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Alliance or Acquisition? A Mechanisms-Based, Policy-Capturing Analysis

Thomas Mellewigt,[1]* Adeline Thomas,[1] Ingo Weller,[2] and Edward J. Zajac[3]
[1] Department of Management, Freie Universität Berlin, Berlin, Germany
[2] Munich School of Management and Organizations Research Group, LMU Munich, Munich, Germany
[3] Kellogg School of Management, Northwestern University, Evanston, Illinois

Research summary: *While alliance researchers view prior partner-specific alliance experience as influencing firms' subsequent alliance or acquisition decisions, empirical evidence on the alliance versus acquisition decision is surprisingly mixed. We offer a reconciliation by proposing and testing an analytical framework that recognizes prior partner-specific experiences as heterogeneous along three fundamental dimensions: partner-specific trust, routines, and value certainty. This allows us to use a policy-capturing methodology to rigorously operationalize and test our mechanism-level predictions. We find that all three mechanisms can increase the likelihood of a subsequent alliance or acquisition, and in terms of the comparative choice between alliances versus acquisitions, partner-specific trust pulls towards alliances, and value certainty pulls towards acquisitions. We conclude with a discussion of the theoretical and empirical implications of our approach and method.*

Managerial summary: *This study focuses on an important corporate decision: When a firm has had an alliance with another firm, how would that experience affect the likelihood of a future alliance or acquisition with that same firm? We first suggest that it will depend on three factors: the level of trust that existed in that prior alliance, the extent to which specific work routines were developed, and the degree to which the firm was able to confidently assess the value of the partner firm's resources. We then find that trust is a particularly strong predictor of future alliances, while confidence regarding value more strongly predicts future acquisitions. In this way, we demonstrate more precisely how past corporate choices can affect (consciously or unconsciously) future ones.*

The corporate decision to engage in alliances and/or acquisitions has received significant research attention in the strategy literature (e.g., Vanhaverbeke, Duysters, & Noorderhaven, 2002; Villalonga & McGahan, 2005; Yin & Shanley, 2008), with *partner-specific alliance experience* emerging as an important predictor of both. Specifically, researchers have shown that firms with a history of prior cooperation will be more likely to enter into new alliances with each other (Chung, Singh, & Lee, 2000; Gulati, 1995b); other researchers, using real options reasoning, have shown that a firm having prior alliances with a partner is more likely to subsequently acquire that partner (Dalziel, 2009; Folta, 1998; Hagedoorn & Sadowski, 1999). Similarly, researchers focusing on the comparative governance choice (i.e., to ally vs. acquire) have shown that partner-specific alliance experience has a greater effect on subsequent alliance versus subsequent acquisition (Villalonga & McGahan,

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*Correspondence to: Thomas Mellewigt, Freie Universität Berlin, Garystr. 21, 14195 Berlin, Germany. E-mail: thomas.mellewigt@fu-berlin.de

2005; Wang & Zajac, 2007), while other comparative choice researchers find that partner-specific alliance experience exerts a stronger influence on subsequent acquisition of the partner rather than a subsequent alliance (Vanhaverbeke et al., 2002).

In this study, we theoretically disentangle when prior experiences are likely to lead to alliances and/or acquisitions, and employ a policy-capturing approach that is particularly well-suited for empirically testing this discriminating logic. Fundamentally, we move away from a reliance on aggregating/counting prior alliance experiences and move toward a disaggregated and mechanism-based analysis that allows us to capture qualitative differences in prior alliance experience.[1] In this way, we can more precisely build upon a growing body of prior research that has tended to operationalize different theoretical mechanisms while relying on similar measures. For example, Villalonga and McGahan (2005) refer to partner-specific trust and argue that trust explains alliance over acquisition choices. Wang and Zajac (2007) emphasize the importance of partner-specific routines for problem solving and learning. Finally, Vanhaverbeke et al. (2002) and Van de Vrande, Vanhaverbeke, and Duysters (2009) refer to knowledge about the partner/target firm's operations and resources to explain acquisition over alliance choices. Note that these studies identify three different key theoretical mechanisms, yet all use the *number* of prior alliances with the partner/target firm to operationalize their constructs.

Our intended contribution can therefore be summarized as follows: We first address explicitly the heterogeneous nature of prior alliance experiences by considering three fundamentally distinct mechanisms (as alluded to above) that are likely to influence governance choice: partner-specific (p-s) trust, p-s routines, and p-s value certainty. We then introduce a policy-capturing methodology to operationalize and test the effect of each of these three mechanisms on governance choices. As we discuss in detail in later sections, our choice of methodology allows us to rigorously assess the differential relevance of the three distinct mechanisms through which partner-specific experiences influence subsequent alliance and/or acquisition decisions.

[1] As Anderson et al. (2006, p. 102) suggest, a mechanisms-based approach analyzes the "theoretical cogs and wheels that explain how and why one thing leads to another." Such mechanisms are typically left implicit in broader theorizing, and making them explicit generates a deeper level of theorizing.

What is Partner-Specific Alliance Experience? Identifying Mechanisms

At first glance, it may seem self-evident that if a firm has had prior alliance experiences with a prospective alliance partner or acquisition target, it would be in a relatively advantageous situation (relative to a firm without such prior experiences) in evaluating the prospective opportunity. However, more careful consideration suggests that "prior alliance experience" is not a simple, unidimensional construct Indeed, Uzzi's (1997) widely-cited work on cooperation in the New York garment industry suggests three elements of social structure as particularly relevant in this context: "trust, joint problem-solving arrangements and fine-grained information transfer" (Uzzi, 1997, p. 42). Turning to the alliance literature, one finds reference to one or more of these three mechanisms when discussing the effect of partner-specific alliance experience on the comparative choice between alliances and acquisitions, i.e. partner-specific trust (Villalonga & McGahan, 2005; Wang & Zajac, 2007), partner-specific routines (Wang & Zajac, 2007) and partner-specific value certainty (Van de Vrande et al., 2009; Vanhaverbeke et al., 2002). We discuss each of these three below.

The Effect of Partner-Specific Trust

Akin to Rousseau, Sitkin, Burt, and Camerer (1998), Bradach and Eccles (1989), and also Uzzi (1997), we view trust – or more precisely "partner-specific trust" (p-s trust) – as the focal firm's belief that the former partner intends to deliver on made promises and would not act in self-interest at the focal firm's expense. P-s trust has repeatedly been connected to the performance of bilateral interorganizational exchange relationships (Das & Teng, 2001; Inkpen & Currall, 1998). Substantial literature contends that p-s trust lowers transaction costs and supports value creation, complementing governance relationships such as alliances and acquisitions (Gulati & Nickerson, 2008; Poppo & Zenger, 1998), where the efficient coordination of activities is crucial to goal fulfillment. P-s trust increases the efficiency of coordination activities because it enables open communication and straightforward, hassle-free arrangements.

