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Reviewer report of doctoral dissertation by Lagdalena Kania

“Development aid policy of sub-state governments in the European Union”

This doctoral dissertation examines the decentralized cooperation in development by focusing on sub-state governments in four European countries (Belgium, Germany, Spain and the United Kingdom). Specifically, it aims to examine what the models are of these sub-state governments, departing from four hypothesis in this regard: (1) EU-driven multi-level governance model, (2) state tutelage model, (3) traditional donor model, and (4) local actors' advocate model.

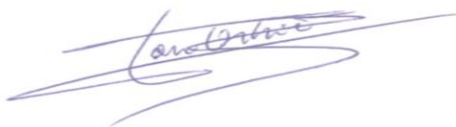
My general evaluation of the research is positive. There is much to be applauded in this research, which is characterized by a profound and comprehensive approach in terms of literature review and empirical scope. The author has examined an impressive number of studies and empirical data and her research has been enriched by interviews with relevant stakeholders and by (archival) research into primary documents. The thesis is also written very fluently, and the author has compiled useful tables and figures synthesizing the main findings.

Hence, I would recommend the thesis for the public defense.

Of course, I do have some comments and questions, which may be addressed during the PhD defense or in the context of further research. Below I specify my feedback while going through the different chapters of the dissertation.

Nonetheless, I would like to specify that these should be seen as an invitation for further discussion and not as obstacles in terms of the successfulness of the dissertation.

Sincerely yours,



Prof. dr. Jan Orbie

1. Introduction

The research goal, research question(s) and research design are elaborate clearly in this chapter. In terms of research questions, the author makes a useful distinction between exploratory / descriptive, explanatory and comparative auxiliary research questions.

The author has indeed found a gap in the literature, and one that is increasingly relevant. She makes a valuable point (p.16) that existing literature would mostly focus on cases studies in 'developing countries'.

Furthermore, key concepts are adequately defined.

One question concerns the framing of the research topic in terms of being 'normative', because it is 'non-economic (p.13; also p.95). Although I understand the author's point, more critical reflection might be welcome on whether development is indeed 'normative' and whether this label does not apply to for instance foreign economic policies. Indeed, further on in the thesis the author mentions economic, political, cultural and security interests (pp.81-82) in development cooperation.

Furthermore, it remains a bit unclear what is meant with the statement that 'public policy being largely a social construct' (p.14), and what the implications of this are for the research in the thesis. Also, the author does not come back to this insight, for instance in the concluding chapter. Furthermore, it is unclear how this fits with the point made on p.43 that the constructivist interpretation (of structure) is not followed in this thesis.

I sympathize with the 'inductive' and 'interpretive' approach that the author seems to advance (pp.17-18), especially because this is a novel field of research. However, it is not entirely clear how this corresponds with the theoretical framework that will be elaborated in the subsequent chapter: is this framework constructed before or after the empirical research? And after having completed the dissertation, what are the authors' reflections on the pros and cons of inductive/interpretative research (some elaboration on this in the conclusion would have been interesting)?

In the same vein, how have the hypotheses (p.25) been constructed? Do they come from literature or rather from the empirical analysis? If they come from literature, then it may be useful to know from which scholarship; if they come from the empirics, then it is unclear why apparently some cases do not fit the hypotheses (see conclusions). As such, the hypotheses are fine and useful, my question only concerns their origins and embedding.

Similarly, while the author's point that the research aims to 'problematize' seems very valid, it is not entirely clear what exactly has been problematized. Again, some ideas on this in the concluding chapter would have been an added value.

In terms of research design, it is clarified on p.20 that the selected cases are limited to stakeholders of the EU, which 'makes it possible to analyze the 'common layer' of EU development cooperation programs and projects directed towards cities and regions from member states'. If this is the case selection rationale, then I wonder why the author has not chosen for 'Europeanization' as (part of) the theoretical framework? Indeed, Europeanization

literature concerns the extent to which the EU has had a differential impact on the policies, politics and politics of EU member states and significant attention has also been paid to impact on sub-state governments.

In terms of auxiliary research questions:

- are (ed)RQ6 and (ed)RQ8 not explanatory (what are determinants / why) rather than exploratory/descriptive?
- are (ex)RQ4 and (ex)RQ5 really explanatory questions (they sound rather descriptive)
- how is (c)RQ1 different from the main research question on the models?

Methodologically, some reflections on the rejected interviews (p.28, table 1.01), and its implications for the research would have been advisable.

2. Theoretical framework: sub-state governments in IR and the model of analysis

Overall, this chapter displays an impressive knowledge of different (meta)theoretical approaches to IR and foreign policy. It constitutes a solid and profound theoretical grounding for the thesis and establishes a framework that will be applied systematically throughout the remainder of the dissertation. This is also in line with the justifiable decision to employ a 'medium-range' theoretical framework that combines insights from different theoretical schools.

