Dear Professor Felix Arndt,

Thank you for the opportunity to revise the manuscript. We appreciate the support from you and both reviewers. We believe that the manuscript has benefited greatly from these comments, and we thank you and the reviewers for your diligence and insights.

Following are your comments on our manuscript, followed by our responses.

*The reviewers have highlighted in their comments what they consider to be both strengths and weaknesses of your paper. They see potential in the paper, but have identified a range of issues that need to be addressed. I believe that at least the majority of the issues can be addressed through a revision of the paper and I am therefore inviting you to revise and resubmit this paper after having made changes in-line with those suggested by the reviewers. Please note that this invitation to resubmit a revised version of the paper does not constitute an offer to publish your paper. Rather, the paper will be reviewed again and a decision will be made at that particular point.*

Thank you very much for a very clear and insightful set of suggestions. Regarding changes, you will notice that we have made important alterations to the paper. These include the following:

1. Per Reviewer 1’s concerns about empirical robustness, we have elaborated on why our chosen dataset and sampling scheme are appropriate for a statistical inference of population properties. In regards to the reviewer’s concerns about potential sample selection bias, we performed additional statistical analyses, which showed that our results are not affected by sample selection bias. Specifically, we have fine-tuned our analysis by controlling for potential sample selection bias through Heckman’s (1979) two-step procedure.
2. We provided better explanations and justifications for our setting of empirical analyses, including industry selection criteria and sample selection processes.

1. We freshened up the references we cite in the theory section by drawing on the recent literature on organizational governance, resource combinations, and inter-firm networks.
2. In response to Reviewer 2’s concerns, we clarified some terms and definitions that were not clear enough in the previous manuscript. Furthermore, per Reviewer 2’s kind suggestions concerning editorial issues, we accepted all of the suggestions and made proper changes to the entire manuscript.

We would like to acknowledge once again our gratitude to both the editor and the reviewers for their hard work in helping us unlock the potential of the manuscript. As we compare the first version of the paper with this revision, we cannot help but remark on how much it has benefited from the review process. Thank you.

In what follows, we provide a detailed set of responses to each of the comments of the reviewers.

Warm regards,

Sungho and Kyuho

**Responses to Reviewer 1**

*First, I note some positives. The paper is eloquently written and the modelling shows flair and sophistication. I have some suggestions for improvement.*

Thank you for the encouraging remarks as well as for your valuable suggestions. As you will notice, in the new version of the paper we have tried very hard to respond to each of your suggestions. We had some fresh eyes look at the paper. Your feedback was very helpful in letting us sharpen the contribution of this research.

*Freshen up the literature review – on p. 2 you write of ‘recent studies’ and then cite a 1993 and 2007 article. The majority of citations in the paper are more than 10 years old – this is a bad look and makes this research article look out of date.*

Thank you for your suggestion. We updated our literature review by including some of the most recent studies on organizational governance, resource combinations, and inter-firm networks. Additional references are as follows:

* Strategic alliances allow firms to augment and recombine internal and external resources and capabilities (Carayannopoulos & Auster, 2010; Rice, Liao, Martin, & Galvin, 2012).
* Geographical cluster co-location as a determinant of governance choice (McCann, Reuer, & Lahiri, 2015).
* Additional citation on dyadic approach to examining governance choice (Yang, Lin, & Lin, 2010).
* Alliances and M&A present differential constraints and opportunities for future corporate growth (Lockett, Wiklund, Davidsson, & Girma, 2011).
* Alliances and M&A offer differential capacities to exploit resource complementarity (Wiklund & Shepherd, 2009).
* Additional citation on the governance choice between alliance and M&A (Reuer & Ragozzino, 2011).
* Alliances as a vehicle for assimilating technological resources and implementing breakthrough innovation (Srivastava & Gnyawali, 2011).

You can find a bibliography at the end of this correspondence.