Therefore, p-s trust speeds up negotiations, enhances conflict resolution (Currall & Inkpen,

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2002; Das & Teng, 1998; Gulati & Nickerson, 2008), facilitates knowledge transfer (Mesquita, Anand, & Brush, 2008; Van Wijk, Jansen, & Lyles, 2008) and counters appropriability concerns by reducing the fear of opportunistic hold-up (Dekker, 2004; Gulati, 1995a) Accordingly, p-s trust also facilitates organizational integration (Stahl, Larsson, Kremershof, & Sitkin, 2011; Valliere, Ni, & Wise, 2008). In sum, we suggest that p-s trust is conducive to interorganizational exchange; thus, we hypothesize that:

Hypothesis 1a (H1a): The greater the level of partner-specific trust, the greater the likelihood of either a subsequent alliance or acquisition (vs. no future cooperation).

Regarding the comparative choice between alliances and acquisitions we see several factors that would suggest that p-s trust influences the transaction value equation in favor of alliances. First, p-s trust unhinges the ubiquitous threat of opportunism and therewith lowers the relative advantage of a hierarchical control structure as described by Williamson (1981, 1991), thus that full control by means of acquiring the partner/target appears less necessary (Nooteboom, Berger, & Noorderhaven, 1997; Zaheer, McEvily, & Perrone, 1998).

Complementing the transaction cost perspective, we contend that the benefits of p-s trust in terms of joint value creation are relatively more important to alliances. Since alliances do not offer recourse to authority and fiat, value creation and productivity in alliances depend all the more on engaged collaboration and knowledge integration (J. Li, Zhou, & Zajac, 2009). While p-s trust is clearly beneficial to acquisition processes too, e.g., during negotiations, due diligence, and integration, its positive impact is limited because hierarchical organization and ownership constrain what p-s trust can do (Gulati & Nickerson, 2008; Mjoen & Tallman, 1997) and potentially even crowd out relational benefits (J. Li et al., 2009). In sum, we argue that p-s trust is, while being conducive to alliances and acquisitions, especially valuable in alliances. Thus, we hypothesize:

Hypothesis 1b (H1b): The greater the level of partner-specific trust, the greater the likelihood of a subsequent alliance versus an acquisition.

The Effect of Partner-Specific Routines

A second fundamental dimension upon which partner-specific alliance experience can differ significantly involves whether or not the experience involved (i.e., relied upon or generated) partner-specific routines. Consistent with Cohen and Bacdayan (1994) and Feldman and Pentland (2003) we view routines as patterned sequences of skilled actions that involve multiple, interdependent actors. More specifically, we focus on *partner-specific* routines (p-s routines), i.e., stable patterns of interaction between the focal firm and the partner/target firm. P-s routines store experiences of "how things are done" (e.g., connecting to the partner/target's operations, management systems, and procedures efficiently), which operate like mutually accepted heuristic guidelines of what actions are to be taken in a specific situation (Feldman & Rafaeli, 2002; Nelson & Winter, 1982). P-s routines contribute to bilateral exchange by providing: faster decision making due to lower ambiguity in the interpretation of information regarding patterns of interaction (Cohen & Bacdayan, 1994); higher reliability of outcomes (Cohen & Bacdayan, 1994); and greater comfort in interaction and the ability to learn from the partner/target (Becker, 2004; Hoang & Rothaermel, 2005). Consequently, p-s routines are a means of coordinating interdependent activities at comparatively low costs (Hoang & Rothaermel, 2005; Nelson & Winter, 1982; Zollo, Reuer, & Singh, 2002), and reflect a functioning interorganizational infrastructure between focal firm and partner/target firm which alleviates issues of concern for both alliances (Arikan & McGahan, 2010; Carayannopoulos & Auster, 2010; Gulati, 1995a; Villalonga & McGahan, 2005) and acquisitions (N. Li, Boulding, & Staelin, 2010; Vanhaverbeke et al., 2002). Therefore:

Hypothesis 2 (H2): The greater the level of partner-specific routines, the greater the likelihood of either a subsequent alliance or acquisition (vs. no future cooperation).

Regarding the effect of p-s routines on the comparative choice between alliances and acquisitions, prior work provides two opposing lines of argumentation, neither of which has been addressed empirically. The first line of argumentation contends that p-s routines increase the focal firm's

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desire for effective safeguarding by means of more hierarchical governance (Santoro & McGill, 2005). This is motivated by the fact that these routines are alliance-specific and not tradable, which exposes partners to opportunism through hold-up or haggling (Santoro & McGill, 2005). This argument suggests that p-s routines do not necessarily coincide with a substantially growing feeling of trust towards the partner/target firm. Once the inner workings of the focal firm are unveiled to the partner/target firm, opportunistic hold-up might in fact become easier. Thus, functioning routine-based structures could result in unintended knowledge transfer between firms, increasing the motivation to install proper safeguarding and thus preferring to acquire instead of ally.

However, a second line of argumentation emphasizes that p-s routines offer an incentive to recreate the context of their creation (i.e., keeping the modus operandi constant) in order to effectively reuse most of the gained capabilities (Anand & Khanna, 2000; Villalonga & McGahan, 2005; Zollo & Reuer, 2010). Thus p-s routines increase the likelihood of entering into a new alliance. Given the lack of a clear theoretical argument regarding the comparative choice of alliance versus acquisition, we forbear formulating a hypothesis.

The Effect of Partner-Specific Value Certainty

Our third dimension upon which we see heterogeneity in partner-specific alliance experience involves what we term p-s value certainty, which captures whether the prior alliance has led to a substantial accumulation of critical, private, partner-specific knowledge. This can encompass knowledge as to the partner's strengths and weaknesses, compatibility of goals, strategy, structure, manufacturing skills and operations, technologies, quality standards and aspirations, as well as culture (Al-Laham, Schweizer, & Amburgey, 2010; Porrini, 2004; Valliere et al., 2008; Wang & Zajac, 2007).[2]

Both alliances and acquisitions benefit from exclusive insights into the partner/target firm. This knowledge is applicable to future exchange relationships with the same firm in helping to overcome valuation difficulties (Balakrishnan & Koza, 1993; Zaheer, Hernandez, & Banerjee, 2010) and increase exchange efficiency (K. J. Mayer & Argyres, 2004; Wang & Zajac, 2007). A deep understanding of the partner-specific complementarities is critical to creating relational rents with the partner/target firm (Dyer & Singh, 1998; Harrison, Hitt, Hoskisson, & Ireland, 2001). Generally, the presence of sufficient critical information should facilitate both modes of governance. Therefore:

Hypothesis 3a (H3a): The greater the level of partner-specific value certainty, the greater the likelihood of either a subsequent alliance or acquisition (vs. no future cooperation).

With regards to the impact of p-s value certainty on the comparative choice between alliances and acquisitions, we expect that the positive effect of p-s value certainty will be stronger for acquisitions, thus increasing the likelihood of acquisition over alliance in a future relationship. Information about the true value and characteristics of a target firm effectively can mitigate the risk that some scholars identify as the most significant concern in acquisitions; namely, the largely irreversible commitment of resources to a potential "lemon" (Balakrishnan & Koza, 1993; Folta, 1998). The possession of inside information on the actual value of the partner/target firm's resources alleviates the challenge of finding a fair and appropriate purchase price with the partner/target firm and also diminishes the fear of adverse selection (Balakrishnan & Koza, 1993; Hoffmann & Schaper-Rinkel, 2001). In addition, it can substantially facilitate and shorten the time needed for due diligence processes as well as integration (Al-Laham et al., 2010; Harrison et al., 2001; Porrini, 2004).