This framework is indeed innovative, although it remains unclear why some decisions have been made. For instance, why is the threefold 'interests – institutions – ideas / perception' distinction used at the subnational level but not at the national and European levels? Or vice versa, why is the distinction between actors, strategies and mechanisms not used at the sub-state level? In other words, the author could have paid more attention to the coherence of the different elements of the framework, and how these relate to each other. Furthermore, the added value of the framework (and perhaps its limitations and its use for further research) could have been reflected upon in the thesis' conclusions.

The author could have reflected more on the implications of choosing the rather 'objective' approach to structure (p.43). What might be missing from the decision to not take a constructivist approach? What would a constructivist approach to external structures imply for this PhD research and how might this be followed in future research?

On p.52 it is stated that realism 'tears off the normative veil': is it really true that there is no normative dimension to realist theory? Or how should we interpret the author's views in this regard?

Still on realism, it may be true that the realist paradigm is resilient in IR (p.59), but is it not the case that *in Europe* realism remains a marginal theoretical approach (see e.g. Jørgensen and Jørgensen 2020)?

The author regularly mentions the 'paradigms' in IR theory (and she even mentions the Kuhnian perspective which leads to interesting questions on whether/how this applies to social science and humanities research). However, critical (including Marxist) approaches are conspicuously

lacking in the thesis (only one reference to Marxist approaches p.61). Why is this the case? What about Foucauldian, neo-gramscian, poststructuralist, feminist, world system theory and other critical approaches? (on p.138 there is a brief critical reflection on how the rise of NGOs and decentralized actors fits with the neoliberal agenda – but this is not elaborated further)

Also, we may discuss whether constructivism really synthesizes the essentials of realism and liberalism (p.75), or whether it constitutes an entirely different paradigm; and whether constructivism is really agency centred.

While the lengthy elaboration on the main IR paradigms is interesting and very well done, the question arises again why not more EU-oriented frameworks such as historical institutionalism and 'europeanization' have been employed for this research (and, for instance, how 'europeanization' relates to the term 'communitization' – p.95).

In terms of actors (p.91), the author explains later in the thesis (but not in this chapter) why the EEAS has not been focused on. However, it is not clear throughout the thesis why the European Investment Bank is not considered.

While the author generally pays much attention to defining key concepts, the notion of 'politicization' (p.95; also p.119) is not defined. This may be necessary given the emerging literature on politicization of EU external (including development) policies which uses the concept in a different way.

3. Global level: from politics to policy in development cooperation

This chapter again demonstrates the author's in-depth knowledge of the context and literatures around the thesis research topic. The chapter provides a useful overview of development policy thinking and policies at the global level, and how regional and local authorities have traditionally been neglected in this regard. The chapter is based on a wide range of secondary literature.

Just one comment: the section 3.3.2 does not seem to follow a logical chronological order. The MDGs are not or barely mentioned, and the SDG / Agenda 2030 agenda is mentioned before the aid effectiveness agenda. It would have been better to start with the aid effectiveness agenda and the MDGs, before elaborating on the Agenda 2030 and the SDGs.

4. European level: toward the EU decentralized cooperation in development

This chapter provides a useful, in-depth overview of the EU's approach to decentralized cooperation. It takes a temporal perspective and is strongly based on secondary literature, expert interviews and official documents. This is an excellent synthesis and also figures (e.g. scheme 4.01, Table 4.02) are very helpful for the reader. Overall, the point made that the Commission focuses more strongly on civil society than on local authorities is a relevant and interesting one. The author may consider writing a separate academic article around this argument.

The chapter also explains why the EEAS has not been considered in the analysis (pp.159-160).

What does the author mean with the 'assumption that the EU has its own and distinctive approach to development cooperation worldwide' (p.158), and having finalized the research how does the author think about this alleged distinctiveness? Specifically, how is the EU's approach different from the global approach to decentralized cooperation in development, i.e. the topic that is covered in the previous chapter? It would have been interesting to explore the similarities and differences between the global and European approach, and this would have made it possible to draw meaningful conclusions on the 'distinctiveness' of the EU in this regard.

The analysis of the European Parliament's role in development is relatively limited. For instance, it would have been useful to involve the chapter by Verschaeve and Delputte and the article by Cardwell and Jancic.

As the author generally refers to 'paradigm shifts' (here on p.185 and 186, p.200, also in chapter 3), it may be interesting to reflect more on what exactly the paradigm constitutes. Despite the reference to Kuhn and some exploration of philosophy of science in the theory chapter, the research would benefit from a more detailed and critical analysis of so-called paradigm shifts. Concretely, would the shift towards decentralized cooperation really constitute a paradigm shift? (see also my article with Sarah Delputte in JCER 2020)

The overview of the EU's development thinking is not completely up to date. From this chapter, it seems that the EU is still very much following the 'aid effectiveness' thinking of the mid-2000s. However, as existing literature has also illustrated, there has been a shift away from the aid effectiveness agenda and the 'new season' in EU development policy is already over.

The analysis of the NSA-LA thematic program is relatively short and superficial (only three pages, compared with the longer analysis of the regional programs that concern much less budgets). Has the author not considered to elaborate more on the thematic program, for instance by examining case studies?