*I acknowledge that SDC Platinum is widely used in the alliance literature, however I am concerned that issues associated with the dataset may have introduced a systemic bias into the paper. As you are looking only at listed firms, SDC will have picked up M&A announcements that would have been reported to firms’ respective listing stock exchanges. However, many firms are more reluctant to report non M&A alliance arrangements. Furthermore, some firms do announce alliances on their websites, but if these announcements are not picked up by the media that SDC scan, the announcements do not appear in the SDC Platinum dataset. I don’t see this as a fatal flaw – however I think the authors need to be completely up front about the manner in which the SDC Platinum dataset is created, acknowledge that this may have created issues of bias and state what steps have been taken to mitigate these issues.*

Thank you for your insightful comment. This is a significant issue to consider, and we recognize your concerns. We agree with you that the SDC database could possibly introduce a systematic bias if the observations are not randomly drawn —i.e., a sort of sample selection bias. However, we believe that sample selection bias is less of a concern for several reasons. On the one hand, SDC collects information on M&A from an extensive set of data sources including more than 200 English and foreign language news sources, SEC filings and their international counterparts, trade publications, wires, and proprietary surveys of investment banks, law firms, and other advisors. Furthermore, listed firms are generally required to disclose information on their M&A deals and the specific terms thereof by filing reports (e.g., 8K, 10K, and Proxy) to the US Securities and Exchange Commission (SEC). Considering that SEC filings are one of the many SDC sources, we believe that the M&A data from the SDC database is less prone to sample selection bias.

On the other hand, we concur with you that not all alliances are covered in the SDC database. Fortunately, however, Schilling (2009) showed that the potentially partial coverage of the alliance data of the SDC database does not yield any systematic bias. She compared various databases of alliances such as SDC, MERIT-CATI, and CORE and examined the pattern of alliance activity in several dimensions. What she found is that even if their samples do not seem to overlap much, they generate essentially similar empirical patterns. More importantly, through replications of three prior studies, she confirmed that statistical results remain consistent across the databases, alleviating the concern about sample selection bias. Furthermore, in support of this view, literature on the M&A and alliance has consistently used the SDC dataset (e.g., Erel, Liao, & Weisbach, 2012; Haleblian, McNamara, Kolev, & Dykes, 2012; Hayward & Hambrick, 1997; McCann, Reuer, & Lahiri, forthcoming; Yang, Lin, & Peng, 2011), lending credence to it as a source of academic inquiries. (In particular, we would like to note that the sampling procedure adopted by one of the recent articles is exactly the same as ours: McCann, Reuer, & Lahiri, forthcoming.) Taken together, we believe that the SDC database suffers less from the non-random sampling.

Following your suggestion, we have added a section on pages 13-14 which details how the SDC dataset is created and elaborates on why there is less concern about sample selection bias.

*Your data screening is interesting, but needs better justification and explanation. Why only the computer industry?*

We agree that we were inarticulate in making clear the rationale behind our choice of the computer industry. There were several reasons for this choice. First, the computer industry is characterized by fast-paced environmental changes and a strong pressure for innovation. Thus in order to survive, firms in the industry are compelled to collaborate with others to combine resources in a synergistic manner. Not surprisingly, they routinely use alliances and M&A as important strategic vehicles to achieve this goal (Rosenkopf & Schilling, 2007; Yang et al., 2011), making this industry an appealing setting for the test of our resource combination argument. Second and equally importantly, this extensive use of alliances and M&A results in well-connected industry networks, making this industry a more appropriate setting for the test of our theory which builds on information circulation within a structurally embedded region. And in our view, this is why many studies drawing from network theory have chosen the computer industry (e.g., Fleming & Marx, 2006; Schilling & Phelps, 2007; Yang, Lin, & Lin, 2010). Finally, sampling from one industry is more suited to meeting the independent and identical distribution (i.i.d.) condition and, in connection with this, is less susceptible to unobserved industry heterogeneity; simply adding industry dummies would not perfectly solve the problem. In our revised manuscript, we have added these points on page 14.

