue and potential threats to internal validity due to, for example, sensitization and learning also increase (Greenwald 1976). Hence, with four factors, each at two levels, subjects receive $4\times2 = 8$ treatments. Comparatively, for the mixed design we implemented, subjects receive only three treatments to provide adequate data for hypothesis testing.

***Between-Subjects versus Within-Subjects Experimental Factors***

For our SBRP experiment, the two between-subjects factors are HSP: *Historical Supplier Performance* and PSI: *Prior Supplier Involvement in Joint Product Development* and the two within-subjects factors are USE: *Unintentional but Serious Supplier Error* and SSA: *Supplier Substitution Availability*. Of these, only HSP and USE are relevant for hypothesis testing.

We designate HSP to be a between-subject factor to anchor buyer expectations regarding future supplier performance on prior supplier performance in Treatment 1. As the buyer (i.e., subject in the role of the buyer) receives new information about a supplier error (i.e., USE), buyer expectations about supplier performance are consequently adjusted. This adjustment is determined by examining how the negative slope corresponding to the effect of HSP on strategic

relationship dissolution changes with the new information. We treat USE as a within-subject factor because the random assignment of subjects to versions isolates the main and moderation effects of USE without these effects being confounded with varying subject attributes (Greenwald 1976; Howitt and Cramer 2011). Moreover, since the manipulated level of USE always progresses from ABSENT to PRESENT, detectable effects of USE satisfy the temporal precedence and covariation conditions of causality and, hence, allow for stronger conclusions to be drawn as to its causal nature.

Though not hypothesized, we manipulated PSI and SSA in our SBRP experiment because both are known to affect relationship dissolution (e.g., Sriram and Mummalaneni 1990; Abdul-Muhmin 2005; Chen et al. 2013). Including PSI and SSA allows their effects to be partialled out when statistically isolating the effects of HSP, USE, and the HSP×USE interaction on relationship dissolution and, moreover, permits conducting post-hoc analyses to assess the robustness of our hypothesis testing results. We treat PSI as a between-subject factor and SSA as a within-subject factor to maximize the benefits of a mixed design that relegates two of four experimental factors to be between-subjects and the remaining two factors to be within-subjects.

***Experimental Factor Levels across Treatments***

The level of HSP is manipulated in Treatment 1 to be either STELLAR or MARGINALLY-ACCEPTABLE, with its level varying across the eight vignette versions (i.e., between subjects) but fixed beyond the first treatment within each version (i.e., within subject). PSI is similarly manipulated to cue either HIGH or LOW between subjects. The effects of HSP and PSI are determined by comparing responses across subjects.

USE and SSA are both manipulated to cue ABSENT in Treatment 1 and PRESENT in Treatment 3. In Treatment 2, either USE or SSA is manipulated to denote PRESENT while the

other remains cued as ABSENT. In this respect, as a subject progresses in the SBRP experiment from Treatment 1 to Treatment 2 to Treatment 3, the levels of these two within-subject factors change one at a time. The effects of USE and SSA are determined by comparing responses across treatments by subject.

**Descriptive Vignette Design and Version Generation**

To avoid writing a non-believable, unrealistic, and inadequately-constructed descriptive vignette (Wason, Polonsky, and Hyman 2002), we enlisted four sourcing professionals from the intended target population were to help develop the eight versions of the descriptive vignette. These sourcing professionals were employed, respectively, in food manufacturing, information technology hardware, logistics services, and consulting. They worked iteratively with the research team over a period of approximately three months to review and critique the descriptive vignette to ensure it reflected external reality.

We pre-tested the eight versions of the descriptive vignette for clarity of instructions and wording with five university professors from a private Midwest university, a public university in the Midwest, and a public university in the Southeast and with 12 advanced doctoral students from a public Midwest university. The five professors and 12 doctoral students have research and/or practical expertise pertaining to strategic buyer-supplier relationships; were randomly assigned to one of the eight versions; were asked to complete their assigned version; and, upon-completion, met with the research team to debrief answers and for the research team to successfully verify their understanding of instructions and experimental cues.

***Baseline Context***

The baseline context of a descriptive vignette ". . . is composed of written statements to provide contextual information that is intended to be invariant across varying versions of the vignette, as

well as written statements intended to yield information about control variables . . . ." (Rungtusanatham et al. 2011, p. 12). For our SBRP experiment, the baseline context references a hypothetical Fortune 500 technology firm with global presence (ELECTRONICS Inc.), whose Image Output Division designs, manufactures, and sells laser printers. The Image Output Division, moreover, sole-sources the Engine Control Module, a critical component, from ZENGINE. We modelled ELECTRONICS Inc. and the sole-sourcing policy after a real global manufacturer of laser printers; an author team member has deep contextual familiarity based on prior employment with this real laser-printer manufacturer. Subjects, when asked about the relationship in the descriptive vignette, affirm that they understood ZENGINE to be a sole supplier of a critical component to ELECTRONICS Inc.

***Contextual Controls***

The baseline context also includes statements to control for three salient issues (relationship duration, bargaining power, and supply market size) that theoretically and empirically affect how strategic buyer-supplier relationships evolve. Relationship duration affects relationship continuity in a non-linear manner, with the likelihood of relationship termination decreasing in the early stages of a relationship due to a "honeymoon effect," increasing beyond this honeymoon period before leveling off, and decreasing afterwards (Fichman and Levinthal 1991). Relationship duration, moreover, also impacts the degree of trust and commitment in relationships (Morgan and Hunt 1994), which in turn affects relationship continuity. To make relationship duration (and, indirectly, the degree of trust) invariant across experimental versions and treatments and, therefore, remove its effects on the response variable, we fixed the length of the relationship between ZENGINE and ELECTRONICS Inc. to be 12 years, which is longer

than the 7-year industry median. This information reinforces the long-term nature of the strategic relationship between ELECTRONICS Inc. and ZENGINE.

The relative bargaining power between two parties relates to the level of relative dependency in the dyad; the entity with more bargaining power is less dependent on a given relationship and, therefore, more likely to exit (Gulati and Sytch 2007). To remove this potential effect, the baseline context states that ELECTRONICS Inc. and ZENGINE have equal bargaining power in the marketplace. The two parties are, in this regard, equally dependent on one another (Crook and Combs 2007).

Finally, the size of the supply market affects relationship continuity (Sriram, Krapfel, and Spekman 1992). In a monopolistic situation wherein only one viable supplier is available, the buyer is unable to switch its supply source, becomes completely dependent on the incumbent supplier, and is more willing to continue its relationship with the incumbent supplier (Sriram et al. 1992). To avoid this potential effect, we included statements in the baseline context that there is more than one possible supplier for the Engine Control Module and comparing the performance of ZENGINE to the industry. ZENGINE, as such, does not have a monopoly in supplying the Engine Control Module.

We asked several questions to affirm subject understanding of the strategic relationship depicted in the descriptive vignette. Subject responses reveal that they understood ZENGINE and ELECTRONICS Inc. have equal bargaining power and how well ZENGINE performs relative to other potential suppliers in the industry.

***Experimental Cues***

Table A2 in the Appendix summarizes the experimental cues used to manipulate the factor levels in the experiment. HSP is cued as either MARGINALLY-ACCEPTABLE or STELLAR, using

statements relating the operational performance of ZENGINE (i.e., unit cost, product quality, delivery punctuality, and flexibility) to that of the industry. USE is cued as either ABSENT or PRESENT, using statements about a recent and major laser printer failure attributed to defective Engine Control modules that had been inadvertently supplied by ZENGINE. PSI is cued to be HIGH with statements indicating previous and recent efforts, as well as financial and engineering investments, by ZENGINE to jointly design laser printers with ELECTRONICS Inc.; PSI is cued to be LOW with a singular statement indicating that ZENGINE had never worked with ELECTRONICS Inc. on joint product development activities and, therefore, had not had opportunities to invest financial and engineering resources to support such activities. Lastly, SSA is cued either as PRESENT or ABSENT, with statements regarding the market entry of an attractive substitute supplier (GAMMA) offering better operational performance than ZENGINE and expressing interest in supplying ELECTRONICS Inc.

Since our research question centers on and to ensure that the unintentionality of the serious supplier is unambiguous, we conducted an in-class exercise with five male and four female graduate students pursuing a degree in supply chain management at a public university in the Midwest. The nine graduate students, at least 24 years of age with prior work experience, were provided with the same textual information about ZENGINE and asked whether or not the shipment of defective Engine Control modules was intentional. Eight answered “No” that the shipment was unintentional; one answered “Yes” erroneously because the individual had misread the question. The anecdotal evidence, as such, elevates confidence that the unintentionality of USE as manipulated is not ambiguous.

***Response Variable***

The response variable in our SBRP experiment, *Likelihood of Strategic Relationship Dissolution*, is operationalized with a three-question measurement scale. The three questions ask how likely (1 = Very Unlikely, 5 = Very Likely) subjects are to recommend that ELECTRONICS Inc.: (i) replace ZENGINE with another sole supplier for the Engine Control module, (ii) continue with ZENGINE as the sole supply source for the Engine Control module (reverse coded), and (iii) source the Engine Control module from another vendor, besides ZENGINE. Subjects are asked to provide a recommendation, as opposed to make a dissolution decision, because the decision to dissolve a strategic relationship often involves personnel across various functions within a firm. The measurement scale is reliable, with Cronbach's α (Cronbach 1951) of .80 (Treatment 1), .74 (Treatment 2), and .79 (Treatment 3). Factor analysis via the principal components method also reveal the measurement scale to be unidimensional across treatments, with factor loadings exceeding the .30 threshold suggested by Hair et al. (1979, p. 236).

**Procedure**

We contacted U.S.-based affiliates of the Institute for Supply Management (ISM) for permission to collect data from attendees at regularly-scheduled, monthly dinner meetings. The following nine ISM affiliates agreed: Boston, MA (78); Cincinnati, OH (25); Cleveland, OH (22); Des Moines, IA (19); Indianapolis, IN (31); Louisville, KY (11); Milwaukee, WI (11); San Diego, CA (26); and the Twin Cities, MN (33). The count of attendees participating at a particular location is noted in parenthesis. Because of sociopolitical, socioeconomic, and legal system differences across countries, we confined data collection to U.S. sites.