By contrast, in a future alliance the partners would work together in only certain parts of the value chain (marketing, production, R + D), where the downside risk is much smaller than would be in an acquisition situation. In sum, the net value-differential provided by p-s value certainty is larger for acquisitions than for alliances (Van de Vrande et al., 2009; Vanhaverbeke et al., 2002). Therefore, we posit that:

Hypothesis 3b (H3b): The greater the level of partner-specific value certainty, the greater the

[2] Parter-specific value certainty is related to the concept of information asymmetry. Whereas the latter stresses that partners have different (levels of) information and may use their advantages opportunistically, the p-s value certainty construct simply assumes that a firm may have more or less knowledge about the target firm's characteristics.

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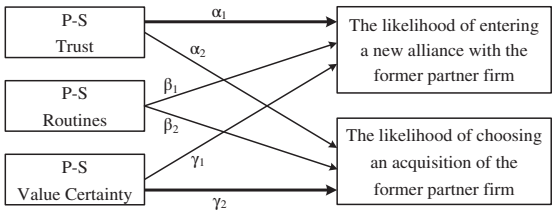

Figure 1. The hypothesized effects of partner-specific trust, partner-specific routines, and partner-specific value certainty on governance choice.
Hypothesis 1a: α_1, α_2 significantly positive.
Hypothesis 1b: $\alpha_1 > \alpha_2$.
Hypothesis 2: β_1, β_2 significantly positive.
Hypothesis 3a: γ_1, γ_2 significantly positive.
Hypothesis 3b: $\gamma_1 < \gamma_2$.

likelihood of a subsequent acquisition versus an alliance.

Figure 1 provides an illustration of all our hypotheses.

Methods and Data

Experimental Scenario Methodology

We chose a method that would allow us to observe managers' choices among governance forms in situations with different configurations of p-s trust, p-s routines, and p-s value certainty. We used an experimental scenario approach, which has a long tradition in disciplines like sociology and survey research (e.g., Atzmüller & Steiner, 2010; Auspurg & Hinz, 2015; Hox, Kreft, & Hermkens, 1991), management and organization (e.g., Aguinis & Bradley, 2014; Aiman-Smith, Scullen, & Barr, 2002; Graham & Cable, 2001; Karren & Barringer, 2002; Priem, Walters, & Li, 2011), and also strategic management (e.g., Connelly, Miller, & Devers, 2012; Di Stefano, King, & Verona, 2014; Reuer, Tong, Tyler, & Ariño, 2013).

Experimental scenario approaches comprise a number of methodologies which in strategy are often summarized as "policy capturing" (PC).[3] In PC studies informants evaluate hypothetical decision scenarios (Aiman-Smith et al., 2002). Scenarios are "carefully constructed description[s] of a person, object, or situation, representing a systematic combination of characteristics" (Atzmüller & Steiner, 2010, p. 128). They consist of a number of variables (also called cues or factors), and the product of all levels of all variables determines the population of possible configurations. In PC studies, like in factorial experiments, the goal is to estimate causal effects from the exogenous manipulation of the treatments (i.e., scenarios). PC methods have advantages over secondary-data and traditional self-report designs. Confined decision situations provide high control and a good assessment of reliability so that main and interaction effects can be isolated and examined in greater detail. The controlled manipulation of orthogonal variables assures minimal multicollinearity. Further, the indirect measurement helps to overcome social desirability issues and is less dependent on self-insight (Aiman-Smith et al., 2002; Karren & Barringer, 2002).

Despite these advantages, Aguinis and Bradley (2014) found that from 1994 to 2013 only a small number of PC studies was published in strategy journals (seven in the *Strategic Management Journal*). But strategy research has much to gain from PC studies, because "they are more likely to tap organization members' theories in use (i.e., the judgment policies they actually apply in practice) rather than their espoused theories (i.e., the judgment policies they wish to show to the world …)" (Priem et al., 2011, p. 558). While we cannot give a complete account of the PC methodology, there are crucial choices in the planning, executing, and analyzing stages of a PC study. In our case, these choices relate to external validity, the number of factors, levels, and scenarios, and the analytical strategy. Our discussion also aims at providing general recommendations for PC studies in the strategy field.

Realism and External Validity

A concern with PC studies is their potentially compromised external validity. For example, it has been argued that "in some high-stakes decision-making scenarios (e.g., mergers and acquisitions), presentation of hypothetical scenarios are not likely to produce the same responses as when those same situations occur in a natural setting." (Aguinis &

[3] We recognize that our study is a "paper people study" (Aguinis & Bradley, 2014; Gorman, Clover, & Doherty, 1978), but decided to use the terms "experimental scenario" and "PC" approach because these terms are often used in strategy, and the differences are not decisive for our purpose (Aiman-Smith et al., 2002).

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Bradley, 2014, p. 559). First, however, as Highhouse (2009, p. 554) has noted, attention should be "paid to the degree to which the treatment manipulation is valid, representative, and strong." Strong manipulations create strong situations (Meyer, Dalal, & Hermida, 2010; Mischel, 1968), in which respondents construe the decision context in the same way. If researchers recruit knowledgeable respondents and create a strong situation, the external validity of the PC study is improved (Karren & Barringer, 2002). Second, empirical evidence suggests that reactions to hypothetical scenarios do in fact resemble real-life decisions. For example, Wiseman and Levin (1996) found that subjects did not differ in their choices when the consequences were incurred or not.

To enhance the realism of our study, we crafted consistent scenario descriptions and made sure that experienced key informants were sampled. We contacted 1,614 corporate development and strategy managers via an online business and career network (XING).[4] Two hundred and thirty experts completed the questionnaire (response rate: 14.2%). We guaranteed anonymity, which relative to confidentiality comes with advantages regarding participation, and minimizes social desirability issues. We further employed a duplicate scenario (Aiman-Smith et al., 2002; Karren & Barringer, 2002) to check whether the respondents were consistent in their answers. The analysis yielded a test-retest correlation of .60 (contingency coefficient), which lead us to continue with only those respondents who had provided identical answers to the scenario and its duplicate. The final sample consisted of 160 experts (75.6% male, 24.4% female), with a mean of 13.8 years of professional experience (median: 13 years) and 3.5 years of acquisitions and alliances experience (median: 3 years). A majority (72%) had management positions (low, middle, or top-managers). Respondents came from a variety of industries, with no particular pattern of distribution.[5]

[4] XING is a German business network that competes with LinkedIn, among others; see https://www.xing.com/.

[5] These were Automotive, Aviation, Biotech, Engineering, Consulting, Financial Services and Insurance, Logistics, Media, IT and Telecommunications, Medical, Pharma, Chemical and Public Services.

Questionnaire

The online questionnaire had five parts.[6] The first part described the decision context. Prior to the main study, the description was validated with a panel of managers and academics to make sure that it represented a realistic choice situation using the common terms and examples as adopted in managerial practice. The description provided the historical background to the current situation, the decision purpose, and instructions for the completion of the questionnaire (Cooksey, 1996). We told the respondents that they worked for a company as the head of the corporate development department. The company pursued several growth projects for which they had to decide whether the resources of a hypothetical partner firm should be accessed via an alliance or acquisition. These partner firms were known to the focal firm from prior alliances, offered compatible and strategically critical resources, and were based in the German speaking economic area. We also assured that the focal firm's financial capacity allowed for both alliance and acquisition.