*Were there other ways of managing missing data than simply excluding all firms missing any data on control variables? This looks lazy.*

Thanks for pushing us on this issue. We agree that this concern needs to be addressed as this sampling scheme could introduce sample selection bias of another kind if the excluded firms are systematically different from the remaining ones. To address this concern, we re-conducted our empirical analysis by employing the two-step procedure proposed by Heckman (1979). Given that all the firms in our sample have sales data even when they have missing values for some other variables (most of the missing values are found in ROA), we estimated the first-stage probit model, which predicts the probability of at least one firm in the dyad having at least one missing value. For this estimation, we used the sales difference between the two firms and year dummies as covariates. The choice of these predictors makes sense, given that the probability increases when a firm does not generate enough sales and confronts unfavorable macroeconomic conditions. After confirming that each first-stage model achieved a sufficient model fit, we calculated the inverse Mills ratio from the results. Under some benign assumptions, this inverse Mills ratio is correlated with the unobserved portion from the selection hazard in the residuals in the second-stage model. We re-estimated all our models after inserting this inverse Mills ratio into all of them as a control variable. Even after controlling for this sample selection bias, we found the statistical pattern remained essentially consistent. In our revised manuscript, we have detailed this procedure in the estimation technique section on page 21 and updated the results section and the tables accordingly.

*You are assuming homogeneity within 4 digit SICs relating to resource similarity. Really? This needs plenty of justification.*

Thank you for raising this issue. Admittedly, our measures based on the Standard Industrial Classification (SIC) code system are inherently unable to take into account heterogeneity within four-digit industry. Indeed, this is one of the limitations confronting all studies that use the SIC code system for the operationalization of resource similarity and relatedness. Nonetheless, the SIC code system has enjoyed wide currency and formed the basis for measuring resource relatedness in a large number of studies, providing an indirect validation of its use (e.g., Lin, Yang, & Arya, 2009; Robins & Wiersema, 1995; Villalonga & Mcgahan, 2005; Wang & Zajac, 2007). Furthermore, our measures originated in Fan and Lang (2000) and Robins and Wiersema (1995) take one step further by relaxing the unrealistic assumption of homogeneity (or equidistance) across SIC levels (see Robins & Wiersema, 1995 for a review) by using CONCOR, one of algorithms for measuring structural equivalence. This algorithm, which was originally developed in the social network literature, measures the correlation among industries by using their input and output patterns. When compared with the measures found in other studies, which are often quite arbitrary in determining the distance across SIC levels (e.g., Lin, Yang, & Arya, 2009; Wang & Zajac, 2007), the virtue of our measures stands out as one kind of Mahalanobis distance addressing the limitation of the Euclidean distance. All in all, we admit that our measures may not be an ideal solution, but we believe that they are practically the best choice and at the same time advance the existing literature that adopts the SIC-based measurement. We hope you agree.

*I think UCINET can provide a fairly rich and continuous measure of structural embeddedness and adjacency – why dichotomise?*

Thank you for pointing this out. Structural embeddedness is basically about the influence of network structure on an actor’s behavior (Granovetter, 1985, 1992). In other words, it does not concern relational contents such as tie strength but the way one actor is structurally related to other actors and how this shapes and constrains the focal actor’s action. (Thus, to our knowledge, structural embeddedness is conceptually orthogonal to adjacency.) Granted, not a few measures have been proposed for capturing this construct. However, it is notable that our theory and analysis are at the *dyad* level. In essence, such ego network measures as ego network density (e.g., Rowley, Behrens, & Krackhardt, 2000) and structural holes (e.g., Burt, 1992) cannot be easily translated to the dyadic measure of structural embeddedness.