At each location, the meeting began with a factual recounting of the strategic relationship termination between Ford Motor Company and Bridgestone/Firestone Inc. This recounting ensured that attendees understood what a strategic relationship is and what strategic buyer-

supplier relationship dissolution means. To assess whether the recounting inadvertently influenced subject reactions and responses, an additional SBRP experiment using only the SG-subscripted versions was conducted at a tenth location (i.e., Detroit, MI), approximately one year after experimental data had been collected from the original nine locations. The Detroit meeting involved 17 attendees and began without mentioning the Ford-Firestone breakup. Comparing responses from the Detroit attendees to those from the original nine locations, we detect no differences in mean scores for the response variable for Treatment 1 ($p$ = .14), Treatment 2 ($p$ = .32), or Treatment 3 ($p$ = .42); these statistical results suggest that recounting the Ford-Firestone breakup at the start of the meetings did not unduly influence subject reactions during our SBRP experiment. Note that the experimental data from the 17 Detroit attendees were excluded in subsequent analyses because of the one-year time lapse in data collection.

Each attendee of the nine original locations was randomly assigned to one version as follows: $I_{GS}$ =33, $I_{SG}$ = 36, $J_{GS}$ = 27, $J_{SG}$ = 36, $K_{GS}$ = 29, $K_{SG}$ = 31, $L_{GS}$ = 32, and $L_{SG}$ = 32. Attendees consenting to participate in our SBRP experiment were instructed to: (i) assume the role of an experienced purchasing manager tasked with responsibility for, and formal assessment of, sourcing the Engine Control module, (ii) review the information provided in each treatment, (iii) answer questions regarding experimental checks, (iv) indicate their recommendation for the response variable, and (v) respond to demographic questions.

**Subjects**

A total of 256 sourcing professionals participated in our SBRP experiment. Sourcing professionals are ideal subjects because of their expertise and familiarity regarding strategic relationships between manufacturers and critical-component suppliers. For our SBRP experiment, 256 subjects completed Treatment 1; 237 completed Treatments 1 and 2; 202

competed Treatments 1, 2, and 3; and 146 completed all three treatments and answered all demographic questions. The 146 subjects who provided demographic data reported an average of 14.7 years of sourcing experience ($\sigma$ = 9.6) and controlled an average of $13.3 million in spend ($\sigma$ = $.16 million). Most (81%) indicated prior experience in a decision to dissolve a strategic supplier relationship, 66% worked for manufacturing firms, and 31% were female.

**Experimental Checks**

Checks for realism, manipulation, confounding effects, and Hawthorne effects were conducted to evaluate the integrity of the experimental design and data (Bachrach and Bendoly 2011; Rungtusanatham et al. 2011). These checks are analogous to assessing construct validity in survey research.

***Realism Check***

A realism check assesses the extent to which the descriptive vignette reflects a realistic situation to which subjects can relate (Louviere et al. 2000). Responding to four questions from Pilling, Crosby, and Jackson (1994) using a 5-point Likert response scale (1 = Strongly Disagree, 5 = Strongly Agree), subjects found the scenarios to be realistic ($\mu$ = 4.06, $\sigma$ = .82), took their roles in the experiment seriously ($\mu$ = 4.51, $\sigma$ = .61), had previously encountered issues underlying this research ($\mu$ = 3.35, $\sigma$ = 1.40), and were highly aware of the issues being investigated in this research ($\mu$ = 3.95, $\sigma$ = 1.03).

***Manipulation Checks for HSP, PSI, USE, and SSA***

Manipulation checks determine whether subjects accurately perceive the cued levels of the experimental factors (Wetzel 1977). The detailed statistical results and their interpretations are documented in Tables A3 and A4 in the Appendix.

For HSP and PSI, one-way ANOVA test results in the shaded cells of Table A3 reveal that subjects assigned to versions in which HSP is cued to be STELLAR report statistically higher average scores for the manipulation questions than those assigned to versions in which HSP is cued to be MARGINALLY-ACCEPTABLE ($p < .001$). Likewise, subjects assigned to versions in which PSI is cued to be HIGH report higher average scores for the manipulation questions than those assigned to versions in which PSI is cued to be LOW ($p < .001$). Subjects, therefore, perceive the levels of these two between-subject factors as cued.

For USE and SSA, TRUE/FALSE questions were asked and assessments were conducted using two separate Fisher's exact tests. When USE is cued to be ABSENT, the shaded cells in Table A4 reveal that 229 of 256 subjects or 89% indicate as TRUE that the relationship between ELECTRONICS Inc. and ZENGINE has been free of critical incidents of a negative nature, but when USE is cued to be PRESENT, this percentage decreases to 39% (54 of 137 subjects). The distribution of TRUE and FALSE responses is statistically different (Fisher's exact test: $p < .001$). Subjects, therefore, perceive the presence (absence) of USE as cued. When SSA is manipulated from ABSENT to PRESENT, the distribution of TRUE and FALSE responses changes from 0% TRUE (0 of 256 subjects) to 93% TRUE (112 of 121 subjects) that GAMMA outperforms ZENGINE, with this distribution being statistically different (Fisher's exact test: $p < .001$). Examining the distribution of TRUE-FALSE responses for SSA = PRESENT only, we also find that more subjects indicate as TRUE that GAMMA outperforms ZENGINE (Fisher's exact test: $p < .001$). Subjects, therefore, perceive the presence (absence) of SSA as cued.

***Checks for Confounding Effects***

Checks for confounding effects determine whether or not subject perceptions about non-manipulated factors levels are affected by experimentally-manipulated factors (Wetzel 1977).

The detailed statistical results in the non-shaded cells of Table A3 reveal that the MARGINALLY-ACCEPTABLE and STELLAR cued-levels of HSP do not produce significantly different (i) subject responses to the PSI manipulation check questions (one-way ANOVA test: $p = .64$, $p = .34$), (ii) subject answers of TRUE or FALSE to the manipulation check question for USE (Fisher's exact test: $p = .49$), or (iii) subject answers of TRUE or FALSE to the manipulation check question for SSA (Fisher's exact test: $p = .24$). Similarly, the LOW and HIGH cued-levels of PSI do not produce significantly different (i) subject responses to the manipulation check questions for HSP (one-way ANOVA test: $p = .14$, $p = .55$, $p = .74$), (ii) subject answers of TRUE or FALSE to the manipulation check question for USE (Fisher's exact test: $p = .65$), or (iii) subject answers of TRUE or FALSE to the manipulation check question for SSA (Fisher's exact test: $p = .73$). These results, therefore, affirm the absence of confounding effects between HSP and the remaining three experimental factors, as well as between PSI and the remaining three experimental factors.

The non-shaded cells in Table A4 show that subject responses of TRUE or FALSE to the manipulation check question for SSA are not distributed differently between the PRESENT and ABSENT cued-levels of USE (Fisher's exact test: $p = .46$). Similarly, subject responses of TRUE or FALSE to the manipulation check question for USE are not distributed differently between the PRESENT and the ABSENT cued-levels for SSA (Fisher's exact test: $p = .05$, not significant with the Bonferroni correction). These results, therefore, affirm an absence of confounding effects between USE and SSA.

***Checks for Hawthorne Effects***

Checks for Hawthorne effects assess whether extraneous factors related to the baseline context affect how subjects perceive and react to the experiment (Adair 1984). Table A5 in the

Appendix documents the statistical results for the two extraneous factors of (i) prior experience that subjects have with situations involving the termination of strategic suppliers and (ii) location of ISM meetings where the experiment was conducted.

For prior subject experience, one-way ANOVA test results are non-significant at $\alpha = .05$ for the HSP and PSI between-subject factors; Fisher's exact test results are non-significant at $\alpha = .05$ for the USE and SSA within-subject factors. One-way ANOVA test results are also non-significant at $\alpha = .05$ with respect to the response variable. Prior subject experience, therefore, does not appear to have Hawthorne effects on the experimental factors or on the response variable. For location where the experiment was conducted, one-way ANOVA test results reveal no differences at $\alpha = .05$ with regards to the mean scores for *Likelihood of Strategic Relationship Dissolution Likelihood* across locations. The location of ISM meetings for our experimentation, therefore, does not appear to have Hawthorne effects on the response variable.

## HYPOTHESIS TESTING

### Model Estimation

Model [1] below specifies the general linear mixed-effects model for the response variable, *Likelihood of Strategic Relationship Dissolution* ($Y_{i,\,jklm}$):

$$
\begin{aligned}
Y_{i,\,jklm} = {} & \text{Intercept} \\
& + \beta_{1_i}(HSP)_j + \beta_{2_i}(PSI)_k && \text{Between-subject factors: } \textit{Historical Supplier Performance} \text{ (HSP) and } \textit{Prior Supplier Involvement in Joint Product Development} \text{ (PSI)} \\
& + \beta_{3_i}(USE)_l + \beta_{4_i}(SSA)_m && \text{Within-subject factors: } \textit{Unintentional but Serious Supplier Error} \text{ (USE) and } \textit{Supplier Substitution Availability} \text{ (SSA)} \\
& + \beta_{5_i}(HSP\times PSI)_{jl} + \beta_{6_i}(HSP\times USE)_{jk} + \beta_{7_i}(HSP\times SSA)_{jm} + \beta_{8_i}(PSI\times USE)_{kl} + \beta_{9_i}(USE\times SSA)_{lm} \\
& + \beta_{10_i}(HSP\times USE\times PSI)_{jkl} + \beta_{11_i}(HSP\times USE\times SSA)_{jlm} \\
& + b_{0_i} + b_{1_{i(jk)}}(USE)_l + b_{2_{i(jk)}}(SSA)_m + e_{i,\,jklm}
\end{aligned}
\qquad [1]
$$

where

| | |
|---|---|
| $Y_{i,\,jklm}$ | Average score of the responses to the three-item measurement scale for *Likelihood of Strategic Relationship Dissolution* by the $i^{th}$ subject for the $j^{th}$ $k^{th}$, $l^{th}$, and $m^{th}$ levels of the HSP, PSI, USE, and SSA factors, respectively |
| $(HSP)_j$ | $j^{th}$ level (MARGINALY-ACCEPTABLE or STELLAR) of the HSP factor |
| $(PSI)_k$ | $k^{th}$ level (LOW or HIGH) of the PSI factor |
| $(USE)_l$ | $l^{th}$ level (ABSENT or PRESENT) of the USE factor |
| $(SSA)_m$ | $m^{th}$ level (ABSENT or PRESENT) of the SSA factor |
| $b_{0_i}$, $b_{1_{i(jk)}}$, $b_{2_{i(jk)}}$ | Random effects |
| $e_{i,\,jklm}$ | Random error |

