
The research for this paper began in 1993. At that time I was still casting around for a viable dissertation topic and Oliver Williamson (my PhD advisor) urged me to look at hybrid organizations because there was “so much to be done” in this area. In the management literature, research on strategic alliances and other inter-firm or hybrid organizations had recently taken off, spurred in part by a high-profile conference and edited volume featuring some of the pioneers in alliance research (Contractor & Lorange, 1988). Early treatments in this literature sought to understand and classify the variety of organizational forms loosely collected under the alliance rubric (see also Killing, 1988; Lorange & Roos, 1992; Gomes-Caseres, 1996). While useful starting points, these taxonomies lacked a theoretical underpinning and opinions differed on how firms should choose among alternative forms of cooperative agreements. As such, the early alliance literature tended to be fragmented and non-cumulative – something that indeed appeared as an opportunity for a student of TCE!

Within TCE the most closely-related prior work was Gary Pisano’s (1988) dissertation research on the organization of R&D collaboration (see also Pisano, 1989; Pisano, Russo & Teece, 1988), analyzing the make-or-buy decision for R&D. In his study Pisano asked under what circumstances external R&D is performed in an equity joint venture and when it is done by an independent R&D firm under contract. Consistent with prior work on vertical integration, Pisano found that equity arrangements (the closest to the “make” decision in traditional vertical integration studies) were favored when contracting hazards related to small-numbers bargaining and uncertainty were present to a significant degree. More specifically, collaborators tended to use equity forms when the activities included R&D, when the scope of collaboration extended beyond a
single project, and when collaboration took place in industry segments with relatively few active players (Pisano, 1989).

Building directly on Pisano’s work, my dissertation project (Oxley, 1995, 1997, 1999a, 1999b) sought to push TCE research on hybrid organizations ahead in three ways: First, while the R&D alliances in Pisano’s study could essentially be seen as variants on the buyer-supplier relationships common to vertical integration decisions (the traditional purview of TCE) I attempted to make the connection between TCE and managerial work on strategic alliances more explicit.¹ Second – also connecting with the managerial work on alliances – I wanted to go beyond the simple equity/contract dichotomy and build on Williamson’s recent (1991) explication of the key governance instruments underlying the market-hybrid-hierarchy continuum (see Table 1), to consider the possibility of a more fine-grained continuum of hybrid organizational forms. Pursuing these goals soon convinced me that attempts to order the numerous alliance forms on a single continuum were futile because of conceptual as well as practical barriers to valid comparisons of the governance attributes of diverse alliances (see Oxley 1999b for details). Nonetheless the resulting 3-way categorization of alliance forms into unilateral contracts, bilateral contracts, and equity joint ventures, along with the exhortation to limit empirical study to samples of alliances within operational domains (e.g., technology development, retailing, component supply), appears to have been a useful contribution. While the majority of empirical papers on choice of organizational form in alliances continue to focus on the equity/non-equity distinction (e.g., ), a sizeable minority also differentiate between unilateral and bilateral contracts (e.g., ). There is also

¹ Certainly the distinction between alliances and buyer-supplier relationships is not sharp. Gulati (1995, 1998) for example, defines alliances as “any independently initiated interfirm link that involves exchange, sharing or co-development.” Such a broad definition – common in empirical research on alliances – certainly encompasses many buyer-supplier relationships. One useful orienting distinction is the idea that alliances represent collaborations among peers in some sense. Thus, even where an alliance relationship is “vertical,” in that output from the alliance is used as an input by one of the partner firms, the existence of an alliance indicates that each firm brings specialized assets and capabilities to the alliance which, for example, brings the identity of the partners more to the fore in alliance research than is commonly the case in the study of buyer-supplier relationships.
increasing attention paid to ensuring that alliances included in the empirical sample are indeed in comparable operational domains, such that firms can reasonably be thought to be choosing among the observed alliance forms.²

The third way in which Oxley 1997 represented a departure from prior work was to shift attention away from asset specificity as the primary source of contracting hazards and to put the spotlight on appropriability as a key hazard in inter-firm alliances (particularly technology alliances). Certainly appropriability hazards were not a new concept – Teece’s (1986) “Profiting from Innovation” was already very influential – but I argued that the sources and consequences of appropriability hazards were somewhat different in alliances than in questions of competition among independent innovators. So, for example, while in the latter context complex, highly “tacit,” knowhow is associated with strong appropriability (because it reduces the likelihood that competitors will be able to imitate an innovation), I argued that when an alliance encompasses activities involving the transfer of highly tacit knowhow this increases the appropriability hazards within the alliance because transfers of tacit knowledge are more difficult to specify and monitor in a contract, and such contracts are also more difficult to enforce. This idea has been picked up in many subsequent papers (e.g., )