To illustrate this, suppose that two companies have high ego network density and that they have neither direct nor indirect relations. Are the two companies structurally embedded? The answer is clearly no. So to measure structural embeddedness at the dyad level, an analysis of subgroups within which actors are somehow structurally related is called for. (This is why we focused on subgroups or network clusters in our manuscript.) One measure for this is the number of common third parties (Gulati & Gargiulo, 1999). Unfortunately, however, the number of common third parties considers only up to triads and is therefore less reflective of networks beyond two-step distant ties; in other words, it is too local-network oriented. Given that our theory is not confined to information circulation within local networks, it will be more pertinent to consider regions beyond triads which can be identified through a subgroups analysis such as cliques, clans, and the like (e.g., Baum, Shipilov, & Rowley, 2003; Rowley, Baum, Shipilov, Greve, & Rao, 2004). Yet cliques are too restrictive and sensitive to changes in node degrees to offer an accurate measure of structural embeddedness (Wasserman & Faust, 1994). While some studies use n-Clan (e.g., Rowley, Greve, Rao, Baum, & Shipilov, 2005), the choice of n is unavoidably arbitrary, as detailed in our manuscript (pp. 17-18). Because the subgroup analysis we chose, the Markov clustering algorithm (MCA), is not prone to such problems, we believe that our chosen method is more appropriate.

When a subgroup analysis is used, measuring structural embeddedness between two firms in the focal dyad amounts to simply examining “whether the firms are located in the same subgroups” (e.g., Rowley, Baum, Shipilov, Greve, & Rao, 2004). (This is why we use the term *network co-location* when we operationalize structural embeddedness.) If subgroups are overlapping as in the cliques and clans, the measure of structural embeddedness will be a count variable. However, because subgroups or network clusters identified by MCA are non-overlapping, our measure of structural embeddedness automatically becomes a dichotomous variable like membership. We also note here that because our measure is not derived from an underlying continuous measure, such information loss as is observed in dichotomization of continuous variables does not occur.

We admit that any operationalization has pros and cons; MCA has its own limitations. Even so, we believe that MCA is more suitable to our purposes than the others. We hope you agree on this.

*I suspect that this paper has a long history, and I think I can see some shortcuts that have been taken that should have been avoided. It is a well written piece, but there is some evident room for improvement.*

Thank you again for all of your help and support throughout this process. We believe it has made our manuscript much stronger.

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**Responses to Reviewer 2**

*I enjoyed reading your manuscript. It is well written and the author's arguments leading to the three hypotheses are clearly laid out. I believe the authors address a topic that has not been the object of previous research, that is the connection between governance, resources within organizations, and interorganizational relationships, particularly alliances and mergers and acquisitions.*

*For the purposes of my review, please note that I use the page # generated by ManuscriptCentral/ ScholarOne (MS/SO). As such, page 1 is the page where the title, abstract, and keywords are reported. Page 2 is the page that includes the title and the introduction of the manuscript. Continuing along this pattern, page 25 is where the list of references start, Table 1 is on page 29, Table 2 is on page 30, and Table 3 is on page 31. With respect to line #s, I have used the MC/SO generated line #s in the left margin of the manuscript.*

Thank you very much for your encouraging comments, as well as for highlighting some areas for improvement. We are in agreement regarding the issues you kindly pointed out. We have provided greater clarity in some of the passages to which you drew our attention.

*Revisions: Please review APA guidelines for authors, particularly in reporting references in text (should be done in alphabetical order (with surname of first author). For a few examples (where this should be corrected), see page 2, lines 22-25; page 2, lines 32-34; page 3, lines 9-11.  
  
As well, when direct quotations are used, the page # reported should include p. (e.g., Smith, 2015, p. 1). For a few examples (where this should be corrected), see page 3, line 39; page 9, lines 53.*

Thank you. Following APA guidelines, we made corrections to the incorrect in-text citations and bibliography.

*Abstract, please provide references in abstract to support your claims ("Prior studies on the governance choice ..."). [If the decision not to include references is based on space limitations/maximum # of words used, please disregard.]*

Thank you very much for your suggestion. However, we were not able to provide references in abstract due to the 150-words limitation. The current abstract without references consists of 149 words.