Note that Model [1] treats the four experimental factors as fixed effects; differences between subject and group means of SG-versions and GS-versions as the random effects, $b_{1_{i(jk)}}$ and $b_{2_{i(jk)}}$, respectively; and the remaining differences across subjects as the random effect, $b_{0_i}$. Model [1] includes two three-way interaction terms (HSP×USE×PSI and HSP×USE×SSA) to allow for post-hoc robustness tests of the invariance of the USE moderating effect as PSI and SSA vary. To comply with the hierarchy principle (Peixoto 1987), Model [1] also includes the four corresponding two-way interaction terms (HSP×PSI, USE×PSI, HSP×SSA, and USE×SSA), in addition to the hypothesized HSP×USE interaction term (i.e., H3). Also, a one-way ANOVA test, prior to estimating Model [1], found no significant effects for years of subject sourcing experience, subject control of spend dollar, employment segment, and gender on the response variable across Treatments 1, 2, and 3 (at $\alpha = .05$). Table A6 in the Appendix summarizes these results. Model [1] is, therefore, estimated without these demographic variables.

**Results**

Table 2 summarizes the estimation results for Model [1], as well as those for a full model that includes four non-hypothesized interaction terms: PSI×SSA, HSP×PSI×SSA, PSI×USE×SSA,

and HSP×PSI×USE×SSA. The estimation results are obtained using the "lme4" (linear mixed-effects models) package in R (Bates and Sarkar 2007).

INSERT Table 2 Here

Because the four non-hypothesized interactions in the full model are not significant, we interpreted the Model [1] estimation results to draw conclusions regarding our hypotheses. For H1, $\beta_1 = 1.42$ ($p < .001$) indicates that when the cued level of HSP is MARGINALLY-ACCEPTABLE, the predicted value of the *Likelihood of Strategic Relationship Dissolution* is 1.42 higher than when the cued level of HSP is STELLAR. A critical-component supplier whose historical performance is marginally-acceptable is, therefore, more likely to be terminated than one whose historical performance is stellar. This result suggests that historical supplier performance and the dissolution likelihood of the strategic relationship is inversely related and supports H1. For H2, $\beta_3 = -.83$ ($p < .001$) indicates that when the cued level of USE is ABSENT, the predicted value of the *Likelihood of Strategic Relationship Dissolution* is .83 lower than when the cued level of USE is PRESENT. A strategic relationship with a critical-component supplier who does not commit an unintentional but serious error is, therefore, less likely to be terminated than a strategic relationship in which a supplier commits such an error. This result suggests that the occurrence of an unintentional but serious error increases the dissolution likelihood of a strategic relationship and supports H2.

Finally, given statistical results for H1, the HSP×USE interaction term ($\beta_6 = .49$, $p < .001$) indicates a positive USE moderation effect. H3b is therefore supported over H3a. The conditional effects plot in Figure 3 corroborates this conclusion. The non-parallel lines suggest an interaction effect, with the slope of the line corresponding to USE = PRESENT being less negative than the slope of the line corresponding to USE = ABSENT. In other words, when

strategic critical-component suppliers with either stellar or marginally-acceptable historical performance commits an unintentional but serious error, their likelihood of being terminated increases. However, the penalty (i.e., increased termination likelihood) for suppliers with stellar historical performance is significantly greater than for suppliers with marginally-acceptable historical performance. We label this result the *positive historical supplier performance penalty effect*. Indeed, as one subject noted, post-experimentation, "Disappointment is a relative term. If performance had been good and suddenly turns poor, the *contrast* is more disappointing than if a poor supplier commits 'another' error."

INSERT Figure 3 Here

**Post-Hoc Analyses: Robustness Check and Alternative Explanations**

Joint product development efforts (Peterson et al. 2005) and lack of attractive alternative suppliers (Ping 1994) are known to strengthen the dependence of a manufacturer on its critical-component supplier and, therefore, should guard against a positive USE moderation effect. As a robustness check, Model [1] estimation results for HSP×PSI×USE and HSP×USE×SSA are examined to determine the extent to which the positive USE moderation effect changes as PSI or SSA varies. Both HSP×PSI×USE ($\beta_{10}$ = .15, $p$ = .22) and HSP×USE×SSA ($\beta_{11}$ = .31, $p$ = .23) are non-significant; these statistical results suggest that the observed positive USE moderation effect is not affected by either prior involvement of the critical-component supplier in joint product development efforts with the manufacturer or the entry into the supply market of a replacement for the incumbent critical-component supplier.

A recency effect (Deese and Kaufman 1957) is present when the most recent event (i.e., unintentional but serious supplier error) is recalled more readily than less recent events (i.e., historical supplier performance) and, hence, more salient in influencing subject responses. To

investigate this possibility, we asked the 17 subjects participating in the SBRP experiment at the Detroit location to reveal how much weight they gave to recent versus historical supplier performance information. Eight placed a greater weight on recent performance, five gave equal weights to historical and recent performance, and four placed a greater weight on historical performance. A one-way ANOVA test detects no differences in the mean scores for *Likelihood of Strategic Relationship Dissolution* across these three groupings for Treatment 1 ($p$ = .65), Treatment 2 ($p$ = .10), or Treatment 3 ($p$ = .31), which suggests an absence of the recency effect. This absence is likely to hold beyond the Detroit location, given the statistical similarity in subject responses between Detroit and the nine original ISM locations. Hence, our hypothesis testing results do not appear to be explained by the recency effect.

Finally, subject risk aversion is unlikely to bias our hypothesis testing results for two reasons. First, the random assignment of subjects to different versions in our SBRP experiment guards against risk-averse subjects being systematically exposed to only certain manipulations while risk-taking subjects being exposed to others. Second, prior research shows that gender, age, and industry sector relate to risk aversion propensity (Weber, Blais, and Betz 2002; Nicholson et al. 2005). Given the non-significant ANOVA results regarding gender, years of sourcing experience (which correlates to subject age), and industry sector, our results, therefore, do not appear to be explained by risk aversion propensity.

## DISCUSSION

### Theoretical Contributions

Figure 4 depicts our two theoretical contributions (using dashed ovals and arrows) to the extant literature (using solid ovals and arrows). The first contribution provides insights into why honest supplier errors that harm both suppliers and buyers should be minimized, regardless of historical

supplier performance levels; the second provides insights into why stellar historical supplier performance, in particular, is a necessary but insufficient condition for relationship continuity.

INSERT Figure 4 Here

***Why Minimize Honest Supplier Mistakes***

Prior research into precipitating events has focused on intentional supplier misbehaviors that benefit the supplier at the expense of the buyer (Abdul-Muhmin 2005; Ganesan et al. 2010; Wang et al. 2010). Our inquiry complements these efforts by focusing on honest supplier mistakes that are not intended to harm the manufacturer or supplier, but whose occurrence results in operational and financial harm to both parties. These mistakes constitute a previously-uninvestigated type of supplier misbehavior that has been neglected in the literature; they differ from supplier opportunism and ethical violations in the intentionality to cause harm. In this regard, our finding that an unintentional but serious supplier error increases the likelihood of strategic relationship dissolution – regardless of historical supplier performance levels – adds a novel insight. A critical-component supplier should be attentive to minimizing honest mistakes that harm itself and its trading partner.

***Why Stellar Historical Supplier Performance Is Necessary but Not Sufficient***

Critical-component suppliers are expected to perform well, with positive assessments of performance deterring dismissal by the manufacturer (e.g., Shin et al. 2000; Abdul-Muhmin 2005). Our inquiry affirms this baseline relationship (i.e., support for H1) and, more importantly, contributes novel and nuanced insights regarding a previously-uninvestigated and robust *positive historical supplier performance penalty effect* (i.e., support for H3b). Suppliers, regardless of historical performance levels, experience a penalty effect from commission of unintentional but serious errors. The penalty effect, however, is harsher for those with a history

of stellar performance than those with a history of marginally-acceptable performance, paralleling the more pronounced negative market reactions to product recalls experienced by automotive firms with good reputations relative to those with marginal reputations (Rhee and Haunschild 2006).

More substantively, this penalty is not tempered by the extent to which an incumbent supplier had been involved in joint product development efforts with the manufacturer or by the unavailability of attractive supplier alternatives in the marketplace. The former increases the level of commitment that the manufacturer has to the critical-component supplier (Petersen et al. 2008); the latter reduces the incentives for the manufacturer to switch (Dwyer et al. 1987; Sriram and Mummalaneni 1990; Abdul-Muhmin 2005). Theoretically, these two factors should buffer an incumbent supplier who errs against relationship dissolution. As such, stellar historical supplier performance, given the robust *positive supplier performance penalty effect*, appears to be a necessary but insufficient condition for sustaining a strategic relationship between a manufacturer and capable, well-performing critical-component supplier.

**Managerial Implications for Buyers and Suppliers**

Our findings provide two pieces of practical advice to safeguard against premature termination of critical-components supplier – one for manufacturers and another for critical-component suppliers. We urge manufacturers to be aware that they may, over time and subconsciously, elevate their expectations of supply performance. This escalation of supply performance expectations increases a tendency to overreact to unintentional but serious supplier errors. The overreaction, in turn, amplifies assessments of the initial negative effect of honest supplier mistakes, overrates alternative supplier options (Ganesh et al. 2000), and eventually biases manufacturers to prematurely terminate critical-component suppliers. This is more concerning

for critical-component suppliers who are, otherwise, capable and have historically performed well. To avoid this particular trap, manufacturers should enact appropriate response plans to cope with historically-capable critical-component suppliers that explicitly reject the option of immediately switching to a new supply source.