Overall, Oxley (1997) has had measurable impact on alliance research in the management literature where it is routinely cited as a foundational source for TCE work on interfirm alliances [put in something here on citation patterns?]. The theory and empirical approach in the paper is nonetheless increasingly recognized as being incomplete in significant ways as research in the field has developed in the intervening years. Relevant critiques and advances have come from both within and beyond TCE as researchers have sought to refine our understanding of the interplay

² This is particularly important given the endogenous matching issues endemic to alliance research (see below).
among different alliance decisions such as partner selection, operational scope, and governance, and to explore the multi-faceted determinants of alliance performance.


Figure 1 presents a useful device for understanding the interplay among various decisions involved in forming an alliance – and for understanding how the field has progressed since Oxley (1997) and what important puzzles remain. This figure indicates that decisions regarding partner identities (“WHO?”) the content of alliance activities (“WHAT?”) and governance structures (“HOW?”) comprise an interdependent system of decisions: What partners attempt to achieve within an alliance may in part depend on the identities of the firm involved as well as, perhaps, the competitive context in which they interact; similarly, the choice of alliance partner will in part be driven by the goals of the alliance. The governance structure chosen for the alliance may depend on the goals of the alliance as well as on partner identities; conversely, where there are restrictions on the governance structure available to alliance partners (perhaps due to regulatory constraints), this may affect who a firm is willing to partner with and what they can attempt to achieve through their alliance activities.

In line with most research in TCE during the mid-1990s, Oxley (1997) focused almost exclusively on the WHAT-HOW link in the system of alliance decisions. This is the alliance variant on the traditional TCE question of “What is the best generic mode to organize X?” As Williamson (1999) suggests in his response to the “competence critique” of TCE, for TCE to participate and contribute fully to the strategic management field we need to move beyond generic governance to ask, “How should firm A-which has pre-existing strengths and weaknesses (core competences and disabilities)-
organize X?” or even “How should firm A, with its pre-existing strengths and weaknesses, reposition for the future in relation to the strategic situation (actual and potential rivalry; actual and potential market niches) of which it is a part or to which it can relate?” (Williamson, 1999, pp. 1103-04)

As alliance research has moved in the direction proposed by Williamson, we have gained a richer consideration of the endogenous matching processes that have always been recognized but previously remained implicit in TCE research. These issues have also naturally come more to the fore as TCE (and other) researchers focusing on alliances have turned their attention to the “SO WHAT?” question of alliances – what (if any) is the impact of alliance activity on firm performance?

In the following sections I first describe recent advances and open questions within the ‘system of alliance decisions’ and then move on to consideration of the “so what?” question. ³

2.1. WHO (What) and HOW? Partner Identity and Alliance Governance

Lack of attention to partner identity formed the basis for the first – and still one of the most oft-heard – critiques of TCE analysis of alliances. Also coming to the fore in the mid 1990s, another strand of research on alliances built on the work of Granovetter (19XX) emphasizing the embeddedness of inter-firm exchange and the role of trust as an alternative to formal governance. The embeddedness perspective is often positioned in opposition to the “undersocialized” perspective of organizational economics (and of TCE in particular), arguing that the atomistic, calculative approach embodied in economic theories of organization ignores the fact that transactions are embedded in a rich social context (Granovetter 1985). Gulati (1995a) for example, observed that the form of an alliance depends on the collaborative history of the participants: when a pair of firms has collaborated in the recent past, subsequent alliances are less likely to be

³ These sections draw extensively on Oxley & Silverman (2006).
equity based.\textsuperscript{4} He argued that this effect reflected the trust that builds up between alliance partners over the course of repeat alliances. An active research stream on this topic in subsequent years continues to debate the relationship between informal governance (trust, norms of cooperation, etc.) and formal governance [add cites here]. Empirical challenges suggest that it may never be possible to fully disentangle the effects of different governance mechanisms (see below), but the weight of evidence suggests that ... [fill in here]

Interestingly the empirical implications of differences between TCE and the relational view may not be as stark as some claim. Gulati’s (1995a) result, for example, is quite consistent with the idea that firms learn about the behavioral characteristics of their alliance partners over the course of collaboration and (assuming that repeat collaboration is reserved for “trustworthy” partners) thus lowers the need for formal controls in subsequent alliances with repeat partners [cite TCE papers now making the same point?]. In addition, to the extent that multiple alliances connect the same partners at a given point in time, overlapping alliances may also create effective hostages, again reducing the need for formal controls [cite to papers on hostages?].