Although I appreciated the analysis of the MFF 2007-2013, the author states that the MFF 2014-2020 could not be analyzed because there is no data available. This is still a shortcoming because it makes the analysis a bit outdated. I wondered if the author has not explored other ways of getting to know more information (e.g. annual reports?). Furthermore, there is no analysis of the MFF 2020-2027 – for instance based on the Commission 2018 proposal and subsequent discussions in the EU institutions.

5. National level: development policy and the domestic structures and inter-governmental relations

Following the theoretical framework, this chapter carefully explores the different dimensions of development policy in the examined countries. This chapter is empirically very rich. It is partly based on primary documents (e.g. legal texts) from within the countries. The synthesizing and comparative tables are to be applauded.

The downside of being so comprehensive, is that the political science analysis of some parts remains relatively scant. For instance, referring to the case that I know best (Flanders) there is no analysis of the politics behind the Lambermont agreement and its deal on development policy, or behind the decision to integrate the Flemish development agency into foreign affairs. Similarly, there is not much attention for explanations.

The introduction frames the chapter into the evolution of development policy from a foreign policy instruments towards an aid effectiveness tool. As stated before, this evolution could always be problematized, as development policy has always been embedded into a wider western (arguably neocolonial) agenda, and certainly in recent years it has more explicitly become overshadowed by other agendas (migration, security, trade and investment). Furthermore, the author seems to suggest that this shift away from foreign policy provides a fertile ground for local authorities to emerge as relevant actors. If this is the case, would this then mean that the increasing securitization and instrumentalization of development policy will entail a declined relevance of decentralized cooperation? In other words, would decentralized cooperation fade away again in light of such trends, or is it here to stay?

While country data are very rich, they are rarely presented in a comparative way. It usually takes very long until a synthesizing and comparative table is produced – usually at the end of a section (e.g. p.317; however table 6.01 misses labels of columns). Further research could engage in a more direct and explicit comparative analysis. This would also imply that the same data are analyzed for each country (e.g. data on Scottish visits to Europe p.281 – how to compare with other countries/ regions).

6. Sub-state level: objective and perceptual determinants of decentralized cooperation

This chapter provides the empirical core of the dissertation. Again, it is very rich empirically, including a wealth of data about the analyzed regions, and it is well embedded in existing literature on e.g. nationalism.

Similar questions come up as in chapter 5 relating to the relative lack of explanations for the findings, the trade-off between comprehensiveness and in-depth analysis, (e.g. the part on Flemish nationalism (p.346) remains rather limited on the role of the first World War and the history of collaboration), and the relative absence of data that are directly comparable (e.g. on preferred areas of intervention, p.429).

On diverging budgets for development cooperation, p.436, scheme 6.10: do you have the data per capita and as a % of GNI? And how would you explain differences between the regions? In many respects, the dissertation makes clear that Flanders and Catalonia are in the same 'camp', but apparently this is not really the case when it comes to budgets? Another why question that emerges : why does the Flemish Government not finance ngos based in Flanders to implement projects and programmes in partner countries?

In section 6.4, the dissertation proceeds with analyzing political, cultural, economic, migration and security interests, as already advanced in the theoretical framework (see above). Why are historical interests (and specifically, the colonial dimension) not addressed here? Is it because

the author thinks that historical interests do not play out here; or are they rather inherent to all other types of interests? There is just one brief reference to 'long-lasting historical links' in the context of cultural interests (p.458).

On the same part, why are 'migration and security interests' treated together? Is it because the observed regions tend to treat them as the same (however, this would be contradicted by the findings), or is it that the author considers migration and security interests to be closely related (how?)?

How is the conclusion that sub-state governments 'make a significant contribution' (p.467) substantiated? For instance, what are criteria for making a 'significant' contribution, as opposed to limited contribution, and how have the analyzed case studies proven to reach these criteria? The analysis has provided a wealth of information on the sub-state governments' policies and activities, but it remains difficult to see on which basis the author has established threshold for limited/mediocre/significant contribution.

7. Conclusion

The author comes back to the main hypotheses, which are systematically tested and compared to the analyzed cases. Interestingly, it appears that some hypotheses are rejected, while others are confirmed, and yet there is also a need for further models. This is a very interesting analysis that could be the core argument in an academic paper on the subject. In doing so, I would advise to elaborate the different models conceptually and also operationalize them in a more detailed way.

Overall, the conclusions are clear and relevant. They come back to the main research question and hypotheses. My main comments concern what is not mentioned in the conclusions, for instance what the author considers to be possible shortcomings and what areas for further research are suggested, and also the different auxiliary questions that were mentioned on pp.23-24 are not referred to explicitly in the conclusion. Furthermore, it would be interesting to discuss with the candidate what possible explanations are for the main findings and to what extent the theoretical model constructed in this dissertation can be used in this regard. Finally, I would also like to discuss the statement that paradiplomacy 'will be more participatory' in the future and specifically whether this expresses the personal hope of the candidate or a scientifically proven (if at all possible) argument.

Typos:

- p.155: Jean Bossuyt (not Fabienne Bossuyt)
- p.212: Communication (not Communicate)