*The purpose outlined on page 4 uses the word "emphasize" ["The purpose of the study is to emphasize ..."]. Did the author mean to use the word 'examine' or 'investigate' instead? The element of 'emphasizing' does not match the purpose statement reported on page 1 (abstract) and implies that the author has already established an impact of structural embeddedness and vertical relatedness on inter-firm resource sharing and choices of governance approaches.*

We agree with your comment. We have changed the term “emphasize” to “examine” on page 4, line 13 of the revised manuscript.

*The author sometimes uses alliance and M&A and sometimes uses alliances and M&A (i.e., singular or plural for the word 'alliance'. As examples of this issue, please see page 2, line 20; page 3, line 51; page 4, line 29; page 4, line 37. I believe the plural should be used. Please review the entire manuscript and make appropriate changes where needed.*

Plural (alliances) is more appropriate, as you kindly pointed out. We have made corresponding changes to the entire manuscript.

*page 3, line 39, the author should provide a brief interpretation/definition of the term "dyadic atomization".*

We apologize for the lack of clarity about the term “dyadic atomization.” In the first place, after reviewing the sentence again per your comment, we acknowledge that the transition word “Furthermore” is not proper. The term “dyadic atomization” in fact summarizes the argument presented in the paragraphs preceding this sentence (p.2, line 46 to p.3, line 34 of the previous manuscript; p.2, line 46 to p.3, line 32 of the revised manuscript). “Dyadic atomization” refers to the treatment of a dyad in a network without consideration of the fact that any dyad is a micro-structure embedded in a network. In this sense, “atomization” means isolation. With this term, we intended to capture the fact that the effects of dyadic attributes on governance choice had been studied in isolation from the effects of networks on dyads.

In the revised manuscript, we changed the sentence “Furthermore, studies taking the dyad as a unit of analysis are often exposed to the problem of ‘dyadic atomization’ (Granovetter, 1992, p. 33).” to “An important limitation of previous studies taking the dyad-level approach (e.g., Wang & Zajac, 2007) is their failure to consider such impacts of networks on the dyad, a problem referred to as “dyadic atomization” (Granovetter, 1992, p. 33).” The modified sentence is on page 3, line 32 of the revised manuscript. We also made a change on page 24, line 22 of the previous manuscript, altering “In other words, there has been a concern about ‘dyadic atomization’ (Granovetter, 1992).” to “In other words, there has been a concern about “dyadic atomization” (Granovetter, 1992), i.e., neglect of the implications of the fact that any dyad is a micro structure embedded in a network." This revised sentence is at page 24, line 22 of the revised manuscript.

*page 3, line 56, add hyphen to "... vertically-related businesses ..." This comment applies as well to page 10, line 20 and page 10, line 46 [because "vertically-related" precedes a noun in those three instances].*

Thank you. We made changes per your suggestion. The changes are made to the followings of the revised manuscript: page 3, line 56; page 10, line 15; page 10, line 25; page 10, line 37; and page 11, line 6.

*page 4, line 27, the author should reword the beginning of the sentence: "The study begins with a discussion of background ..." [because the study cannot discuss (people discuss)!]. Same comment applies to page 4, line 32, "The study then develops ..." Please change to "Theories and hypotheses are then developed ..."*

Thank you for kindly pointing those out. We accepted all of your suggestions and made changes accordingly. The changes were made on page 4, line 32 of the revised manuscript.

*Related to this point, the author may also want to adjust the sentence on page 4, line 37, to avoid the use of "we" (twice, when 'we' has been avoided in previous sentences). "We outline how we test ..."*

Thank you for your suggestion. We changed “We outline how we test . . .” to “We outline how to test the developed hypotheses . . .” on page 4, line 37 of the revised manuscript.

*Page 5, line 25, can the author explain the difference between "vertical integration" and "vertical contracting"?*

“Vertical contracting” refers to a contractual relationship or other type of non-equity strategic alliance formed between suppliers and buyers, as opposed to hierarchical control through vertical integration or vertical financial ownership (Mahoney, 1992). We now explain the difference in a footnote in the revised manuscript, on page 5. Thank you for giving us the opportunity to clarify these terms.