In a more significant departure from extant TCE research, Gulati (1995b) showed that.......XXXXXXXXXX. An example of other work in this vein is Jones, Hesterly and Borgatti (1997), which attempted a synthesis between TCE and embeddedness, proposing that in some circumstances embeddedness may safeguard against opportunism by diffusing information about reputations and by facilitating collective sanctions. These authors also point to ways that insights from organizational economics can help explain how network positions arise and change, echoing the pioneering work of Grief (1993), who has argued both that social relations and other

\textsuperscript{4} Note that this is still a controversial observation: Oxley (1997) and Sampson (2004) found no relationship between prior collaboration and the propensity to adopt an equity-based alliance structure. [Add cites to papers where effect is confirmed]
mechanisms can help to overcome opportunism-related hazards that otherwise would have prevented a wide range of exchanges, and that economic actors will seek to create or influence such mechanisms in the pursuit of their own self interest. Thus, whether we accept that the relational view of alliance governance constitutes an entirely distinct rationale for collaboration or not, Gulati and others have most definitely brought partner identity – and firms’ position in the broader industry network – squarely into the picture.

The most recent work in this stream is perhaps also the most convincing: In an analysis of the network of alliances assembled in the biotechnology industry Robinson and Stuart (2005) demonstrate that the prior network of relationships among biotechnology firms serves as a substitute for hierarchical arrangements in newly founded alliances. Specifically, they find that as partners’ proximity and centrality within the network increases, equity-based governance decreases, even as the dollar-value of the alliance increases. This study thus provides very plausible evidence that network embeddedness can go some way toward mitigating the hold-up problem that is traditionally associated with more hierarchical governance in the TCE framework.

In the 10+ years since the publication of Oxley (1997) TCE researchers have explored a variety of other aspects of partner identity on alliance governance. Oxley & Sampson (2004), for example, explored the impact of competitive context and argued that when alliance partners are direct competitors the hazards of cooperation are increased and, as such, even “protective” governance structures at the hierarchy end of the market-hierarchy continuum of alliances may be inadequate to support cooperation. In their empirical study of R&D alliances in the microelectronics and telecommunication equipment industries, they showed that when a firm allies with a competitor there is a tendency to reduce the scope of activities in the alliance – limiting activities to “pure” R&D, rather than extending the alliance activities to cover manufacturing and/or marketing
– and that this in turn impacts the alliance governance structure chosen. This study thus effectively engages two links in the WHO-WHAT-HOW system: here, partner identity simultaneously affects the scope of alliance activities as well as the governance structure – and scope and governance are also inter-related, impacting each other in a reciprocal manner.

As the resource-based view of the firm (RBV) gained ascendency in the strategy field in the late 1990s alliance researchers not surprisingly turned their attention to the impact of firm capabilities on alliance management and performance. Building on Oxley (1997), for example, Sampson (2004) argues that the degree of overlap between partner firms’ knowledge bases (technological capabilities) will have a predictable impact on governance costs in an alliance. Using a sample of R&D alliances, Sampson finds evidence of an inverted U-shaped relationship between technological overlap and the use of equity as a governance structure in alliances, and interprets this as supportive evidence for the TCE / appropriability logic. Interestingly, Columbo (2004/3?), in a similar study published around the same time, finds instead [add Columbo results here]... Thus the question of how partner selection, capabilities and governance are co-determined remains open, and represents an interesting direction for continued future research.

2.2. (Who, What) & HOW? Beyond discrete structural alternatives to contractual terms

Another salient critique of Oxley (1997) – and an exciting area of current research relates to the simplified – one may even say simplistic – characterization of alliance governance. Certainly for practitioners the idea that governance can be boiled down to the choice among unilateral or bilateral contracts or the establishment of an equity joint venture must seem far-fetched. Thus the aim of one current area of alliance research is to increase our understanding of “HOW” alliances are (or, ultimately, should be) structured. As the discussion so far suggests, prior alliance research has
tended to focus on the choice between two or three discrete governance structures. This in part reflects the underlying logic of TCE, whereby markets, hybrids and hierarchies are viewed as discrete governance alternatives that are supported by “syndromes” of governance instruments (Table 1); it also reflects difficulties in obtaining information on the actual provisions of alliance contracts, as well as a lack of nuanced theory to guide our understanding of the role and consequence of individual contract provisions. As such, fully explicating the role and impact of alliance contracts is a challenging area of research.