The second part defined the terms "alliance" and "acquisition" and provided an example scenario to avoid misinterpretations and reduce start-up effects (Aguinis & Bradley, 2014). The third part consisted of 12 scenarios and one duplicate. Each scenario was presented on a single page, and the 12 scenarios were presented in random order to avoid order effects (Aiman-Smith et al., 2002). In the fourth part we surveyed individual-level controls. The fifth part consisted of some manipulation checks, which allowed us to verify whether the manipulation was successful and as strong as expected. All texts were provided in German (we present our own translations here).

Experimental Measures

A critical question with PC studies is how many factors and levels per factor to use. A PC study should employ the most salient and important causal drivers for the decision context of interest (Aguinis & Bradley, 2014; Atzmüller & Steiner, 2010), and to identify these, both theoretical and practical knowledge is needed. Since too many factors create large amounts of complexity, we decided to stop at five factors (Aiman-Smith et al., 2002). These were the variables from our theory (p-s routines, p-s trust,

[6] We provide screenshots on request.

DOI: 10.1002/smj

and p-s value certainty) plus market uncertainty and asset specificity. We included the latter two because they have established theoretical links with interfirm cooperation decisions (Williamson, 1985). Like traditional control variables, they were used to isolate the p-s variables effects which are potentially confounded with uncertainty and asset specificity effects.

The wording of the variables is essential for a successful experimental manipulation. It requires a balancing act between consistency with established measures in academic literature and creating a hands-on environment that is reflective of the key informants' knowledge. During iterative feedback processes with our expert panel, we determined the essential key words and final wordings which resulted in short texts of one or two sentences depicting each level of every factor. In the case of "p-s trust" we relied on Rousseau et al. (1998), R. C. Mayer, Davis, and Schoorman (1995) and Zaheer et al. (1998); the goal was to create a sense of accepted vulnerability and positive behavioral expectations on the side of the respondents. With respect to "p-s routines" we relied on the definitions by Feldman and Pentland (2003) and Becker (2004) to describe how partner-specific alliance experience did or did not result in stable patterns of interaction, for example with regard to communication, conflict resolution, and decision-making. The levels of "p-s value certainty" were based on Balakrishnan and Koza's (1993) ideas but adapted to our context so that they described whether or not the focal firm was able to gain exclusive insights into the partner firm (e.g., processes, business practices, capabilities, and technologies) by means of partner-specific alliance experience.

As noted above, to control for TCE concerns (Williamson, 1975, 1985) we included market uncertainty and asset specificity. Market uncertainty had two levels (0 = low, 1 = high), and asset specificity had three levels (0 = no specific investments, 1 = unilateral specific investments, 2 = mutual specific investments).

Incomplete Block Design

Since the p-s variables and market uncertainty had two levels each, and asset specificity three, a population of $3 \times 2^4 = 48$ scenarios resulted. There is no clear recommendation regarding how many scenarios a respondent should answer. Some authors argue that both too few (start-up effects) and too many (fatigue effects) scenarios per respondent create problems (Aguinis & Bradley, 2014; Atzmüller & Steiner, 2010). In our context 48 scenarios per respondent were considered too much. If participants respond to a subset of scenarios only (rather than to the full set), scenarios can be selected randomly, or an experimental plan can be used for selection. Random selection results in a complex random confounding structure (Atzmüller & Steiner, 2010), whereas a fractional design based on an experimental plan can be used to deliberately confound selection with main and interaction effects.

We decided to use an experimental plan and employed an incomplete block design (Cochran & Cox, 1957; Graham & Cable, 2001). In this design, the population of scenarios is divided into blocks of equal size, such that respondents reply to a block of scenarios only. Since all blocks, and thus all scenarios, are used, incomplete block designs "require fewer scenarios per participant than full factorial designs," but there is no "loss of the validity of the results generated" (Graham & Cable, 2001, pp. 28–29). For the last condition to hold, a few rules must be followed. First, "The main effects of a factor will be kept clear of block effects if every block contains each level of the factor the same number of times." (Cochran & Cox, 1957, p. 203). Hence, block size must be a multiplier of the factor levels involved (6 scenarios per block, or a multiple of 6, if factor levels are 3 and 2, like in our case). As a compromise between too few and too many scenarios, we decided to use blocks of 12 scenarios per respondent. We present the experimental plan in Appendix B).

Other Measures

The *dependent variable* is governance choice. We asked the respondents to indicate whether the resources offered by the partner firm were best accessed via an alliance or an acquisition. As a third option, the respondents could indicate that under the given circumstances neither an alliance nor an acquisition type of future cooperation was a suitable option.

At the individual level we controlled for gender, professional and governance choice experience (both in years), and management position (no managerial position, low, middle, top management). We also controlled for managerial hubris (Hambrick & Mason, 1984; Hayward & Hambrick, 1997; Roll,

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1986) by capturing its personality dimension, that is, narcissism (Chatterjee & Hambrick, 2007). To this end, we constructed a German version of the NPI-16, a short version of the Narcissistic Personality Inventory (NPI) (Ames, Rose, & Anderson, 2006). Finally, to achieve a balanced PC design (i.e., where each scenario receives the same number of responses) blocks need to be assigned to individuals equally often. Since we excluded some respondents because of missing test-retest consistency, our design was potentially unbalanced, and we used block dummies as a control (Atzmüller & Steiner, 2010).

Pretest and Manipulation Checks

Prior to the main study, a pretest was conducted. In sum, 34 individuals participated in the pretest, and 21 (18 full-time students from a major German research university, and three employed individuals) completed the questionnaire. All participants were German, and two thirds (14) were female. All participants were encouraged to provide comments which were filed and used to realize minor changes in the wording, spelling, and formatting of the survey. The pretest also lead us to include an introductory sample scenario and a short cover letter issued and signed by the principal researcher. The average response time was slightly more than 23 minutes, including the provision of comments, which we considered appropriate.

Prior and parallel to hypothesis testing, we conducted some validity checks. At the end of the questionnaire, we asked the respondents to rank the five experimental factors in terms of their importance for governance choice decisions (from "1 – most important" to "5 – least important"). Overall, p-s value certainty received the highest ranking (mean: 2.19; median: 2), followed by p-s trust (mean: 2.71; median: 3) and market uncertainty (mean: 2.74; median: 2), asset specificity (mean: 3.06; median: 3), and finally p-s routines (mean: 4.3; median: 5).[7] This ranking resembled the multivariate results discussed below; because the implicit decision processes triggered by the treatments received support from explicit rankings we conclude that the manipulation of the factors was successful and operated like expected.

Finally, we asked some further validation questions. An example item is "Trust enables the efficient coordination of workflows with the partner firm." These control items were measured on five-point rating scales (with "disagree – agree" anchors) and correlated with the rankings of the experimental factors. Overall, the correlational patterns were theoretically sound and statistically significant (e.g., the trust item and the trust ranking were correlated with $r = -0.40$; note that lower numbers mark higher trust rankings, thus the negative correlation).