*page 4, lines 32-34, the author writes "alliances and M&A in particular, involve inter-firm resource combinations." This part of the sentence appears to suggest that governance is simply about the combination of resources - when in fact, the literature on governance addresses larger elements of decision making and power. What role do these two elements play in this study?*

Thank you for your insightful comment. Indeed, governance affects not only resource combinations but also in large part decision making and power. Alliances and M&A differentially affect decision making and power *ex post*. Precisely because M&A is a more hierarchical governance than alliances are, M&A grants more power to the managers of a controlling firm after a merger or acquisition. Alliances provide the involved parties with access to the pooled resource stocks without full ownership. By contrast, M&A offers the acquiring firm not only access to, but controlling rights over the combined resource stocks.

As the title of this paper suggests, the term “governance” in our study refers to *organizational governance*, which is a separate area of study in the field of management from *corporate governance*. One of the key issues that the organizational governance literature addresses is choice of governance form (e.g., hierarchy or alliance) for organizing economic transactions. The references cited regarding governance in our study come from the organizational governance literature (e.g., Argyres & Zenger, 2012; Barney, 1999; Hennart & Reddy, 1997; Leiblein, 2003; Lockett et al., 2011; Mahoney, 1992; Nickerson & Silverman, 2003; Wang & Zajac, 2007; Williamson, 1991; Zajac & Olsen, 1993).

Among the many kinds of decisions firms would make, this study focuses on the decisions firms make between alliances and M&A to govern their resource combinations given the effects of network-channeling information and resources. We have included the above argument in the discussion section of the revised manuscript (page 26, line 40 to page 27, line 8).

*page 6, line 34, why identify Ronald S. in the references here? Why not Burt, 2009?*

Thank you for kindly pointing it out. We fixed the typo.

*page 7, line 27, "... the whole networks ..." Why is network plural?*

Thank you for your comment. Singular is more appropriate here because there is only one *whole* network. We changed “the whole networks” to “the whole network.” The modified sentence is on page 7, line 41 of the revised manuscript.

*page 8, line 3, can the author identify these "... three kinds of network embeddedness ..."*

Thank you for your comment. The three kinds of network embeddedness are identified as relational, structural, and positional embeddedness (Gulati & Gargiulo, 1999). Per this, we made a change to page 8, line 3 of the previous manuscript, as follows: “In particular, among three kinds of network embeddedness (Gulati & Gargiulo 1999) that have distinct mechanisms for affecting governance choice—relational, structural, and positional embeddedness—structural embeddedness has been largely neglected.”

*page 8, line 13, add hyphen "Resource-based view ..." for consistency.*

Thank you for kindly pointing this out. We fixed the typo. Throughout the revised manuscript, the term “resource-based view” is consistently used in place of “resource based view.”

*page 12, line 3, the author should consider adding details to the beginning of the sentence. I am suggesting "Between these two categories of network embeddedness lies structural ..." [Since it is a new paragraph and the reader may not know to what refers "these two".]*

Thank you for your suggestion. We accepted it and have made a corresponding change to page 12, line 18 of the revised manuscript (p. 12, line 3 of the previous manuscript).

*page 13, under the section Sample, are there other reasons why the computer industry was selected (beyond the "abundance of alliance and M&A activities")? Can the author explain why those 2 time periods were selected (1990-2010 and 1989-2010)? What does the author mean by observations (line 48)? Can examples be provided? What about "unique firm entries" (line 50)? Can examples be provided?*

We are sorry for the lack of sufficient explanations of why we chose the computer industry as the setting for testing our hypotheses. First of all, for the purposes of our research, the most important criterion for industry selection is whether resource combination is an important driver of governance choice. The computer industry is a good representative industry for testing the association between governance choice and resource combinations. The computer industry is characterized by rapid innovations and technological changes; in addition, institutional pressures are not a significant driver of industry evolution in the computer industry (Rosenkopf & Schilling, 2007; Yang et al., 2010).