For critical-component suppliers, we caution that an impeccable performance record is necessary but not enough to protect against dismissals when unintentional but serious mistakes occur. Because manufacturers expect increasing returns from their critical-component suppliers over time (Autry and Golicic 2010), supplier performance complacency is a real threat to maintaining relationships with manufacturers (Beverland et al. 2004). Critical-component suppliers, in general, should pay attention to escalating manufacturer expectations. Those with a history of stellar performance should be especially vigilant and proactive in managing escalating performance expectations that manufacturers set over time. To this end, they should habitualize regular sit-downs with customers to review contractual performance obligations and surface implicit performance expectations that may expose them to greater chances of committing unintentional but serious errors. Equally important, when unintentional but serious mistakes occur, suppliers, regardless of prior performance, must marshal mitigation resources quickly and visibly to minimize harm and return to normal operating conditions. Suppliers with stellar historical performance should, in parallel, aggressively frame an honest error as a one-time aberration, whose non-repeatability is demonstrated by corrective action plans.

## FUTURE RESEARCH

Our investigation suggests several research opportunities related to dissolution of strategic buyer-supplier relationships. First, we adopt the perspective of the manufacturer as it forms a judgment regarding relationship continuity with a critical-component supplier based on new

information. A more comprehensive appreciation for our research question may consider the perspective of the critical-component supplier. For example, in the event of a serious manufacturer error with devastating performance implications for the supplier, does the likelihood of strategic relationship dissolution mirror the effects detected in this research?

Second, our inquiry assumes that (i) blame for an unintentional but serious error can be clearly attributed to the critical-component supplier, (iii) the buyer is able to verify that the supplier error is unintentional, and (iii) this verification does not vary due to personality attributes like locus of control. If these assumptions do not hold – for example, when the critical-component supplier is only partially responsible or when the buyer is unable to validate absence of malicious intent beyond reasonable doubt – would the *positive supplier performance penalty effect* hold? Does the extent to which the critical-component supplier is at fault – real or perceived – altar the positive moderation effect observed here and, if so, in what manner? Would buyer inability to validate unintentionality and the *positive supplier performance penalty effect* be directly associated?

Third, in designing our SBRP experiment, we fixed the level of bargaining power by setting this to be equal between the manufacturer and critical-component supplier, as well as the supply market size by stating that four suppliers are available. Doing so makes these attributes invariant across all treatments and versions and removes their effects on the response variable. Relaxing these constraints, how would differences in power imbalances or supply market size alter either the direct effect of an unintentional but serious supplier error on relationship dissolution or the indirect effect through trust and commitment? Moreover, would varying these controls temper the positive moderation effect of such a mistake (again, either directly or indirectly through trust and commitment)?

Finally, our post-hoc analysis finds the *positive supplier performance penalty effect* to be robust to prior involvement of the critical-component supplier in joint product development efforts with the manufacturer and to the entry of a capable replacement into the market place. Intuitively, the penalty effect should have been tempered by joint product development efforts and amplified by the availability of attractive substitutes. Why then is the *positive supplier performance penalty effect* robust to these attributes? With respect to the former, is it because joint development efforts do not automatically connote joint responsibility? With respect to the latter, is it because the effect of "fear of the unknown" trumps that of "fear of the known" in dismissing the attractiveness of the potential substitute?

## CONCLUSIONS

The strategic relationship between a manufacturer and its critical-component supplier is ideally intended to last many years. This is particularly true when the supplier has performed well over time. Performing well, however, is a double-edged sword. Stellar supplier performance, on one hand, is necessary to maintain strategic relationships over time. On the other hand, it may also elevate manufacturer expectations about supplier performance such that not meeting them shocks the manufacturer into possibly exiting the strategic relationship.

Theorizing and examining the moderating effect of an unintentional but serious supplier error on the inverse relationship between historical supplier performance and the likelihood of strategic relationship dissolution, our research reveals that a critical-component supplier with stellar historical performance is subject to a *positive supplier performance penalty effect*. This finding reinforces advice for critical-component suppliers to work closely with manufacturers to: (i) set initial supply performance expectations, (ii) evolve supply performance expectations over time, and (iii) proactively manage manufacturer reactions when honest but critical supply

mistakes arise. Manufacturers sourcing from critical-component suppliers that have historically performed well must likewise be aware that they may be conditioning themselves to inadvertently levy harsher penalties following honest supplier mistakes.

In the parlance of scientific progress, the *positive supplier performance penalty effect* qualifies conventional wisdom that positive historical supplier performance is always desirable in terms of strategic buyer-supplier relationship continuity. What makes this finding interesting is that it essentially ". . . denies an old truth . . . [and] . . . constitute[s] an attack on the taken-for-granted world . . . ." (Davis 1971, p. 311). In doing so, it becomes a legitimate, value-added theoretical contribution since it ". . . affects the accepted relationships between variables . . . and alters our understanding [about a phenomenon] by reorganizing our causal maps" (Whetten 1989, pp. 492-493).

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Table 1. Design Matrix [#, &, %]

| | | Treatment 1 | | | | | Treatment 2 | | | | | Treatment 3 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | FACTOR Levels | | | | | FACTOR Levels | | | | | FACTOR Levels | | | | |
| | | HSP | PSI | USE | SSA | Response 1 | HSP | PSI | USE | SSA | Response 2 | HSP | PSI | USE | SSA | Response 3 |
| Version | $I_{GS}$ | + | + | – | – | $Y_{1,++--}$ | + | + | – | + | $Y_{1,+++-+}$ | + | + | + | + | $Y_{1,++++}$ |
| | $I_{SG}$ | + | + | – | – | $Y_{2,++--}$ | + | + | + | – | $Y_{2,+++-}$ | + | + | + | + | $Y_{2,++++}$ |
| | $J_{GS}$ | + | – | – | – | $Y_{3,+---}$ | + | – | – | + | $Y_{3,+--+}$ | + | – | + | + | $Y_{3,+-++}$ |
| | $J_{SG}$ | + | – | – | – | $Y_{4,+---}$ | + | – | + | – | $Y_{4,+-+-}$ | + | – | + | + | $Y_{4,+-++}$ |
| | $K_{GS}$ | – | + | – | – | $Y_{5,-+--}$ | – | + | – | + | $Y_{5,-+-+}$ | – | + | + | + | $Y_{5,-+++}$ |
| | $K_{SG}$ | – | + | – | – | $Y_{6,-+--}$ | – | + | + | – | $Y_{6,-++-}$ | – | + | + | + | $Y_{6,-+++}$ |
| | $L_{GS}$ | – | – | – | – | $Y_{7,----}$ | – | – | – | + | $Y_{7,---+}$ | – | – | + | + | $Y_{7,--++}$ |
| | $L_{SG}$ | – | – | – | – | $Y_{8,----}$ | – | – | + | – | $Y_{8,--+-}$ | – | – | + | + | $Y_{8,--++}$ |
| Version | $I_{GS}$ | + | + | – | – | $Y_{9,++--}$ | + | + | – | + | $Y_{9,+++-+}$ | + | + | + | + | $Y_{9,++++}$ |
| | $I_{SG}$ | + | + | – | – | $Y_{10,++--}$ | + | + | + | – | $Y_{10,+++-}$ | + | + | + | + | $Y_{10,++++}$ |
| | $J_{GS}$ | + | – | – | – | $Y_{11,+---}$ | + | – | – | + | $Y_{11,+--+}$ | + | – | + | + | $Y_{11,+-++}$ |
| | $J_{SG}$ | + | – | – | – | $Y_{12,+---}$ | + | – | + | – | $Y_{12,+-+-}$ | + | – | + | + | $Y_{12,+-++}$ |
| | $K_{GS}$ | – | + | – | – | $Y_{13,-+--}$ | – | + | – | + | $Y_{13,-+-+}$ | – | + | + | + | $Y_{13,-+++}$ |
| | $K_{SG}$ | – | + | – | – | $Y_{14,-+--}$ | – | + | + | – | $Y_{15,-++-}$ | – | + | + | + | $Y_{14,-+++}$ |
| | $L_{GS}$ | – | – | – | – | $Y_{15,----}$ | – | – | – | + | $Y_{16,---+}$ | – | – | + | + | $Y_{15,--++}$ |
| | $L_{SG}$ | – | – | – | – | $Y_{16,----}$ | – | – | + | – | $Y_{16,--+-}$ | – | – | + | + | $Y_{16,--++}$ |
| Version | $I_{GS}$ | + | + | – | – | $Y_{17,++--}$ | + | + | – | + | $Y_{17,+++-+}$ | + | + | + | + | $Y_{17,++++}$ |
| | $I_{SG}$ | + | + | – | – | $Y_{18,++--}$ | + | + | + | – | $Y_{18,+++-}$ | + | + | + | + | $Y_{18,++++}$ |
| | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . |
| | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . | . . . |

[#] HSP (*Historical Supplier Performance*) and PSI (*Prior Supplier Involvement in Joint Product Development*) are between-subject factors; USE (*Unintentional but Serious Supplier Error*) and SSA (*Supplier Substitution Availability*) are within-subject factors. "+" denotes STELLAR (for HSP), HIGH (for PSI) or PRESENT (for USE and SSA); "–" denotes MARGINALLY-ACCEPTABLE (for HSP), LOW (for PSI), or ABSENT (for USE and SSA).

[&] $Y_{i,\,jklm}$ is the response for the *Likelihood of Strategic Relationship Dissolution* from the $i^{th}$ subject at the $j^{th}$, $k^{th}$, $l^{th}$, and $m^{th}$ levels (+ or –) of the HSP, PSI, USE, and SSA factors.

[%] We compared the means of the response variable from the SG-subscripted versions to those from the GS-subscripted versions for Treatments 1, 2, and 3 and find no significant differences ($p = .62$, $p = .16$, $p = .69$, respectively) to indicate a sequencing effect with respect to USE and SSA.