In one of the earliest examinations of alliance contract provisions, Parkhe (1993) searched the legal literature and identified a variety of provisions that were commonly (but by no means universally) adopted in alliance contracts. He then used this information to create measures of contractual complexity for a sample of alliances (based on a count of provisions employed, weighted by the level of stringency). This approach – or variants thereon – has subsequently been adopted in several studies seeking to understand the relationship between formal and informal alliance governance, as discussed above (e.g., Kale, Singh & Perlmutter, 2000; Poppo and Zenger, 2002.)

A more recent survey of alliances in the German telecommunications industry by Reuer, Arino and Mellewigt (2005) using Parkhe’s (1993) classification of contract provisions, highlights both the significant heterogeneity in the use of various provisions across alliance agreements, and a lack of systematic differences between equity and non-equity alliances in the use of specific provisions (with the exception of the right to reports of relevant transactions and auditing rights). Notably, these authors find that factors such as the “strategic importance” of an alliance to a partner affected the complexity of the contract that formalizes the alliance, but did not affect the choice of governance mode for the alliance; in contrast, the asset-specificity inherent in the alliance
affected governance choice but did not affect contractual complexity. These findings suggest that we have much to learn about how specific contract structures “map” onto the discrete structural alternatives that have been the focus of previous alliance research, and how the use of different contract provisions is shaped by characteristics of the underlying alliance activities, partner resources and capabilities, and relational history.

Although work in this arena is still in its infancy, one early empirical study speaks to the promise of the approach: In a detailed examination of a small sample of 42 alliance contracts extracted from SEC filings, Ryall and Sampson (2003) again highlight the observed heterogeneity in the level and detail of contract specification and in the specific provisions included. They further show that – at least in this sample of contracts – as firms gain greater alliance experience (whether or not with the current partner) they tend to write more “complete” contracts, particularly in terms of development specifications or timeframe, but when alliance partners have other concurrent alliances together, contracts tend to be less complete. This evidence is consistent with research on supply contracts suggesting that firms tend to adopt more complex contracts over time as they learn about new contingencies that can usefully be specified in the agreement (Mayer & Argyres, 2004), as well as research on the hostage value of overlapping agreements (Gulati, 1995a). The authors of the study nonetheless emphasize that the substantial variation in the contract terms employed (illustrated effectively in detailed case studies of three alliance contracts) make direct cross-case comparisons difficult. This once again highlights the need for additional theory-building by both economist and lawyers, to better understand how individual contract provisions coalesce in alliance governance structures.

Another promising approach to this set of issues is to focus on cases where contractual terms are renegotiated. To the extent that renegotiation of specific terms can be tied to changes in
the alliance activities or environmental context of the alliance, this approach may shed further light on the factors that lead to the adoption of specific contractual provisions. Reuer and Arino (2002) offer a first step in this direction with their study of renegotiation, at a highly aggregate level, within Spanish firms’ collaborative agreements. They find evidence that renegotiation is positively associated with initial “misalignment” in alliance governance, with asset-specificity (if asset-specificity is low, partners are likely to exit the agreement rather than spend time renegotiating it), and with changes in partners’ strategies. In this study, it is not possible to tell whether renegotiation represents a jointly beneficial move towards greater efficiency or whether it represents an opportunistic demand by one of the partners (in fact, another finding is that renegotiation occurs less frequently when the contract includes specific deterrents to renegotiation, which may suggest that renegotiation represents opportunistic behavior). Future work in this line can further inform these questions by explicitly connecting renegotiation to (actual or perceived) performance effects, and to unpacking more disaggregated detail about which types of provisions are renegotiated in the presence of which triggering factors.

2.3. SO WHAT? Alliances and Performance

For strategy researchers in 2008, the most salient criticism of Oxley (1997) has to be the “so what?” question: For alliance governance choice to be relevant to firm strategy we need to demonstrate not only that managers choose alliance governance structures in a “discriminating manner” but also that those choices have performance implications. Oxley 1997 is silent on the performance issue, relying instead on the idea that populations of firms (or alliances in this case) will move towards more efficient organizational forms, either because individual firms will learn through trial and error what is the least-costly governance structure for a particular alliance, or because less-efficient structures will lose out in competition with more-efficient structures. This
weak-form selection assumption was common to almost all empirical research in TCE through the late 1990s [cite for this?]