Analytical Strategy

In our study, the level of analysis is the scenario. Because each respondent judged 12 scenarios allocated to individuals via a random draw of one of four blocks, responses are potentially correlated within individuals. We used the Intraclass Correlation Coefficient (Shrout & Fleiss, 1979) to estimate the degree of data dependency. Across all observations the ICC(1,1) was 0.11. When also taking blocks and scenarios into account, the ICC(2,1) ranged from 0.11 (block 3) to 0.14 (block 2).[8] The analyses revealed that 11–14% of the variance in governance choice was attributable to respondents (i.e., person effects), and that 86–89% came from the treatments. Thus, we saw that the manipulations were effective. However, we also observed a substantial degree of data non-independence that we needed to address.

The two standard solutions for such non-independence are multilevel or Hierarchical Linear Modeling (HLM) (Hox et al., 1991; Raudenbush & Bryk, 2002), which exploits a random-effects strategy, or fixed-effects methods such as person dummies (Di Stefano et al., 2014). We decided to use person dummies for two reasons. First, trading off estimation efficiency and sample

[7] Except for the p-s trust and market uncertainty means, all pairwise comparisons of means (t-tests) were statistically significant. Exact results are provided in Appendix A.

[8] All ICC estimates (ICC(1,1) and ICC(2,1)) were statistically significant (see Appendix A). For the ICC analyses, we made the simplifying assumption that our governance choice decisions may be arranged on a continuum from market (no decision for either alliance or acquisition) to hierarchy (acquisition), with alliance as the hybrid type in between. This allowed us to estimate the ICC coefficients from a simple linear model. As a (more complex and difficult to interpret) alternative, one could estimate a (multinomial) logit model and partition the variance across levels by assuming a latent random effect on level-1 with a standard logistic distribution (where the level-1 variance is expressed as $\pi^2/3 \approx 3.29$; Raudenbush & Bryk, 2002; Snijders & Bosker, 1999).

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size characteristics, we concluded that with only 160 respondents (relatively small cross section) but 12 repeated observations per respondent (relatively large number of repetitions), fixed-effects were preferred. Second, we found some selectivity after we excluded the non-reliable respondents. In the reduced sample, we saw that the four blocks were unequally distributed among the respondents, which resulted in unequal distributions of scenarios as well. Given that the block effect may be overshadowed by unobserved individual-level heterogeneity (given the observed selectivity), fixed person effects were our preferred choice.

Because governance choice (our DV) had three categories, we estimated multinomial logit regression models. A dependent variable with three categorical outcomes requires the calculation of two equations, one for each category relative to the reference category (Menard, 2002):

$$\log\left[\mathrm{P}\left(\mathrm{y}=\mathrm{j}\right)/\mathrm{P}\left(\mathrm{y}=\mathrm{J}\right)\right]=\beta_{\mathrm{j}}\mathrm{X};$$

with $\mathrm{j}=1,\ \ldots\ ,\mathrm{J}-1$; $\beta_{\mathrm{j}=}[\beta_{\mathrm{j}0,}\ \ldots\ \beta_{\mathrm{jm}}]$ and $\mathrm{X}=[\mathrm{X}_1,\ \ldots\ ,\mathrm{X}_{\mathrm{m}}]$

Accordingly, all J−1 probabilities can be denoted as:

$$P\left(y=j\right)=\frac{e^{\beta jX}}{1+\sum_{j=1}^{J-1}e^{\beta jX}},$$

while the probability of membership in the reference category equals:

$$P\left(y=J\right)=\frac{1}{1+\sum_{j=1}^{J-1}e^{\beta jX}}.$$

Results

The final sample consisted of 1,920 governance decisions from 160 key informants. We obtained 794 alliance and 642 acquisition decisions, and 484 observations with no decision for either one of the two alternatives. Because our main models employed person fixed effects, we did not include the individual-level covariates in these models. Table 1 reports descriptive information for all variables, and correlations among the dependent variable's levels and all independent variables (level of analysis is the scenario).

The correlations reveal that p-s trust, p-s routines, and p-s value certainty are all negatively related to the "no decision" category of the DV. P-s trust is positively correlated with alliances, and p-s value certainty relates positively to acquisitions. These results are in line with our expectations. Market uncertainty is positively correlated with alliances, and negatively with acquisitions, and the relationships between asset specificity and the DV's categories are small and rather negligible.

H1a states that p-s trust is positively associated with the likelihood of both alliance and acquisition choices (relative to none of the two forms). As shown in Models 1 and 2 of Table 2, the raw coefficients are both positive and significant. Figure 2 illustrates that a change in p-s trust from zero to one induces an increase in the average predicted probability of an acquisition outcome from 32 to 35%, of an alliance outcome from 31 to 51%, while the predicted probability of "no decision" declines drastically. Similarly, H2 is supported. With p-s routines moving from zero to one the predicted probability of "alliance" increased by 9%. The associated change in the predicted probability for "acquisition" amounts to 7%. At the same time, the predicted probability of "no decision" drops by 15%. Further, our results support H3a suggesting that p-s value certainty is positively associated with both the likelihood of alliance and acquisition choices when compared to no decision. Figure 2 shows that as p-s value certainty goes up from low to high levels, the average predicted probability of "no decision" strongly declines, while acquisitions become much more likely, and alliances remain relatively stable.

Model 3 of Table 2 is employed to test H1b and H3b, which examine the differential effect of p-s trust and p-s value certainty on the comparative choice between alliance and acquisition. H1b suggests that p-s trust has a stronger relationship with alliances than with acquisitions. Model 3 shows that the coefficient for alliance is positive and significant, which is supportive of H1b. H3b argues that p-s value certainty will encourage acquisitions more strongly than alliances. Model 3 shows a negative and significant coefficient for alliance choice, which is in support of H3b. The differential effects of p-s trust and p-s value certainty are nicely illustrated by Figure 2 and further explained by the marginal effects reported in Table 3. P-s trust has a strong and significant marginal effect on alliances, but the marginal effect on acquisitions is low and insignificant. P-s value certainty predicts acquisitions, but

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Table 1
Correlations between the Dependent and the Independent Variables

Variable	M	s.d.	GC1	GC2	GC3
Governance choice					
No decision	0.25	0.43			
Alliance	0.41	0.49			
Acquisition	0.33	0.47			
P-S trust	0.5	0.50	−0.29 (.00)	0.21 (.00)	0.04 (.05)
P-S routines	0.5	0.50	−0.20 (.00)	0.10 (.00)	0.08 (.00)
P-S value certainty	0.5	0.50	−0.23 (.00)	−0.03 (.19)	0.24 (.00)
Market uncertainty	0.5	0.50	0.00 (.83)	0.19 (.00)	−0.20 (.00)
Asset specificity					
No investments	0.33	0.47	−0.01 (.79)	0.01 (.82)	−0.00 (1.00)
Unilateral investments	0.33	0.47	0.03 (.16)	−0.04 (.05)	0.02 (.47)
Mutual investments	0.33	0.47	−0.03 (.25)	0.04 (.09)	−0.02 (.47)
Gender (male)	0.76	0.43	−0.07 (.00)	0.03 (.26)	0.03 (.13)
Professional experience	13.78	7.93	0.04 (.09)	−0.01 (.80)	−0.03 (.19)
Governance experience	3.52	3.65	0.07 (.00)	−0.09 (.00)	0.03 (.26)
Managerial position					
No management	0.12	0.32	0.01 (.56)	0.01 (.70)	−0.02 (.35)
Lower management	0.16	0.37	−0.07 (.00)	0.05 (.02)	0.01 (.63)
Middle management	0.39	0.49	0.01 (.63)	0.02 (.49)	−0.03 (.24)
Top management	0.33	0.47	0.04 (.10)	−0.07 (.00)	0.03 (.14)
Narcissism	0.45	0.18	0.02 (.44)	−0.01 (.54)	−0.00 (.94)
Blocks					
Block 1	0.24	0.43	−0.03 (.22)	−0.00 (.95)	0.03 (.23)
Block 2	0.21	0.41	0.02 (.36)	0.03 (.20)	−0.05 (.03)
Block 3	0.29	0.45	0.07 (.00)	−0.07 (.00)	0.02 (.49)
Block 4	0.26	0.44	−0.06 (.01)	0.05 (.03)	0.00 (.87)