Table 2. Model Estimation Results [#, &]

| Source | | | Model [1] Estimate | Model [1] S.E. | Model [1] *p* | Full Model Estimate | Full Model S.E. | Full Model *p* |
|---|---|---|---|---|---|---|---|---|
| Fixed Effects | | Intercept | 2.00 | .10 | < .001 | 2.01 | .10 | < .001 |
| | H1 | HSP | 1.42 | .15 | < .001 | 1.42 | .15 | < .001 |
| | | PSI | –.11 | .14 | .42 | –.12 | .15 | .38 |
| | H2 | USE | –.83 | .14 | < .001 | –.81 | .14 | < .001 |
| | | SSA | –.78 | .12 | < .001 | –.76 | .16 | < .001 |
| | | HSP × PSI | –.08 | .20 | .71 | –.07 | .22 | .76. |
| | H3 | HSP × USE | .49 | .11 | < .001 | .61 | .20 | < .01 |
| | | HSP × SSA | .26 | .17 | .12 | .25 | .23 | .28 |
| | | PSI × USE | .13 | .16 | .42 | .08 | .19 | .67 |
| | | PSI × SSA | | | | –.06 | .23 | .79 |
| | | USE × SSA | –.38 | .16 | .08 | –.33 | .19 | .08 |
| | | HSP × PSI × USE | .15 | .22 | .49 | .04 | .28 | .89 |
| | | HSP × PSI × SSA | | | | .06 | .33 | .86 |
| | | HSP × USE × SSA | .31 | .23 | .17 | .17 | .28 | .41 |
| | | PSI × USE × SSA | | | | –.10 | .27 | .71 |
| | | HSP × PSI × USE × SSA | | | | .34 | .39 | .38 |
| Random Effects | | Variance: Subjects | .49 | | | .49 | | |
| | | Variance: GS version | .23 | | | .23 | | |
| | | Variance: SG version | .49 | | | .51 | | |
| | | Residuals (Error) | .26 | | | .25 | | |
| Model Fit | | AIC | 1757 | | | 1768 | | |
| | | BIC | 1843 | | | 1872 | | |
| | | *–2 Log Likelihood* | 1719 | | | 1722 | | |
| | | $\chi^2$ difference test | $\chi^2$ (4) = 3, *p* = .558 | | | | | |

[#] The full model adds four additional, non-hypothesized interaction terms to those already in Model [1]: PSI×SSA, HSP×PSI×SSA, PSI×USE×SSA, and HSP×PSI×USE×SSA. Because the four non-hypothesized interactions in the full model are not significant, conclusions about hypotheses can be drawn from interpreting the Model [1] estimation results.

[&] HSP is coded as either MARGINALLY-ACCEPTABLE (base group) or STELLAR; PSI is coded as LOW (base group) or HIGH; USE is coded as ABSENT (base group) or PRESENT; and SSA is coded as ABSENT (base group) or PRESENT. To interpret the results for H1 and H2, the signs of the corresponding regression coefficients indicate the level of the predicted value of the *Likelihood of Strategic Relationship Dissolution* for the base group. For example, with HSP, a $\beta_1 = 1.42$ reveals that when the level of HSP is cued to be MARGINALLY-ACCEPTABLE, the predicted value for the *Likelihood of Strategic Relationship Dissolution* is 1.42 higher than when the level of HSP is cued to be STELLAR. Conversely, for USE, a $\beta_3 = –.83$ reveals that when the level of USE is cued to be ABSENT, the predicted value for the *Likelihood of Strategic Relationship Dissolution* is .83 lower than when the level of USE is cued to be PRESENT. For H3a versus H3b, $\beta_6 = .49$ indicates the moderation effect of USE to be positive in support of H3b.

[%] Estimation results are based on average scores for the three-question measurement scale operationalizing *Likelihood of Strategic Relationship Dissolution*. Using factor scores in lieu of average score produced similar and consistent results; these estimation results are available upon request.

Figure 1. Theoretical Model

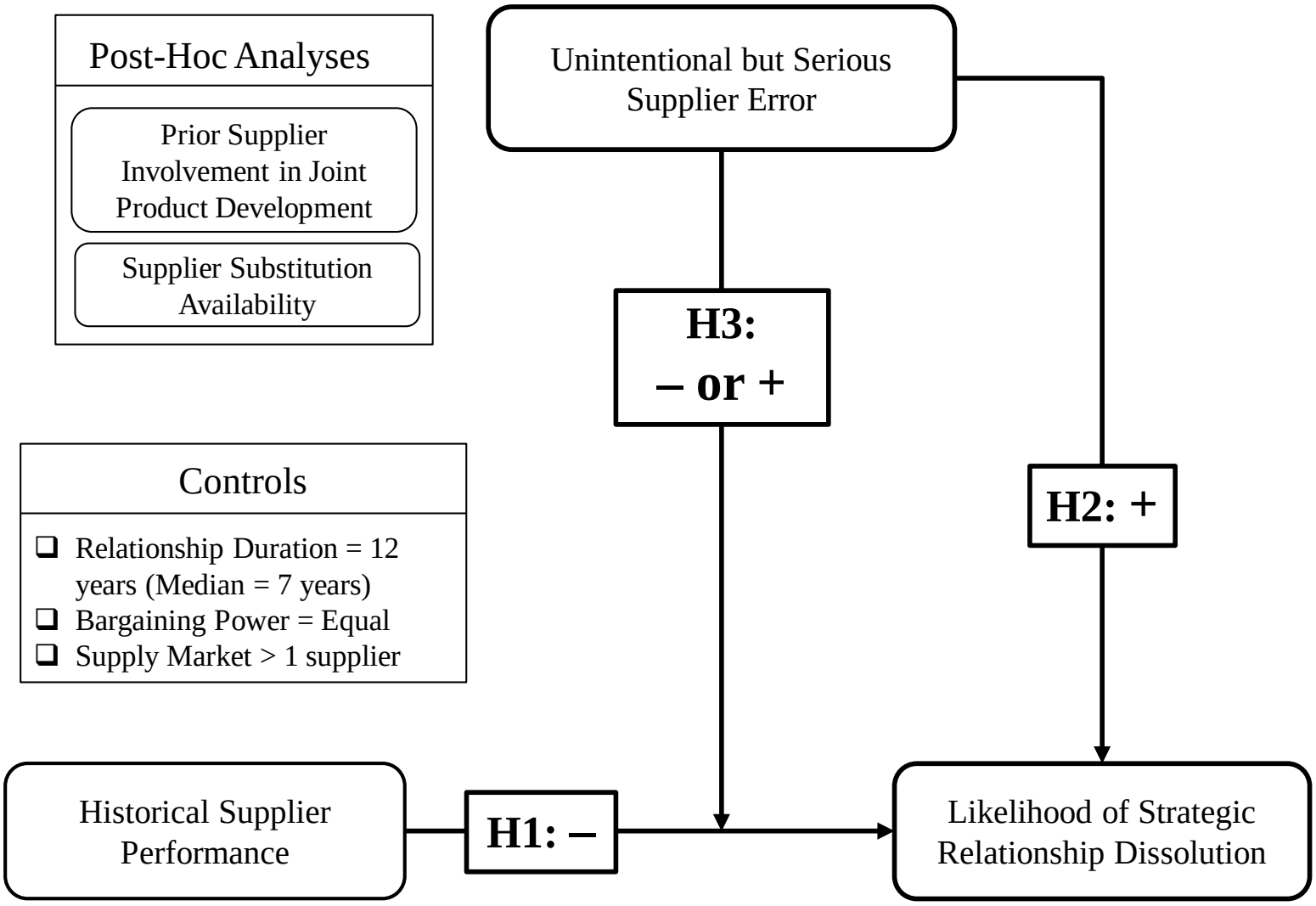


Figure 2. Moderation Effects of an Unintentional but Serious Supplier Error: Assimilation versus Contrast Effects