Studying performance effects of alliances and alliance governance is difficult for several reasons. First and foremost, it is often difficult to measure alliance outcomes. Unlike public companies, alliances do not regularly publish financial information. Further, although the dissolution of a firm can be interpreted as a negative performance outcome, alliance dissolution can reflect positive as well as negative outcomes. For example, an alliance may be dissolved because it fails to meet partners’ expectations – or because it has successfully accomplished all of the tasks its partners desired. Consequently, scholars are justifiably suspicious of using alliance survival as an outcome measure. In sum, none of the most commonly used measures of firm-level performance is easily applicable to alliance research.

Recent research has addressed this challenge through three avenues: alliance case studies and surveys of alliance participants; event-studies of alliance announcements; and studies on narrower, non-financial measures of performance that are intuitively connected to alliance activities (e.g., changes in a firm’s patenting behavior or innovative search patterns).

Case studies and survey research offer the potential benefit of measuring outcomes – or at least the perceived outcomes – specific to an alliance. Although some have questioned the reliability of managerial assessments of alliance performance, recent studies that combine both subjective and objective measures of performance suggest that survey responses may indeed be accurate (Geringer and Hebert 1991; Kale et al. 2002).

Poppo and Zenger,
Some scholars have applied event-study methods to measure the *anticipated* performance of alliances. In an event study, a researcher identifies the precise date on which each alliance in her sample is announced, and then explores whether the alliance partners experienced “abnormal” stock market returns upon the announcement of the alliances. The event-study methodology rests on the assumption that the stock market is efficient – that is, the stock market responds immediately to any new news about a firm so that the firm’s stock price reflects all existing information. Given this assumption, any abnormal change to the share price of a firm on the day of (or multi-day window around) an alliance announcement can be interpreted as the market’s expectation of the performance impact of the announced alliance.

Most event studies have found small but significantly positive stock market reactions to the announcement of a new joint venture (e.g., Koh and Venkatraman, 1991; Anand and Khanna, 2000; but see McGahan and Villalonga 2005 for negative findings). Factors found to significantly increase the magnitude of the imputed value of the venture include greater alliance experience (Anand & Khanna, 2000), relatedness of the joint venture partners and higher overlap between the joint venture activities and the focal firm’s existing product or geographic markets (Koh & Venkatraman, 1991), and the presence of a dedicated alliance function within the firm (Kale, Dyer & Singh, 2002).

There are no event studies to date that explicitly consider the effect of appropriate governance on stock market reaction. This reflects significant challenges in implementing such a study: beyond the usual problems associated with identifying an appropriate event date and eliminating the effect of potential “confounding events” that may produce a spurious stock market reaction unrelated to the alliance announcement itself, one must deal with the issue of how to
measure “appropriate” governance (or lack thereof). This represents a significant challenge in itself (see discussion of endogeneity, below) which has, at least to this point, defied implementation in the event study context, but which represents a potential avenue for ambitious future research.

A third method for studying alliance performance has been to focus on a specific, non-financial set of outcomes that are plausibly related to the goals of an alliance. For example, a large proportion of interfirm alliances are technology-based R&D alliances. Such alliances are most likely to influence a firm in terms of research productivity or, even more specifically, the accessing and acquisition of its partner’s technological capabilities. Given this, several scholars have explored the effect of a firm’s alliances on the evolution of its technological knowledge base (Mowery et al 1996, 1998, 2002; Sampson 2004, 2006; Oxley and Wada 2005).

Mowery et al. (1996) study changes in pre-alliance vs. post-alliance technological overlap across a sample of allying firms, as captured by changes in patent citation patterns. They interpret an increase in technological overlap as a measure of successful knowledge transfer. They find that, for learning alliances, increases in technological overlap are positively associated with absorptive capacity between partners, and that the use of equity-based governance is associated with higher levels of knowledge transfer.

