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Report on the dissertation thesis 'Mapping the Proliferation of Strategic Partnerships' submitted by Ondřej Rosendorf

This outstanding dissertation thesis has three major aims: *first*, to map the proliferation of strategic partnerships between 1993 and 2020; *second*, to account for their formation; and *third*, to determine whether strategic partnerships relate to formal alliances in a complementary or substitutive way. It is a highly original piece of work that makes a significant contribution to advancing our knowledge of international relations. More precisely, by analysing the proliferation of strategic partnerships as a distinct form of alignment, this dissertation advances the literatures on security cooperation, on informal global governance institutions, and on global governance complexes. The contribution of the thesis to furthering the extent of our empirical knowledge about strategic partnerships is particularly noteworthy. Indeed, the development of the "Bilateral Intergovernmental Strategic Partnerships (v1.0)" dataset of strategic partnerships (SPs) which captures a total of 382 BISPs established between G20 'members' (excluding the EU) and any other country during the period from 1993 to 2020 can be considered groundbreaking. A major theoretical contribution of the dissertation is the typology of strategic partnerships which is based on their underlying functions: "strategic partnership in name only"; "strategic partnership as a tool of soft balancing"; "strategic partnership as an extension of alliance ties"; and "strategic partnership as a reassurance tool" (see pp. 68-71).

In the following, I will examine the theoretical and methodological underpinnings of the thesis; the quality and comprehensiveness of the literature review; the coherence, presentation, and logical structure of the dissertation's main arguments; and the clarity and logical structure of the results presented in the thesis.

1. *Theoretical and methodological underpinnings*

The dissertation is based on a clear and stringent conceptualization of strategic partnerships which is rooted in a set of academic literatures that have so far been largely unconnected. The major upshot of this exercise is the typology of strategic partnerships mentioned above. The author's thorough

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conceptual and theoretical work establishes a solid basis both for the encompassing mapping of strategic partnerships and the time-series cross-section (TSCS) analysis of their creation between 1993 and 2020.

The research questions are clearly articulated and analytically highly significant (see p. 4-5): *First*, what is the extent to which strategic partnerships have proliferated among the G20 over time? *Second*, what factors explain the formation of strategic partnerships? *Third*, do strategic partnerships complement or substitute for traditional alliances?

The author engages in a very encompassing data-gathering exercise which involves human coding and is carried out in a very transparent manner (see Chapter 3). On that basis, he implements a time-series cross-section (TSCS) analysis of BISP onset with which he tests a set of ten hypotheses, two of which are originally developed by the author and eight of which are derived from existing theoretical approaches to informal global governance institutions. It should also be noted that Chapter 3 appropriately discusses the possible limitations of the author's empirical approach.

I find the overall research design as well as the data collection and the methods of analysis very much convincing. That said, the theoretical stringency of the dissertation could have been further strengthened by explaining in greater detail how hypotheses 2-10 were derived from the three general types of explanations (functionalist; power-oriented; domestic politics). In other words, the dissertation would arguably have benefited from relating each of those hypotheses more strongly to the respective general type of explanation. In that context, it is also conceivable that a functionalist explanation might put a stronger emphasis on the ability of strategic partnerships (as a specific form of global governance institution) to solve a given collective action problem.

Regarding the challenge to exclude 'false positives', I think the author's inclination to adopt a minimum level of activity as a requirement for including a SP in the dataset in the next iteration of the dataset is the right one (see p. 225). In my reading, this move would be conducive to further strengthening the validity of the dataset.

2. *Quality and Comprehensiveness of the literature review*

The thesis demonstrates excellent knowledge of the pertinent strands of the IR literature. It is firmly rooted both in scholarship on security cooperation and in research on global governance institutions. Said in a different way, the author successfully relates his research project to more general bodies of knowledge in the field. Indeed, he connects strands of the IR literature which have hitherto been largely unconnected. Importantly, the author also situates his BISP dataset in the landscape of existing datasets that are relevant to his research project (see pp. 91-99). All in all, the author skilfully identifies both shortcomings of existing datasets and gaps in the pertinent academic literatures. In doing so, he shows the exercise of critical judgement regarding both his own work and that of other scholars in the field. I find this exemplary. Going forward, I recommend the author considers relating his work also to the "contextual design approach" developed by Copelovitch and Putnam (2014) which theorizes the impact

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of the "institutional context", that is, of the "presence or absence of existing and prior agreements between prospective partners" on the design of a new institution. This approach also informs the contribution by Reinsberg & Westerwinter (2021) which is cited several times in the thesis.

3. *Coherence, presentation, and logical structure of the dissertation's main arguments*

This thesis is a coherent body of academic work that presents the results of the author's research in a critical and scholarly way. The overall structure of the dissertation is very clear and convincing.

To start with, the author clearly defines the central concepts of the thesis and carefully distinguishes them from cognate concepts. On pp. 33-37, for example, he painstakingly distinguishes strategic partnerships from alliances, security communities, and coalitions. Relatedly, he convincingly conceptualizes strategic partnerships as informal institutions, more precisely, as a specific form of IIGOs (see Section 2.3).