Notes. GC1–GC3: Categories of the DV (1 = No Decision, 2 = Alliance, 3 = Acquisition); P-S = Partner-Specific; N = 1,920; M = mean; s.d. = standard deviation; reference category for gender is female; exact *p*-values in parentheses.

is insignificantly related to alliances. The marginal effects are informative because p-s trust and p-s value certainty have effects on both alliance and acquisition choices over no form of interfirm cooperation, but the probability for either one category depends on how all the other effects balance out.

Our analysis also indicates that p-s routines contribute to alliance and acquisition choices over no form of cooperation, but do not predict the choice between alliances and acquisitions. Moreover, it is important to recognize that a realistic decision context is not exclusively represented by relational considerations. Market uncertainty has positive effects on alliances, and negative effects on acquisitions (in both cases as compared to no decision), and also increases the probability of an alliance over an acquisition. TCE traditionally suggests a higher degree of integration when market uncertainty is high. However, our findings are indicative of real option considerations that have already been substantiated in other contexts, as for example by Folta (1998). In times of market uncertainty, alliances might turn out to be a viable investment option while minimizing downside commercial risk (Dalziel, 2009; Folta, 1998). Asset specificity, in our models, is virtually unrelated to all governance choices. An explanation is that in face of a reliable exchange history between the transaction partners, i.e., prior partner-specific alliance experience, specific investments become less decisive (Eckhard, Mellewigt, & Weller, 2009). All potential covariate effects and person-based unobserved heterogeneity were absorbed by the person dummies.

In a last step we conducted a novel robustness check. We tested for start-up and fatigue effects by estimating 12 separate multinomial logit models for each of the 12 positions of scenarios in the experiment. In other words, a model used all first, second, third, etc. scenarios which the respondents replied to. These models are potentially insightful, yet some caveats apply: Because we cannot employ fixed-effects in these models (since each individual is surveyed only once in each model), we used

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Table 2
Results of Multinomial Logistic Regressions with Fixed-Effects

	Model 1		Model 2		Model 3	
	Alliance vs. no decision		Acquisition vs. no decision		Alliance vs. acquisition	
Intercept	−2.36 (1.05)	[−4.42; −0.30]	−1.25 (.97)	[−3.15; 0.65]	−1.11 (.82)	[−2.71; 0.49]
P-S trust	2.57 (.19)	[2.20; 2.94]	2.12 (.19)	[1.74; 2.50]	0.45 (.12)	[0.20; 0.69]
P-S routines	1.59 (.18)	[1.23; 1.95]	1.60 (.19)	[1.23; 1.97]	−0.01 (.12)	[−0.25; 0.23]
P-S value certainty	1.62 (.18)	[1.28; 1.97]	2.53 (.18)	[2.17; 2.89]	−0.90 (.13)	[−1.15; −0.66]
Market uncertainty	0.71 (.16)	[0.39; 1.03]	−0.52 (.17)	[−0.85; −0.18]	1.23 (.12)	[0.98; 1.47]
Asset specificity						
Unilateral investments	−0.32 (.19)	[−0.70; 0.06]	−0.17 (.20)	[−0.56; 0.22]	−0.15 (.15)	[−0.43; 0.14]
Mutual investments	0.18 (.20)	[−0.20; 0.57]	0.07 (.20)	[−0.32; 0.47]	0.11 (.14)	[−0.17; 0.39]
Person fixed-effects	Yes		Yes		Yes	
Observations			1,920			
Individuals			160			
−2 Log-likelihood			−1352.45			
LR Chi^2 (330)			1437.81 (.00)			
Pseudo-R^2 (McFadden)			0.35			

Notes. Raw coefficients (standard errors in parentheses) [95% confidence intervals in brackets]. LR Chi^2: exact *p*-value in parentheses. Marginal effects are reported in Table 3. P-S = Partner-Specific.

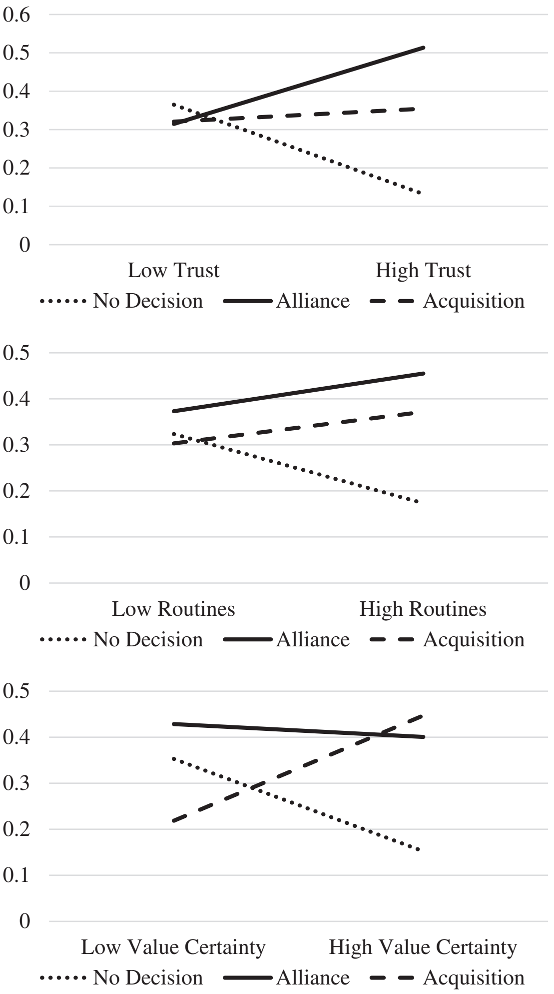

Figure 2. Marginal effects and predicted values.

the control variables described above; however, this makes it harder to directly compare the estimates across modeling strategies. Given the relatively low number of observations per model, the maximum likelihood estimation of the multinomial logit model may be problematic because its large sample properties may be compromised. Along the same line, the statistical power of these models is smaller as compared to the models presented above such that Type II errors are more likely. Finally, because the scenarios were drawn in random order within blocks, the models may contain unbalanced sets of scenarios. We found that the simple main effects (of p-s trust on alliances and p-s value certainty on acquisitions) were reliably detected in all but three (out of 24) instances. Moreover, later position models were slightly more likely to detect a significant,

