

Dr. Christophe Nihan
Associate Professor
Hebrew Bible and History of Ancient Israel
Institut romand des sciences bibliques, Director
Faculty of Theology and Religious Studies
University of Lausanne
Switzerland
Christophe.Nihan@unil.ch
++41 21 692 27 17
++ 41 21 692 27 30 (secretary)

Lausanne, September 6

Report on the doctoral dissertation by Jan Rückl: “A Sure House: Studies on the Dynastic Promise to David in the Books of Samuel,” Charles University of Prague/ University of Lausanne

1. This dissertation, as its title indicates, is a study of the motif of the dynastic promise to David in 1-2 Samuel (= 1-2 Reigns). The first part of the study (pp. 17-212) is devoted to the text of 2 Sam 7, where this motif is developed in detail, whereas the second part (pp. 213-288) addresses various passages in Sam that seem to presuppose the dynastic promise as formulated in 2 Sam 7, namely 1 Sa 2:27-36; 1 Sam 25:28; 2 Sam 22:51 and 23:1-7. In a rather brief section entitled “Conclusions and further perspectives” (pp. 289-309), Rückl addresses further issues related to his topic, such as especially the passages in Kings that appear to introduce a concept of the dynastic promise as conditional to the king’s loyalty to YHWH. There follows in addition a long appendix (310-337), in which Rückl offers a detailed discussion of the so-called “law of the king” in Deut 17:14-20 in relation to the books of Sam and Kgs, especially 1 Sam 8 and 1 Kgs 11.

Overall, this is a solid dissertation, which offers some excellent analyses and makes several important points. In many ways, it represents an important contribution to the research on 2 Sam 7 and the motif of the dynastic promise to David that deserves to be published, although perhaps with some revisions (more on this below). From a formal perspective, the argumentation is generally clear and well structured. The analysis is usually thorough, especially as regards matters of textual criticism, Hebrew linguistics, and analysis of the comparative evidence—such as, e.g., royal inscriptions—that Rückl discusses at several points in his study. Unfortunately, the English is not very good, and could have been improved. There are grammatical errors almost on every page of the dissertation and, more importantly, in several instances the syntax is awkward and significantly complicates the reader’s understanding of Rückl’s argument, or may even lead the reader to misconstrue that argument. Compare, among many other examples, the following sentence: “We should mention, in this context, that v. 1b is not merely 11aβ in the 3rd p, which itself makes McCarter’s reconstruction of scribal errors leading to v. 1b highly doubtful” (p. 24). In addition, some abbreviations are clearly not English but may presumably reflect Czech usage (?), such as for instance the abbreviation “Kr” for Kings on p. 166, or the use of the letter “n”

instead of “f” to refer to the following verse in the discussion of 2 Sam 7:6-7 (p. 195, 196). Clearly, the English will require extensive revision for the publication. That said, Rückl should nonetheless be praised for his decision to write his dissertation in English, which will no doubt make it accessible to a broader audience.

In terms of its content, the dissertation achieves several important results. Rückl’s general argument—namely, that the promise of an everlasting Davidic dynasty in 2 Sam 7 and related passages is not a “remnant” (so to speak) of royal ideology going back to the time of the First temple but on the contrary a complex ideological construction postdating the end of the kingdom of Judah, whose basic function is to reinterpret the traditional relationship between king and temple in Ancient Near Eastern (or Western Asian) societies in order to dissociate the Davidic dynasty from control of then temple—is attractive and rather compelling, although it may require some qualification (see below). In this respect, the dissertation represents a major contribution to our understanding of royal ideology and—more generally—the construction of the discourse on kingship in ancient Judah. At the same time, Rückl’s study also represents an important contribution to the scholarly discussion on the textual and redactional history of 2 Sam 7. Against several recent studies that have privileged the Greek text (either the Vaticanus [LXX^B] or the Antiochic text [LXX^L]) as the oldest and best witness to the ancient text of 2 Sam 7, Rückl convincingly demonstrates that the matter is significantly more complex, and that in many passages there are solid grounds to prefer the Masoretic text (MT) against the LXX. Rückl’s careful and thorough discussion of the relationship between the various textual witnesses to 2 Sam 7—such as especially 2 Sam MT, 2 Sam LXX, 1 Chr 17 MT and LXX—has many further implications for the present discussion on the transmission of the text of Samuel and the relationship between Sam and Chr in the Second Temple period. The close reading of 2 Sam 7 that Rückl offers is often illuminating; even if one could have wished more in-depth engagement with some of the literary-critical observations that have been made in 2 Sam 7, I think Rückl does make a substantial case for the relative unity of that text (or, more precisely, of vv. 1-17), a finding that has likewise significant implications for the compositional history of the books of Samuel. Other aspects of Rückl’s discussion are also illuminating, and contribute to significantly improve our understanding of the David traditions in Samuel, especially as regards his analysis of 1 Sam 2:27-36 and Deut 17:14-20 as two very late passages, presumably of post-dtr origin. Throughout his dissertation, Rückl demonstrates considerable philological expertise, especially as regards the discussion of text-critical issues as well as of Hebrew lexicology and syntax, as well as a good command of the social, political and economic history of Judah between the 7th and 5th centuries BCE. He also demonstrates good command of the non-biblical material, and the way in which he uses epigraphic evidence in his discussion is generally illuminating and convincing.