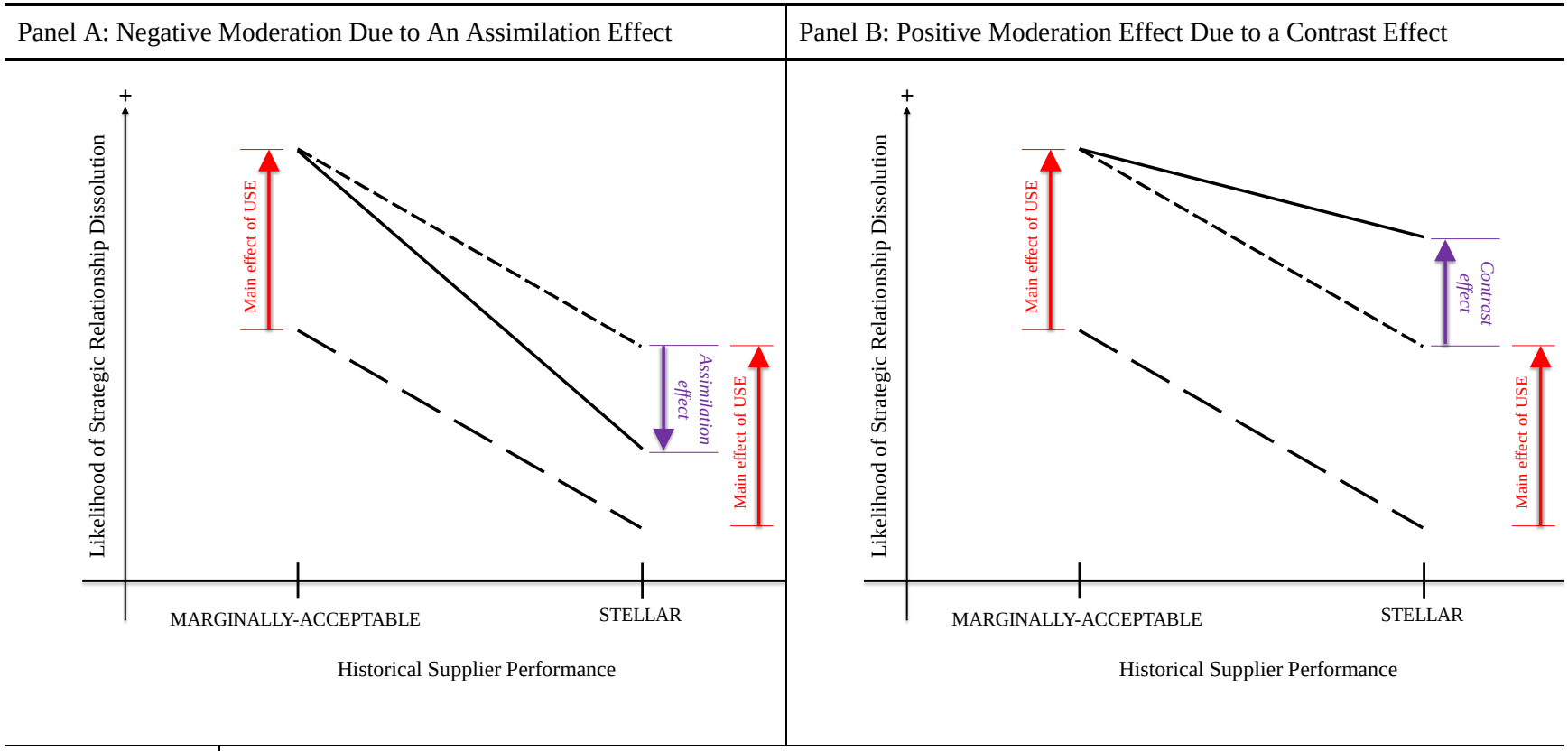
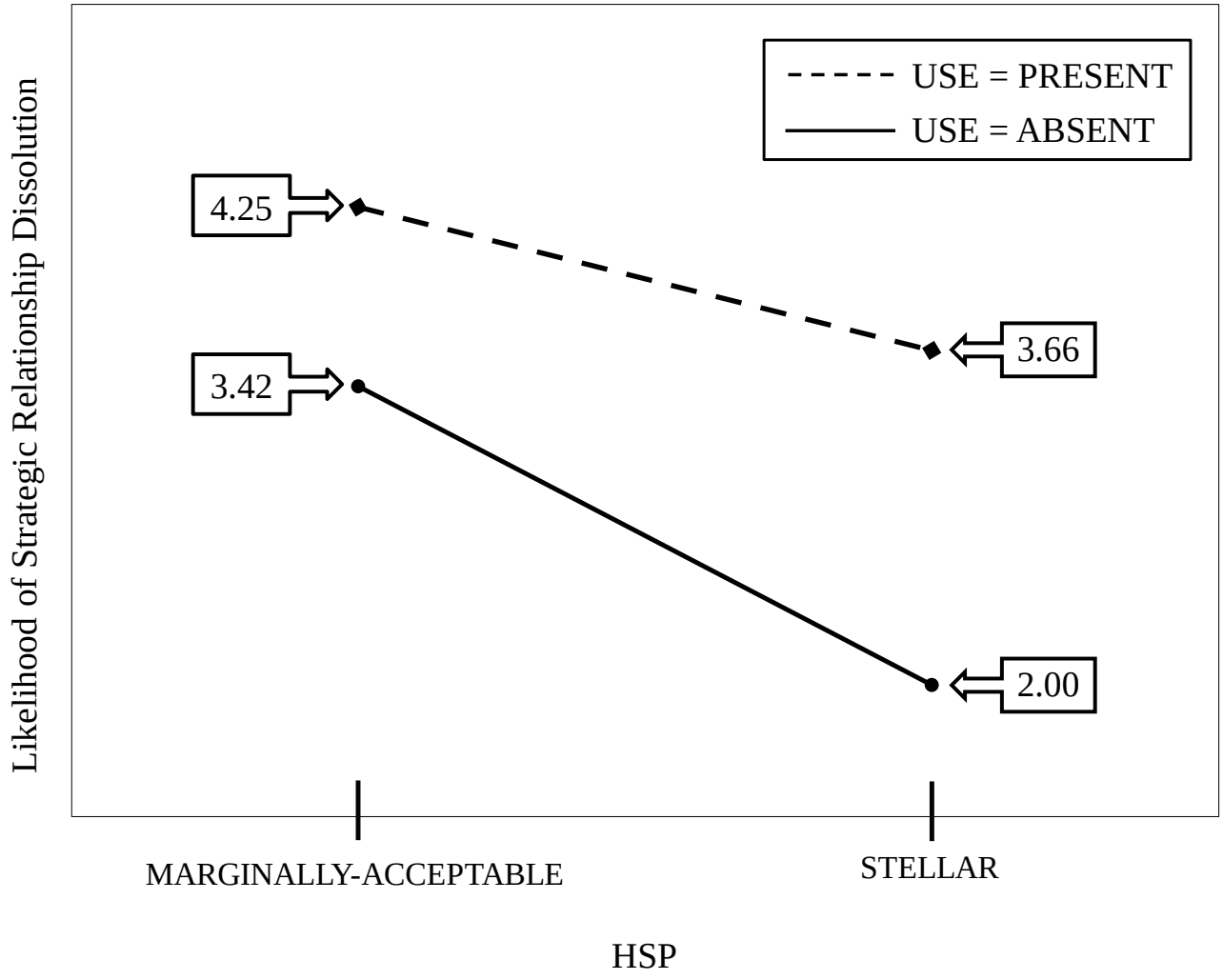
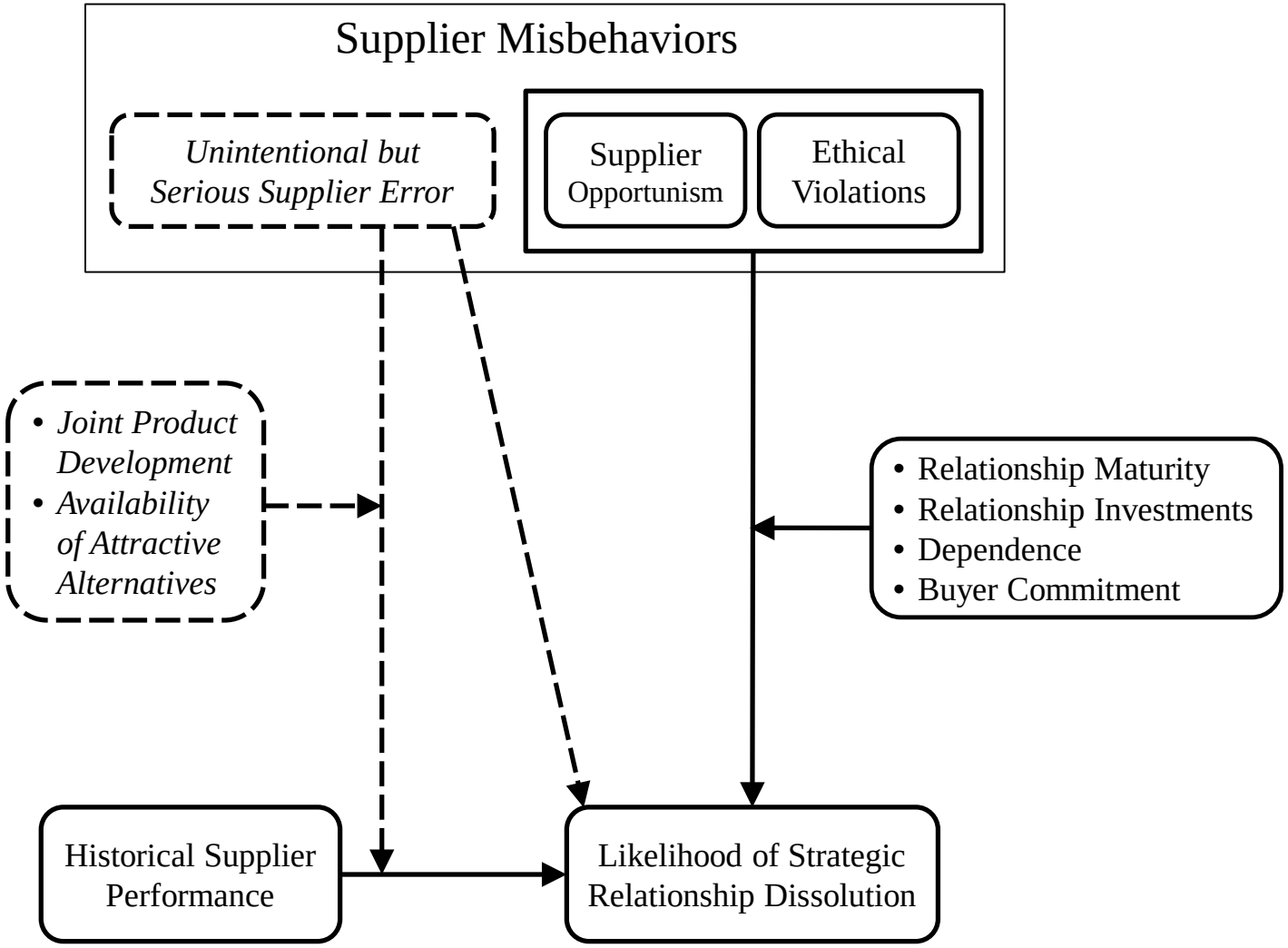


Figure 3. HSP×US Conditional Effects (Interactions) Plot [#]




[#] The non-parallel lines suggest the existence of an HSP×USE interaction effect. Since the HSP×USE interaction term is positive and significant ($\beta_6$ = .49, $p$ < .001) per Model [1] results in Table 2, we conclude in favor of a positive USE moderation effect.

Figure 4. Theoretical Contributions [#]




[#] Theoretical contributions are shown with dashed ovals and arrows; findings reported in the extant literature are shown with solid ovals and arrows.