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Table 3
Marginal Effects

	No decision dy/dx (s.e.)		Alliance dy/dx (s.e.)		Acquisition dy/dx (s.e.)	
P-S trust	−0.22 (0.01)	[−0.24; −0.20]	0.19 (0.02)	[0.15; 0.22]	0.03 (0.02)	[−0.00; 0.07]
P-S routines	−0.15 (0.01)	[−0.18; −0.12]	0.08 (0.02)	[0.04; 0.12]	0.07 (0.02)	[0.03; 0.10]
P-S value certainty	−0.19 (0.01)	[−0.21; −0.17]	−0.03 (0.02)	[−0.07; 0.01]	0.22 (0.02)	[0.18; 0.25]
Market uncertainty	−0.01 (0.01)	[−0.04; 0.01]	0.19 (0.02)	[0.15; 0.22]	−0.17 (0.02)	[−0.21; −0.14]
Asset specificity						
Unilateral investments	0.02 (0.02)	[−0.01; 0.01]	−0.03 (0.02)	[−0.08; 0.01]	0.01 (0.02)	[−0.03; 0.06]
Mutual investments	−0.01 (0.02)	[−0.05; 0.02]	0.02 (0.02)	[−0.02; 0.01]	−0.01 (0.02)	[−0.06; 0.03]

Notes. Marginal effects (standard errors s.e. in parentheses) [95% confidence intervals in brackets]. Marginal effects (for factorial variables) represent the discrete change from the base level, which is 0, to 1; note that for asset specificity the base level is "no specific investments." P-S = Partner-Specific.

discriminating effect of p-s trust and p-s value certainty on alliances over acquisitions; however, these differences between early and later models were small. We interpret these results as supportive for our theory and the scenario experiment's validity, and as helpful for understanding the functioning of the experiment. We report the p-s trust, p-s routines, and p-s value certainty coefficients of these analyses in Appendix C.

Discussion and Conclusion

While a growing body of research has considered the relevance of prior p-s experiences in predicting future alliances/acquisitions, the presumed theoretical mechanisms and the empirical findings have been surprisingly divergent. We sought to provide additional clarity to this situation by (a) explicitly addressing three alternative mechanisms through which prior p-s experience can affect future alliances and/or acquisitions, and (b) operationalizing and rigorously testing our mechanism-level predictions with a policy-capturing methodology.

In terms of the three key mechanisms (p-s trust, p-s routines and p-s value certainty), we found, as predicted, that p-s trust facilitates subsequent alliances and acquisitions, and that the effect is significantly stronger for alliance (vs. acquisition). We also found that the presence of p-s routines triggers both subsequent alliance and subsequent acquisition choices (and is an equally strong predictor of each). Finally, we found that p-s value certainty motivates later alliances and acquisitions, and does so more strongly for acquisitions versus alliances (as predicted). Taken together, our results highlight that future research would benefit from a more explicit consideration of the heterogeneity in prior alliance experiences. Indeed, our results suggest that a failure to consider – both theoretically *and* empirically – the alternative mechanisms through which prior alliance experiences influence future interfirm combinations may mask significantly different effects. While reliance on simple counts of prior alliance experiences can be justified in earlier work, we suggest that future research should endeavor to better match presumed theoretical mechanisms with specific alternative empirical operationalizations of those mechanisms.

In terms of the managerial relevance of our study, we see our study as addressing a simple but important question: Given that the "shadow of the past" experiences will likely loom large when considering current decisions (e.g., whether to do an alliance or acquisition), what are the particularly relevant aspects of those prior experiences, and how can we improve managerial – or even investor – decision making as a result of knowing this? We see our approach, method, and findings as offering assistance in helping senior managers (and boards) avoid bad decisions. For example, the board of Firm A, when asked by a senior-level manager to "green-light" a new alliance with Firm B (with whom Firm A had a prior alliance), would be advised to probe the nature of that prior relationship and ask about the specifics of that prior relationship (i.e., Was it high-trust? Were working routines built? What did we specifically learn about the value they offer?) One should not expect to see a simple learning-by-doing effect, given the heterogeneity involved in the doing.

In terms of limitations and future extensions of our study, we acknowledge that our methodological approach meant limits on how many cues we could use and manipulate (hence our focus on three

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fundamental mechanisms that have individually or collectively been considered in prior research). We endeavored to provide our key informants with a realistic and meaningful decision context. Given the importance in policy-capturing studies of incorporating managerial characteristics into strategic choice models, we assembled an expert panel and included individual-level control variables in our study. However, given that policy-capturing studies rely on hypothetical decision-making scenarios, we cannot rule out possible differences with actual decision-making situations in the business world (external validity).

In terms of how our work can spur new research, we see a number of future research avenues: First, the strength of our findings and our focus on specific mechanisms suggest a likely benefit to opening the "black box" of partner-specific alliance experience even wider to consider additional contextual and firm-specific characteristics. This echoes recent calls for more contingency models in this area of strategy research (Steensma & Corley, 2001; Van de Vrande et al., 2009; Villalonga & McGahan, 2005). Future research could also investigate some of our partner-specific alliance experience mechanisms in greater depth. For example, might different types of trust, e.g., ability-, benevolence- or integrity-based trust (R. C. Mayer et al., 1995) have the same effect on the alliance vs. acquisition decision? When might distrust drive decisions towards acquisitions vs. leading to alliances with more detailed contracts and exit options? One could also consider more fine-grained governance choice variables that distinguish between acquisitions, equity-based alliances (such as joint ventures) and non-equity/contractual alliances (Dyer, Kale, & Singh, 2004).

We also hope that our use of the policy-capturing method, with its potential for a more rigorous research design, would also contribute to the methodological plurality in the arena of alliance and acquisition research. Finally, and more generally, we hope that our approach motivates strategy and organizational researchers to discuss more specifically and precisely which theoretical mechanisms are presumed to be driving their predictions. Moving closer to mechanism-based explanations (with distinct measures for each) could represent an important advance for strategic management scholars interested in explanation and understanding, as well as prediction.

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Appendix A

Results from *t*-tests and ICC analyses

Mean rank differences and *t*-tests	P-S routines	P-S value certainty	Market uncertainty	Asset specificity
P-S trust	−1.59 (.00)	0.53 (.00)	−0.03 (.44)	−0.35 (.02)
P-S routines		2.11 (.00)	1.56 (.00)	1.24 (.00)
P-S value certainty			−0.55 (.00)	−0.88 (.00)
Market uncertainty				−0.33 (.02)

Notes. N = 160; estimates are mean differences; exact *p*-values (from paired *t*-tests) in parentheses. P-S = Partner-Specific.

ICC analysis	ICC(1,1)
12 scenarios	0.11 (.00)
ICC analyses (scenarios and blocks)	ICC(2,1)
12 scenarios in block 1	0.13 (.00)
12 scenarios in block 2	0.14 (.00)
12 scenarios in block 3	0.11 (.00)
12 scenarios in block 4	0.12 (.00)

Notes. N = 160; 12 scenarios (ICC[1,1]) nested in four blocks (ICC[2,1]); exact *p*-values in parentheses. ICC = Intraclass Correlation Coefficient.