Oxley & Wada (2005) push the idea of governance structures shaping the knowledge outcomes of alliances one step further. In a study of patent in-licensing by Japanese firms, they show that – in line with prior research – bilateral agreements lead to greater knowledge transfer to the Japanese firm (as measured by increased patent citation to the licensor’s patents). However, when the licensing activity takes place within the context of an equity joint venture linking the licensor and licensee, they show that this increases knowledge transfer in areas closely related to
alliance activities (as indicated by citations to patents in the same technology class as the licensed patent) but that knowledge transfers in unrelated areas are actually reduced, relative to that observed in a “bare” license. They suggest that the restricted scope of knowledge flows in the equity joint venture may reflect the enhanced control features of this governance structure and argue that the ability of the licensor to prevent unintended leakage of knowledge unrelated to alliance activities is an important underpinning of the enhanced alliance-relevant knowledge flows in joint ventures observed in prior research.

2.3.1. Endogeneity (It’s everywhere…)

As noted above, alliance research faces a distinctive challenge in the measurement of performance outcomes. Even when this problem is solved, however, studying the performance effects of alliances and their governance is quite difficult for a more general reason: concern about endogeneity [discussed elsewhere in the volume?]

Since economic actors are presumed to behave as boundedly rational profit maximizers, it follows that each firm makes optimal governance decisions, conditional on its own idiosyncrasies. Given this presumption, what should a researcher conclude when she sees that a) firms whose alliances have similar attributes vary in their alliance governance decisions and b) firms whose alliance governance choices vary from those prescribed by TCE also have lower performance? Absent other information, she must conclude that for those firms whose alliances are “misaligned,” being misaligned is optimal due to some unobserved firm or transaction characteristics (which may also affect performance). Anything else would be hubris.

Most extant research on alliances has not fully addressed this issue. Consider, for example, three commonly-cited results regarding capabilities and alliance decisions: (i) Firms are more likely
to choose alliance partners with overlapping capabilities, up to a point (Mowery, Oxley & Silverman, 1998); (2) Firms with overlapping capabilities are, up to a point, more likely to choose equity structures (Sampson, 2002); (iii) Alliances where partners have greater overlapping capabilities have greater knowledge-flows, as do equity-based alliances (Mowery, Oxley & Silverman, 1996).

Back in 1996, we interpreted the Mowery, Oxley & Silverman (1996) as evidence that the presence of an equity stake facilitated the flow of knowledge across firms. However, when recognizing that the choice to include an equity stake is an endogenous decision of the partners, this interpretation becomes less clear: perhaps the appropriate interpretation is that those alliances that are undertaken with aspirations to transfer a lot of knowledge are also ones in which partners have high overlapping capabilities and in which firms choose to include an equity stake.

Fortunately, Sampson (2006) provides confirmation of the link between equity governance and enhanced knowledge flows in a study that directly addresses the endogeneity issue. Analyzing a sample of R&D alliances she employs a two-stage method and demonstrates that alliance outcomes vary systematically with governance form even after correcting for governance selection. More specifically, she shows that when the technological diversity of alliance partners is high - a situation that poses the most challenges for effective knowledge sharing - partners’ post-alliance patenting (an indication of innovative output) is significantly higher in equity joint ventures than in non-equity alliances – up to 100 times higher, depending on the level of technological diversity.

These recent developments notwithstanding, future alliance research would benefit more generally from careful consideration of unobserved heterogeneity. Effort devoted to surmounting these issues is well spent; investigating how firm strategy and alliance organization interact to
affect firm performance continues to present one of the most exciting frontiers of research in TCE today.
### Table 1: Alliances as Hybrid Governance Structures

<table>
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<tr>
<th>Instruments</th>
<th>Market</th>
<th>Hybrid</th>
<th>Hierarchy</th>
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<tbody>
<tr>
<td>Incentive Intensity</td>
<td>++</td>
<td>+</td>
<td>0</td>
</tr>
<tr>
<td>Administrative Controls</td>
<td>0</td>
<td>+</td>
<td>++</td>
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<tr>
<td>Contract Law</td>
<td>++</td>
<td>+</td>
<td>0</td>
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<tr>
<th>Performance Attributes</th>
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<tbody>
<tr>
<td>Autonomous Adaptability</td>
<td>++</td>
<td>+</td>
<td>0</td>
</tr>
<tr>
<td>Bilateral Adaptability</td>
<td>0</td>
<td>+</td>
<td>++</td>
</tr>
</tbody>
</table>

++ = strong effect  
+  = semi-strong effect 
0   = weak effect

Source: Adapted from Williamson (1991)
Figure 1: The “Who What and How” of Alliances

**WHO?**

**WHAT?**

**HOW?**

Partner identities

Alliance activities

Governance