# APPENDIX

Table A1. Factors Associated with Buyer-Supplier Relationship Dissolution and Exemplary Literature

| | |
|---|---|
| **Entity-Centric Factors**<br>*Factors describing, or specific to, one entity (i.e., buyer or supplier) in a buyer-supplier dyad* | |
| Firmographics (e.g., size, reputation, centrality of position in the market) | Anderson and Weitz (1989); Fichman and Levinthal (1991); Baker, Faulkner, and Fisher (1998) |
| Policy (e.g., pricing, supply base, service policy, etc.) and/or personnel changes | Doyle et al. (1980); Broschak (2004) |
| Satisfaction with performance of the other entity in the buyer-supplier dyad and/or with being in a relationship with the other entity | Ping and Dwyer (1992); Beverland et al. (2004); Abdul-Muhmin (2005); Payan et al. (2010) |
| Dissatisfaction with performance of the other entity in the buyer-supplier dyad and/or with being in a relationship with the other entity | Doyle et al. (1980); Ping and Dwyer (1992); Fichman and Levinthal (1991); Beverland et al. (2004); Helm et al. (2006) |
| Trust toward, commitment toward, conflict with the other entity in the buyer-supplier dyad | Anderson and Weitz (1989); Baker, Faulkner, and Fisher (1998); Fichman and Levinthal (1991); Abdul-Muhmin (2005), Ganesan et al.(2010); Payan et al. (2010) |
| Complexity of supplied product or supply task | Gadde and Mattsson (1987) |
| Intentional misbehaviors of one entity in the buyer-supplier dyad (e.g., unethical behavior and flagrant opportunism) | Ganesan et al.(2010); Wang et al. (2010) |
| **Relationship-Centric Factors**<br>*Factors describing attributes of a buyer-supplier relationship* | |
| Relationship duration | Anderson and Weitz (1989); Sriram and Mummalaneni (1990); Fichman and Levinthal (1991) |
| Relationship-specific investments | Sriram and Mummalaneni (1990); Wang et al. (2010) |
| Nature of communication | Anderson and Weitz (1989); Freeman and Browne (2004) |
| Similarity or dissimilarity between entities in a buyer-supplier relationship (e.g., cultural differences, goal incongruence, beliefs, etc.) | Anderson and Weitz (1989); Freeman and Browne (2004); Ellis (2006); Payan et al. (2010); Wang et al. (2010) |
| Power imbalance or relative dependency | Anderson and Weitz (1989); Wang et al. (2010) |
| **Environment-Centric Factors**<br>*Factors existing outside of a buyer-supplier relationship and not subject to entity control* | |
| Nature of supply or demand market | Gadde and Mattsson (1987); Ellis (2006) |
| Supply alternatives | Sriram and Mummalaneni (1990), Ping (1994) |
| Media and government interventions | Chen et al. (2016) |

Table A2. Factors and Experimental Cues

BETWEEN-SUBJECT Factor: *Historical Supplier Performance* (HSP)

| STELLAR | MARGINALLY-ACCEPTABLE |
|---|---|
| • ZENGINE has a historical defect rate of 1% on average; the median defect rate in the industry for similar products is 3%.<br>• ZENGINE has an on-time delivery performance of 97%; the industry median for on-time delivery performance is 95%.<br>• ZENGINE sells the Engine Control module to your company at a unit price that is, on average, 1% lower; in this industry, a 1% pricing difference is considered to be substantial.<br>• ZENGINE has historically been very flexible and very responsive to unexpected, last-minute changes in order quantities and order delivery schedules. | • ZENGINE has a historical defect rate of 15% on average; the median defect rate in the industry for similar products is 3%.<br>• ZENGINE has an on-time delivery performance of 93%; the industry median for on-time delivery performance is 95%.<br>• ZENGINE sells the Engine Control module to your company at unit price that is, on average, 1% higher, in this industry, and a 1 % pricing difference is considered to be substantial.<br>• ZENGINE has not been very flexible nor very responsive to unexpected, last-minute changes in order quantities and order delivery schedules. |

WITHIN-SUBJECT Factor: *Unintentional but Serious Supplier Error* (USE)

| PRESENT | ABSENT |
|---|---|
| • Statements as those for ABSENT plus<br>o ELECTRONICS Inc. has received a large number of complaints over the weekend about malfunctions with the LP5 laser printers.<br>o Preliminary reports strongly suggest that the malfunctions are caused by overheating in the Engine Control Module; the excessive heat melts other parts of the laser printer and renders the printer no longer fixable.<br>o ZENGINE . . . Is launching a full investigation but believes the root cause may be due to unintentional shipments of defective batches of the Engine Control Module to ELECTRONICS Inc. | • During the last 12 years, while there have been a few minor supply problems, these have generally not been out of the ordinary.<br>o The strategic relationship between ZENGINE and ELECTRONICS Inc., therefore, has been relatively uneventful and incident-free.<br>• During the last 6 months, there were no critical and significantly-negative incidents involving ZENGINE. |

Table A2. Factors and Experimental Cues (Continued)

BETWEEN-SUBJECT Factor: *Prior Supplier Involvement in Joint Product Development* (PSI)

| HIGH | LOW |
|---|---|
| • ZENGINE had previously worked with ELECTRONICS Inc. to jointly design the five laser printer product lines.<br>• In these joint design efforts, both firms had invested substantial and equitable resources, financial and engineering, to support one another.<br>• The LP5 was recently introduced 3 months ago. | • As a strategic supplier, ZENGINE had never worked with ELECTRONICS Inc. to jointly design any of the laser printer product lines. |

WITHIN-SUBJECT Factor: *Supplier Substitution Availability* (SSA)

| PRESENT | ABSENT |
|---|---|
| GAMMA, a relatively new entrant into the supply market for the Engine Control Module, has recently approached ELECTRONICS Inc. and expressed an interest in building a long-term relationship with ELECTRONICS Inc., if given the opportunity.<br>• GAMMA has a defect rate less than 0.05%, relative to X % for ZENGINE and the industry median of 3%.<br>• GAMMA has an on-time delivery performance of 99%, compared to the X% for ZENGINE and the industry median of 95%<br>• Gamma prices its Engine Control module at a unit price that is 2% lower than the industry median; ZENGINE, in comparison, sells its Engine Control module at a unit price that is, on average, 1 % higher (or lower) than the industry median.<br>In supplying these other products, GAMMA has achieved a strong reputation for being very flexible, very responsive, and very fair to its customers. | No information given about GAMMA. |

Table A3. Manipulation Checks and Checks for Confound Effects for BETWEEN-SUBJECT Factors [#]

| | | | | Cued-Level of HSP | | | Cued-Level of PSI | | |
|---|---|---|---|---|---|---|---|---|---|
| | | | | MARGINALLY-ACCEPTABLE | STELLAR | *p* | LOW | HIGH | *p* |
| Manipulation Check Questions for | HSP | 1 | Mean | 2.62 | 4.80 | < .001 | 3.65 | 3.91 | .14 |
| | | | s.d. | 1.21 | .51 | | 1.46 | 1.35 | |
| | | 2 | Mean | 1.42 | 4.79 | < .001 | 3.10 | 3.23 | .55 |
| | | | s.d. | .82 | .62 | | 1.82 | 1.85 | |
| | | 3 | Mean | 1.67 | 4.82 | < .001 | 3.25 | 3.32 | .74 |
| | | | s.d. | 1.03 | .52 | | 1.75 | 1.79 | |
| | PSI | 1 | Mean | 2.75 | 2.85 | .64 | 1.41 | 4.15 | < .001 |
| | | | s.d. | 1.80 | 1.87 | | .99 | 1.40 | |
| | | 2 | Mean | 2.63 | 2.83 | .34 | 1.50 | 3.92 | < .001 |
| | | | s.d. | 1.68 | 1.77 | | 1.03 | 1.39 | |
| | USE | | # T | 28 | 26 | .49 | 29 | 25 | .65 |
| | | | # F | 34 | 43 | | 37 | 40 | |
| | SSA | | # T | 56 | 49 | .24 | 52 | 53 | .73 |
| | | | # F | 2 | 6 | | 4 | 4 | |

[#] The manipulation check questions for HSP asked*:* (1) How well does ZENGINE perform as a supplier of the Engine Control module? (1 = Extremely Poor, 5 = Extremely Well), (2) How flexible has ZENGINE been to last-minute order changes initiated by ELECTRONICS Inc.? (1 = Very Inflexible, 5 = Very Flexible), and (3) How responsive has ZENGINE been to last-minute order changes initiated by ELECTRONICS Inc.? (1 = Very Unresponsive, 5 = Very Responsive). The manipulation check questions for PSI asked: (1) To what extent has ELECTRONICS Inc. involved ZENGINE in the design of the laser printer product lines? (1 = Highly Uninvolved, 5 = Highly Involved) and (2) Would you agree that ELECTRONICS Inc. has recently involved ZENGINE in efforts to design the laser printer? (1 = Strongly Disagree, 5 = Strong Agree).

The one-way ANOVA test results in the shaded cells reveal statistical differences in the average scores for the manipulation check questions between the two levels of HSP and between the two levels of PSI. Subjects assigned to versions in which HSP is cued to be STELLAR report higher average scores for the manipulation questions than those assigned to versions in which HSP is cued to be MARGINALLY-ACCEPTABLE ($p < .001$). Likewise, subjects assigned to versions in which PSI is cued to be HIGH report higher average scores for the manipulation questions than those assigned to versions in which PSI is cued to be LOW ($p < .001$). Subjects, therefore, perceive the levels of these two between-subject factors as cued.

The non-shaded cells reveal that the MARGINALLY-ACCEPTABLE and STELLAR cued-levels of HSP do not produce significantly different (i) subject responses to the PSI manipulation check questions (one-way ANOVA test: $p = .64$, $p = .34$), (ii) subject answers of TRUE or FALSE to the manipulation check question for USE (Fisher's exact test: $p = .49$), or (iii) subject answers of TRUE or FALSE to the manipulation check question for SSA (Fisher's exact test: $p = .24$). Similarly, the LOW and HIGH cued-levels of PSI do not produce significantly different (i) subject responses to the manipulation check questions for HSP (one-way ANOVA test: $p = .14$, $p = .55$, $p = .74$), (ii) subject answers of TRUE or FALSE to the manipulation check question for USE (Fisher's exact test: $p = .65$), or (iii) subject answers of TRUE or FALSE to the manipulation check question for SSA (Fisher's exact test: $p = .73$). These results, therefore, affirm an absence of confounding effects between HSP and the remaining three experimental factors or between PSI and the remaining three experimental factors.