Thus, in the computer industry, it is likely that the choice between alliances and M&A is driven by firms’ efforts to foster innovation or implement technological changes rather than by mimetic forces or isomorphic pressures (Deephouse, 1996; Hannan & Freeman, 1977). For survival and competitive advantage, firms in this industry are compelled to collaborate with each other and combine their resources in a synergistic manner. The level of technological dynamism and uncertainty is high in the computer industry, making alliances, in particular, a viable option for mitigating technological and market uncertainties. Furthermore, because of the high modularity in the product architectures and value chain of the computer industry (Rosenkopf & Schilling, 2007), either alliances or M&A can be a good vehicle for gaining competitive advantage in this industry.

In addition, the extensive use of alliances and M&A results in well-connected industry networks, making this industry a more appropriate setting for the test of our theory, which builds on information circulation within a structurally embedded region. And in our view, this is why many studies drawing from network theory have chosen the computer industry (e.g., Yan, Lin, & Lin, 2010; Schilling & Phelps, 2010). On page 14, line 13 to line 32 of the revised manuscript, we added the above reason for selecting the computer industry as our empirical setting.

One reason that two time periods were selected is the lag between the financial control variables and the dependent variable. For the financial control variables, we used one-year lagged values to allow for the “recording interval” of financial statements, which can be as long as one year (Zaheer, Albert, & Zaheer, 1999).

Per your comment, we recognized that such an explanation was missing from the previous manuscript. Thank you. We added the following sentences to the revised manuscript on page 14, line 36: “Two different time periods were chosen because one-year lagged values were used for financial control variables. This one-year lag is purported to allow for the ‘recording interval’ of financial statements (Zaheer, Albert, & Zaheer, 1999), given that accounting data is updated on a fiscal year basis and takes up to one year to be generated.”

Our sample begins in 1990 because alliances and M&A transactions are relatively complete in the SDC database from 1990 onward. For the same reason, other related studies adopt very similar sampling periods (e.g., Wang & Zajac, 2007; Yang, Lin, and Lin, 2009). The availability of the alliances and M&A database to the authors goes up to 2010. In addition, a 20-year time window provides a reasonable number of samples for studying the impact of resource combinations and networks on the choice between alliances and M&A.

“Observations” means data records (financial information from COMPUSTAT) or alliance or M&A transactions (from SDC Platinum), which are included in our sample. To avoid confusion, we made changes to the revised manuscript by changing the term “observation” to “firm-year observations” or “transactions” wherever applicable, as the latter is the more common term in the literature: see page 15, line 6 of the revised manuscript, for an example. “Unique firm entries” refers to the set of firms included to the sample. In other words, it simply means “sample firms.” We used the term “unique” to distinguish clearly between “observations” and “firm entries.” To avoid confusion, we made changes to the revised manuscript by changing the term “unique firm entries” to “firm entries”: see page 15, line 8 of the revised manuscript, for an example.

For example, in our sample of alliance transactions, 3Com Corp engaged in 57 alliances in total between 1990 and 2010. IBM was one of its alliance partners. IBM formed a total of four alliances with 3Com Corp; 3Com Corp and IBM formed four separate alliances, in 1990, 1992, 1995, and 1999. In this case, between 3Com Corp and IBM there are four alliance “observations,” “transactions,” or “firm-year observations,” and there are two “unique firm entries,” which are 3Com Corp and IBM.