Appendix B

Population of $3 \times 2^4 = 48$ scenarios and incomplete blocks

Block 1	Block 2	Block 3	Block 4
00000	01000	00010	01010
00001	01001	00011	01011
00002	01002	00012	01012
00110	01110	00100	01100
00111	01111	00101	01101
00112	01112	00102	01102
11000	10010	11010	10000
11001	10011	11011	10001
11002	10012	11012	10002
11110	10100	11100	10110
11111	10101	11101	10111
11112	10102	11102	10112

Notes. Digits (in order of appearance) refer to p-s trust, p-s routines, p-s value certainty, market uncertainty, and asset specificity; levels are low (0) and high (1) for the first four variables, and no (0), unilateral (1), and mutual investments (2) for asset specificity. Each block contributes equally to all factors, such that all main effects can be estimated free from block effects.

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Appendix C

Results of start-up and fatigue effects analyses

Variable	Position	Alliance vs. no decision	Acquisition vs. no decision	Alliance vs. acquisition
P-S trust	01	1.49 (0.60) [0.31; 2.67]	0.66 (0.64) [−0.60; 1.92]	0.82 (0.47) [−0.10; 1.75]
	02	1.35 (0.79) [−0.20; 2.90]	1.15 (0.78) [−0.38; 2.69]	0.20 (0.45) [−0.68; 1.07]
	03	1.90 (0.58) [0.78; 3.03]	1.58 (0.64) [0.33; 2.82]	0.32 (0.47) [−0.60; 1.24]
	04	1.54 (0.67) [0.23; 2.86]	1.47 (0.67) [0.15; 2.79]	0.07 (0.47) [−0.85; 1.00]
	05	2.48 (0.69) [1.13; 3.84]	2.97 (0.74) [1.52; 4.42]	−0.49 (0.51) [−1.49; 0.52]
	06	1.74 (0.59) [0.58; 2.90]	0.81 (0.58) [−0.33; 1.94]	0.93 (0.52) [−0.08; 1.94]
	07	1.33 (0.60) [0.15; 2.50]	1.01 (0.64) [−0.24; 2.26]	0.31 (0.54) [−0.74; 1.37]
	08	1.75 (0.54) [0.69; 2.81]	1.03 (0.59) [−0.12; 2.19]	0.72 (0.47) [−0.19; 1.64]
	09	2.68 (0.70) [1.30; 4.06]	1.81 (0.72) [0.39; 3.22]	0.87 (0.46) [−0.04; 1.78]
	10	2.63 (0.63) [1.40; 3.85]	2.25 (0.72) [0.84; 3.67]	0.37 (0.53) [−0.68; 1.42]
	11	3.41 (0.74) [1.97; 4.86]	2.39 (0.69) [1.02; 3.75]	1.03 (0.58) [−0.11; 2.17]
	12	2.06 (0.60) [0.89; 3.23]	0.81 (0.59) [−0.34; 1.97]	1.24 (0.51) [0.24; 2.25]
P-S routines	01	1.61 (0.62) [0.39; 2.83]	1.61 (0.67) [0.31; 2.92]	−0.00 (0.46) [−0.91; 0.91]
	02	2.21 (0.77) [0.69; 3.72]	1.62 (0.78) [0.10; 3.14]	0.59 (0.43) [−0.26; 1.44]
	03	0.83 (0.59) [−0.33; 1.99]	0.80 (0.65) [−0.48; 2.08]	0.03 (0.47) [−0.88; 0.95]
	04	1.19 (0.58) [0.05; 2.34]	0.42 (0.60) [−0.75; 1.59]	0.77 (0.44) [−0.10; 1.63]
	05	1.45 (0.71) [0.06; 2.83]	1.25 (0.74) [−0.20; 2.70]	0.19 (0.54) [−0.86; 1.25]
	06	1.64 (0.60) [0.46; 2.81]	0.94 (0.58) [−0.19; 2.07]	0.70 (0.52) [−0.31; 1.71]
	07	2.00 (0.59) [0.84; 3.15]	1.90 (0.65) [0.62; 3.18]	0.10 (0.52) [−0.92; 1.12]
	08	0.72 (0.55) [−0.37; 1.80]	0.79 (0.60) [−0.39; 1.96]	−0.07 (0.47) [−0.99; 0.85]
	09	2.04 (0.73) [0.61; 3.48]	2.15 (0.75) [0.68; 3.63]	−0.11 (0.46) [−1.02; 0.80]
	10	1.21 (0.59) [0.06; 2.35]	2.10 (0.69) [0.74; 3.46]	−0.89 (0.54) [−1.94; 0.16]
	11	0.81 (0.73) [−0.62; 2.24]	0.90 (0.68) [−0.43; 2.22]	−0.08 (0.56) [−1.18; 1.02]
	12	1.76 (0.59) [0.61; 2.91]	1.04 (0.58) [−0.10; 2.19]	0.72 (0.50) [−0.26; 1.70]
P-S value	01	0.62 (0.61) [−0.58; 1.82]	1.20 (0.66) [−0.10; 2.50]	−0.58 (0.49) [−1.53; 0.38]
	02	1.02 (0.67) [−0.30; 2.34]	1.89 (0.66) [0.59; 3.19]	−0.87 (0.45) [−1.75; 0.01]
	03	0.74 (0.56) [−0.36; 1.83]	2.18 (0.64) [0.92; 3.43]	−1.44 (0.50) [−2.42; −0.46]
	04	2.44 (0.67) [1.13; 3.75]	2.71 (0.66) [1.42; 4.00]	−0.27 (0.48) [−1.21; 0.67]
	05	1.30 (0.60) [0.11; 2.48]	2.10 (0.64) [0.84; 3.35]	−0.80 (0.49) [−1.76; 0.16]
	06	1.92 (0.64) [0.66; 3.18]	2.31 (0.61) [1.10; 3.51]	−0.38 (0.55) [−1.46; 0.69]
	07	1.21 (0.57) [0.09; 2.34]	2.19 (0.65) [0.93; 3.46]	−0.98 (0.53) [−2.01; 0.05]
	08	0.99 (0.57) [−0.12; 2.10]	1.57 (0.60) [0.39; 2.75]	−0.58 (0.48) [−1.52; 0.36]
	09	1.61 (0.57) [0.48; 2.73]	1.47 (0.58) [0.33; 2.60]	0.14 (0.46) [−0.77; 1.05]
	10	1.68 (0.60) [0.51; 2.86]	3.82 (0.71) [2.42; 5.23]	−2.14 (0.57) [−3.25; −1.03]
	11	−0.29 (0.65) [−1.57; 0.99]	1.43 (0.56) [0.32; 2.53]	−1.72 (0.55) [−2.79; −0.65]
	12	0.41 (0.56) [−0.69; 1.50]	1.09 (0.56) [−0.00; 2.19]	−0.68 (0.48) [−1.62; 0.26]

Notes. Raw coefficients (standard errors in parentheses) [95% confidence intervals in brackets]. Estimates are taken from 12 separate multinomial logistic regression models. All models contain the market uncertainty and asset specificity variables plus the full set of covariates (gender, professional experience, experience in M&A, managerial position, narcissism, block). All full tables are available upon request. P-S = Partner-Specific.

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