The author convincingly argues that strategic partnerships represent "low-cost alternatives" to formal alliances. Going forward, I encourage him to think about what follows from the low-cost nature of strategic partnerships for their ability to provide the collective good 'security'. In other words, given what we know/what has been argued about the governance strengths and weaknesses of low-cost institutions, how can we expect outcome variables that IR scholars care about to be affected by the creation of a strategic partnership as a "low-cost alternative" to a formal alliance? Said in a slightly different way, which drawbacks emerge the fact that strategic partnerships are not a full but only a low-cost alternative to alliances? Relatedly, the coherence of the argumentation would possibly have benefited from a more systematic discussion of the governance strengths (e.g., flexibility) and weaknesses (e.g., inability to establish a credible commitment/send costly signals) of informal institutions (such as strategic partnerships) in the security realm.

The author argues that "IIGOs are better suited to the post-Cold War international environment, which is characterized by high uncertainty stemming from rapid political and technological changes" (p. 45). In my reading, this claim might be a bit too sweeping. While IIGOs certainly fulfil an important function in an international system that features high uncertainty, I am somewhat hesitant to accept that they are *across the board* better suited to such an environment than FIGOs. In fact, I would argue that this depends, among other things, on the cooperation problem that needs to be solved. Thus, going forward, I would encourage the author to elaborate more on this sweeping claim (which might be based on a set of tacit assumptions) or to condition it.

What is more, the author argues that "there has been a predominant focus on recognizing the complementary and competing functions, with comparatively less attention given to the substitutive and accommodating functions" of global governance institutions (p. 63). While I agree with the author that the existing institutionalist IR literature focuses strongly on whether overlapping global governance institutions compete with or complement one another, I am wondering whether and to what degree his

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own concepts (*substitutive* and *accommodating*) are genuinely different from the those used in the existing literature (*complementary* and *competing*). In my reading, substitution may be the result of (some form of) competition; and accommodation may be very similar to (or the precondition of) complementarity. Against this backdrop, I would encourage the author to explain how the (new) concepts developed by him relate to the (old) concepts used in the existing literature as he moves forward with this research project.

Finally, as a result of the careful revisions that the author has implemented subsequent to the successful small defence, strategic partnerships are now npresented as "general purpose" institutions which are contrasted with "task specific" institutions (see, for example, p. 44). While I do not dispute the accuracy of this categorisation, I wonder about its implications for the relationship between strategic partnerships and alliances. If I see it correctly, alliances facilitate military cooperation in the face of a military conflict. If so, they are, by definition, task specific. Thus, the author effectively seems to argue that a general-purpose institution (a strategic partnership) can serve as a *complement* or a *substitute* of/for a task specific institution (an alliance). In my reading, the author may be well advised to dwell a bit on that point.

4. *Clarity and logical structure of the results presented in the dissertation*

The main findings of the dissertation are, *first*, that strategic partnerships have proliferated more significantly than existing research assumes; *second*, that strategic partnerships are crated for different reasons with security concerns being only one of them; and *third*, that strategic partnerships serve both as complements and "low-cost alternatives" (substitutes) to formal alliances. Especially since the dataset developed by the author captures a total of 382 BISPs established between G20 members (excluding the EU) and any other country during the period from 1993 to 2020, the empirical contribution of the dissertation is truly impressive. In addition, the descriptive data presented in Chapter 4 reveal several trends that can be considered important for international relations. Moreover, uncovering the factors that significantly influence the emergence of strategic partnerships is an important contribution that significantly advances our knowledge of this form of (security) cooperation among states.

The author appropriately analyses the formation of strategic partnerships as a multicausal phenomenon and tests a large set of variables derived from different types of explanations. To the best of my knowledge, the time-series cross-section (TSCS) analysis of the creation of strategic partnerships between 1993 and 2020 and the use of logistic regression methods to analyse dyadic BTSCS data is both appropriate and well-executed. To my mind, it is also worth mentioning that the author reflects well both on what the specific nature of dyadic BTSCS data means for the empirical analysis and on the implications of his coding procedure; and that he appropriately controls for temporal dependence dyadic clustering. The author is also to be commended for discussing the limitations that both his method of case selection and the informal nature of strategic partnerships impose on the empirical findings of his dissertation. He acknowledges explicitly that the dataset includes 'only' a specific type

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of SPs which is characterised by a considerable power asymmetry. All in all, my reading is, *first*, that the data gathered and used in the dissertation enable the author to develop convincing answers to the three research questions that are at the core of this ambitious project; *second*, that the quantitative methods used are appropriate and well-executed; and *third*, that the results are presented in a clear and understandable way.

Let me close by reiterating that I read this piece of work as an outstanding dissertation thesis which makes a significant contribution to advancing our knowledge about strategic partnerships as an important institutional vehicle to facilitate cooperation among states. In my reading, the careful revisions implemented by the author subsequent to the small defence have furthered strengthened the thesis. Against this backdrop, I consider this piece of work an extraordinarily successful dissertation thesis and wish to congratulate the author on this impressive achievement.

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References:

Copelovitch, Mark S. & Putnam, Tonya L. (2014): Design in Context: Existing International Agreements and New Cooperation. In: International Organization 68: 2, pp. 471-493.

Reinsberg, Bernhard, & Westerwinter, Oliver (2021): The global governance of international development: Documenting the rise of multi-stakeholder partnerships and identifying underlying theoretical explanations. Review of International Organizations, 16: 1, pp. 59–94.