Table A4. Manipulation Checks and Checks for Confound Effects for WITHIN-SUBJECT Factors [#]

| | | | Cued-Level of USE | | | Cued-Level of SSA | | |
|---|---|---|---|---|---|---|---|---|
| | | | ABSENT | PRESENT | *p* | ABSENT | PRESENT | *p* |
| Manipulation Check Questions for | HSP | 1 | Tests as to whether or not HSP varies with levels of USE or SSA are not necessary since the level of the HSP factor given in Treatment 1 is constant across Treatment 2 and Treatment 3 | | | | | |
| | | 2 | | | | | | |
| | | 3 | | | | | | |
| | PSI | 1 | Tests as to whether or not PSI varies with levels of USE or SSA are not necessary since the level of the HSP factor given in Treatment 1 is constant across Treatment 2 and Treatment 3 | | | | | |
| | | 2 | | | | | | |
| | USE | # T | 229 | 54 | < .001 | 212 | 98 | .05 |
| | | # F | 27 | 83 | | 54 | 41 | |
| | SSA | # T | 105 | 100 | .46 | 0 | 112 | < .001 |
| | | # F | 8 | 4 | | 0 | 9 | |

[#] The manipulation check question for USE asked whether or not the relationship between ELECTRONICS Inc. and ZENGINE has been free of critical incidents of a negative nature (T = TRUE, F = FALSE). The manipulation check question for SSA asked whether or not GAMMA outperforms ZENGINE in terms of quality, on-time delivery, and unit pricing (T = TRUE, F = FALSE).

For SG-subscripted versions, when USE is cued to be ABSENT in Treatment 1, the shaded cells reveal that 229 of 256 subjects or 89% indicate as TRUE that the relationship between ELECTRONICS Inc. and ZENGINE has been free of critical incidents of a negative nature, but when USE is cued to be PRESENT in Treatment 2, this percentage decreases to 39% (54 of 137 subjects). The distribution of TRUE and FALSE responses is statistically different (Fisher's exact test: $p < .001$). Subjects, therefore, perceive the presence (absence) of USE as cued.

For GS-subscripted versions, when SSA is experimentally set to ABSENT, it does not make sense to ask subjects to respond to the SSA manipulation check question. The distribution of TRUE-FALSE responses when SSA = ABSENT is therefore unknown but was specified to be 0 responses of TRUE and 0 responses of FALSE to allow conducting a Fisher's exact test. With this caveat, as SSA is manipulated from ABSENT in Treatment 1 to PRESENT in Treatment 2, the distribution of TRUE and FALSE responses changes from 0% TRUE (0 of 256 subjects) to 93% TRUE (112 of 121 subjects) that GAMMA outperforms ZENGINE and is statistically different (Fisher's exact test: $p < .001$). Examining the distribution of TRUE-FALSE responses for SSA = PRESENT only, we also find that more subjects indicate as TRUE that GAMMA outperforms ZENGINE (Fisher's exact test: $p < .001$). Subjects, therefore, generally perceive the presence (absence) of SSA as cued.

The non-shaded cells show that subject responses of TRUE or FALSE to the manipulation check question for SSA are not distributed differently between the PRESENT and the ABSENT cued-levels of USE (Fisher's exact test: $p = .46$). Similarly, subject responses of TRUE or FALSE to the manipulation check question for USE are not distributed differently between the PRESENT and the ABSENT experimentally-cued levels for SSA (Fisher's exact test: $p = .05$, not significant with the Bonferroni correction). These results, therefore, affirm an absence of confounding effects between USE and SSA.

Table A5. Checks for Hawthorne Effects [#]

1. Subject Prior Experience and Perceptions Regarding Experimental Factors

| | | | | Subject Prior Experience with Terminating a Strategic Supplier Relationship? | | | | |
|---|---|---|---|---|---|---|---|---|
| | | | | NO | | YES | | *p* |
| Manipulation Check Questions for | HSP | 1 | Mean | 3.84 | | 3.70 | | .55 |
| | | | s.d. | 1.53 | | 1.46 | | |
| | | 2 | Mean | 3.13 | | 3.14 | | .94 |
| | | | s.d. | 1.93 | | 1.84 | | |
| | | 3 | Mean | 3.13 | | 3.32 | | .86 |
| | | | s.d. | 1.93 | | 1.76 | | |
| | PSI | 1 | Mean | 2.61 | | 2.73 | | .40 |
| | | | s.d. | 1.92 | | 1.88 | | |
| | | 2 | Mean | 2.42 | | 2.75 | | .51 |
| | | | s.d. | 1.73 | | 1.78 | | |
| | | | | Cued-Level of USE | | Cued-Level of USE | | |
| | | | | ABSENT | PRESENT | ABSENT [&] | PRESENT | |
| | USE | | # T | 32 | 7 | 118 | 27 | .89 |
| | | | # F | 1 | 25 | 28 | 120 | .11 |
| | | | | Cued-Level of SSA | | Cued-Level of SSA | | |
| | | | | ABSENT | PRESENT | ABSENT | PRESENT | |
| | SSA | | # T | NA | 31 | NA | 139 | .89 |
| | | | # F | NA | 2 | NA | 7 | |

[#] For HSP and PSI, statistical significance is based on one-way ANOVA tests with results being non-significant at the $\alpha = .05$ level of significance. For USE and SSA, statistical significance is based on Fisher's exact tests with results being non-significant at the $\alpha = .05$ level of significance. Prior subject experience, therefore, does not appear to have Hawthorne effects on the four experimental factors.

[&] 144 subjects completed all three treatments and answered all questions but 148 subjects completed at least two treatments and provided an answer to this demographic question. Hence the column total > 144.

Checks for Hawthorne Effects (Continued)

2. Subject Prior Experience and Response Variable [#]

| | Subject Prior Experience with Terminating a Strategic Supplier Relationship? | | | | |
|---|---|---|---|---|---|
| | NO | | YES | | |
| | Mean | s.d. | Mean | s.d. | *P* |
| Treatment 1 | 2.38 | 1.19 | 2.64 | 1.17 | .42 |
| Treatment 2 | 3.35 | 1.18 | 3.15 | 1.00 | .40 |
| Treatment 3 | 3.53 | 1.19 | 3.54 | .98 | .39 |

[#] One-way ANOVA test results are non-significant at the $\alpha = .05$ level of significance for all three treatments. Prior subject experience, therefore, does not appear to have Hawthorne effects on subject responses to the *Likelihood of Strategic Relationship Dissolution*.

3. Location Where Experiment Was Conducted and Response Variable [#]

| | Treatment 1 | | Treatment 2 | | Treatment 3 | |
|---|---|---|---|---|---|---|
| | Mean | s.d. | Mean | s.d. | Mean | s.d. |
| Boston, MA | 2.76 | 1.08 | 3.25 | 1.06 | 3.66 | .94 |
| Cleveland, OH | 2.97 | 1.08 | 3.06 | .92 | 3.43 | 1.14 |
| Cincinnati, OH | 2.37 | 1.15 | 3.05 | 1.02 | 3.65 | .94 |
| Des Moines, IA | 2.33 | 1.12 | 2.84 | 0.93 | 3.43 | .87 |
| Indianapolis, IN | 2.46 | 1.10 | 3.37 | 1.03 | 3.48 | 1.10 |
| Louisville, KY | 2.76 | .96 | 3.48 | .96 | 3.63 | 1.31 |
| Milwaukee, WI | 1.97 | 1.04 | 3.15 | 1.04 | 3.73 | 1.04 |
| San Diego, CA | 2.83 | 1.24 | 3.35 | 1.05 | 3.56 | 1.06 |
| Twin Cities, MN | 2.47 | 1.06 | 2.94 | .85 | 3.03 | .98 |
| *p* | .15 | | .47 | | .32 | |

[#] One-way ANOVA test results are non-significant at the $\alpha = .05$ level of significance for all three treatments. The location of ISM meetings where our SBRP experiment was conducted, therefore, does not appear to have Hawthorne effects on subject responses to the *Likelihood of Strategic Relationship Dissolution*.

Table A6. Subject Demographics and *Likelihood of Strategic Relationship Dissolution* [#]

| | Treatment 1 | | Treatment 2 | | Treatment 3 | |
|---|---|---|---|---|---|---|
| | Mean | s.d. | Mean | s.d. | Mean | s.d. |
| Years of Sourcing Experience | | | | | | |
| ~ 7 | 2.45 | 1.12 | 3.04 | 1.05 | 3.30 | 1.05 |
| 8 ~14 | 2.51 | 1.08 | 3.20 | 1.05 | 3.59 | 1.11 |
| 15 ~ 21 | 2.62 | 1.09 | 3.21 | .94 | 3.30 | .94 |
| 22 ~ 28 | 2.76 | 1.30 | 3.31 | 1.16 | 3.94 | .90 |
| ≥ 29 | 1.93 | 1.74 | 3.07 | 1.32 | 3.13 | 1.07 |
| *p* | .64 | | .88 | | .15 | |
| Subject Control of Spend Dollars (in millions) | | | | | | |
| ~2.5 | 2.72 | .94 | 3.10 | .86 | 3.17 | .92 |
| <10 | 2.20 | 1.23 | 2.92 | 1.19 | 3.42 | 1.02 |
| 10.01~24 | 2.65 | 1.18 | 3.07 | 1.14 | 3.45 | 1.12 |
| ≥ 24.01 | 2.41 | 1.05 | 3.21 | .92 | 3.63 | 1.05 |
| *p* | .30 | | .80 | | .45 | |
| Employment Segment | | | | | | |
| Manufacturing | 2.67 | 1.09 | 3.24 | 1.04 | 3.53 | 1.01 |
| Service | 2.77 | 1.22 | 3.30 | 1.03 | 3.56 | .92 |
| *p* | .65 | | .78 | | .88 | |
| Gender | | | | | | |
| Male | 2.72 | 1.17 | 3.24 | 1.09 | 3.59 | 1.01 |
| Female | 2.34 | .96 | 3.05 | .90 | 3.37 | 1.02 |
| *p* | .10 | | .45 | | .28 | |

[#] One-way ANOVA test results are non-significant at the $\alpha = .05$ level of significance for all three treatments. Subject responses to the *Likelihood of Strategic Relationship Dissolution*, therefore, do not differ across years of sourcing experience, subject control of spend dollars, employment segment, or gender.