*page 14, lines 17-27, where do the number for the "final sample" (654) come from (that is 633 dyads [or dyad observations?] and 487 firm entries)? How is it possible that the final sample is greater than the addition of the observations (for dyads and for M&A)?*

We agree that our writing about how the final sample was constructed was somewhat confusing. This confusion arose mainly from the repetition of dyads and firms. Because a firm can engage in more than one alliance or M&A in a sample, the number of firms uniquely identified in the sample is typically smaller than the simple sum of the observed firms. For example, suppose that we have three alliances: IBM and 3Com, IBM and HP, and IBM and Dell. The number of firms found in this sample is six, but the number of firms uniquely identified in this sample is just four. Likewise, a dyad can appear more than once in a sample. Suppose we have four alliances: IBM and 3Com (1995), IBM and 3Com (1997), IBM and HP, and IBM and Dell. In this example, IBM and 3Com created two alliances, in 1995 and 1997, for some reason. (In the current business landscape in general and the computer industry in particular, such repeated alliances seem to be the norm rather than the exception; [Gulati, 1995]). The number of dyads found in this sample is four, but the number of dyads uniquely identified in this sample is just three. In this way, the number of firms or dyads (or transactions) found in the sample is typically greater than the number of firms or dyads uniquely identified in the sample.

At the same time, after a close examination, we found that the numbers of firm entries reported in the previous manuscript were not accurate, although the numbers of dyad entries were. Our sincere apologies. The firm entries were 305 in total, with 241 for alliances and 107 for M&A. This means that 305 firms were unique and that 43 (= 348 – 305) firms engaged in both alliances and M&A.

We admit that we were inarticulate in explicating this point. In our revised manuscript, we have rewritten this paragraph for greater clarity. In so doing, to avoid any unnecessary confusion, we included just the number of transactions found in the sample. We clarified that 21 dyads came from the repetition of unique dyads (i.e., 633 dyads out of 654 transactions were unique). Finally, we apprised our readers that we reported robust standard errors to address potential inefficiency from such correlated errors.

*page 16, line 45, "On the other hand" must be preceded by "On the one hand"*

Thank you for your comment. We changed “On the other hand” to “Meanwhile.“

*page 18, line 13 and line 27, what is the difference between "prior alliance experience" and "alliance history"?*

Alliance history captures dyad-specific effects (partner specificity), whereas prior alliance experience is agnostic about partner specificity. For “alliance history,” we counted the number of alliances that two given firms had forged in the previous five years. For “prior alliance experience,” we counted the number of alliances that each firm in a dyad had entered into in the previous five years, and then summed those numbers for that dyad.

For example, imagine the following case: Firms A and B constitute a dyad. Firm A has had six alliances during the past five years, Firm B has had five alliances during the past five years, and among the alliances that Firms A and B had, two alliances were formed between Firm A and Firm B. In this case, the prior alliance experience of the dyad of Firms A and B is 6 + 5 = 11, whereas the alliance history of a dyad consisting of Firms A and B is 2.

For greater clarity on these terms, we revised page 19, line 34 to page 20, line 6 of the revised manuscript.

*page 22, line 44, add "may" (and delete s on "regulates"). So the sentence should be "... how structural embeddedness may regulate governance choice."*

Thank you. We made changes per your suggestion. Now the revised sentence is on page 24, line 32 of the revised manuscript.

In addition to responding to the concerns you raised, we updated our literature review in the theory section by including some of the most recent studies on organizational governance, resource combinations, and inter-firm networks. Additional references are as follows (you can find a bibliography at the end of this correspondence).

* Strategic alliances allow firms to augment and recombine internal and external resources and capabilities (Carayannopoulos & Auster, 2010; Rice, Liao, Martin, & Galvin, 2012).
* Geographical cluster co-location as a determinant of governance choice (McCann, Reuer, & Lahiri, 2015).
* Additional citation on dyadic approach to examining governance choice (Yang, Lin, & Lin, 2010).
* Alliances and M&A present differential constraints and opportunities for future corporate growth (Lockett, Wiklund, Davidsson, & Girma, 2011).
* Alliances and M&A offer differential capacities to exploit resource complementarity (Wiklund & Shepherd, 2009).
* Additional citation on the governance choice between alliance and M&A (Reuer & Ragozzino, 2011).
* Alliances as a vehicle for assimilating technological resources and implementing breakthrough innovation (Srivastava & Gnyawali, 2011).

Again, thank you very much for providing your invaluable comments and insights, which greatly helped us improve this manuscript.

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