At the same time, Rückl’s dissertation does raise in my view a number of issues of method. A comprehensive discussion of these questions cannot be offered in the limits of this report. Instead, I would like to mention three points in particular: (i) Rückl’s discussion of the textual history of 2 Sam 7; (ii) his interpretation of 2 Sam 7; and (iii) the place of 2 Sam 7 in the compositional history of the books of Samuel and, more broadly, of the collection formed

by Sam-Kgs (1-4 Reigns). My most serious criticism, as it turns out, concerns the third point; as regards the first two points, I mostly offer some qualifications to Rückl's approach.

2. As noted above, Rückl's dissertation begins with a comprehensive and painstaking discussion of the various forms preserved for 2 Sam 7 in the main textual witnesses, which covers 120 pages in total (p. 17-136), i.e., more than a third of the entire dissertation, even if one includes the appendix. I fully agree that the traditional preference given by the majority of critics to the MT can no longer be maintained, and that scholars need to take much more seriously the issue of textual fluidity in the period of the Second Temple when they analyze these texts. Furthermore, as I mentioned above, the detail and rigor of Rückl's analysis of the variations between the main textual witnesses for 2 Sam 7 are impressive, and represent a valuable contribution to our understanding of the textual history of that passage—and, beyond, of the books of Samuel—, especially (albeit not exclusively) as regards the value of MT as a witness to the ancient text of 2 Sam 7. Nonetheless, there are some aspects of his analysis that I find problematic.

2.1. To begin with, however one appreciates the text-critical decisions made by Rückl, the space devoted to this discussion seems out of proportions with regard to the actual impact of that discussion for the general theme of this study, which, after all, is not to offer a new critical edition of 2 Sam 7 but to analyze the motif of the dynastic promise to David in Sam. To be sure, *some* of the text-critical decisions made by Rückl have important implications for his overall analysis of 2 Sam 7—as, e.g., in the case of vv. 11-12—but in most instances this is not the case and the variants discussed, however interesting they may be from a strict text-critical perspective, do not significantly contribute to the following interpretation of the text. This point may seem a little formal, but it is actually quite significant in my opinion because that space could have been much better used to address other issues that, despite being central for Rückl's theme, are hardly touched—or even entirely ignored—by him, as we will see below. In addition, there is something slightly positivistic to this entire section. In many instances, the reader gets the impression that the purpose of the whole discussion is to reconstruct something like the original text of 2 Sam 7—a term that Rückl himself uses several times, although, interestingly enough, often with quotes (“original”)—, and this impression is further corroborated at the end of the section when Rückl offers a comprehensive classification of all (!) the variant readings in 2 Sam 7. To me, however, what this lengthy discussion demonstrates very clearly is precisely that we are no longer able to reconstruct the original text of 2 Sam 7, at least in the present state of our documentation; there are simply too many places where we do not have sound criteria to decide between variant readings. The best we can do is to approximate what the earliest retrievable form of the text (which is different from the “original” text!) may have looked like, and even so, many details will forever remain uncertain. This makes it all the more important, in my opinion, to focus on the textual differences that are really significant for the interpretation of a given text or passage, rather than to attempt to classify each and every variant reading.

2.2 A further problem is raised by the fact that one would have expected the discussion of individual variants to be prefaced with a general discussion of the various textual forms preserved by the main witnesses. Instead of this, the reader must wait until p. 105 (!) for a

systematic comparison between 2 Sam MT, LXX (B and L), 1 Chr 17 MT and LXX; and it is not until p. 126 (!!) that the methodological issue raised by Chr as a textual witness for the ancient text of Sam is addressed in some detail. This choice has several problematic implications, especially because the evaluation of individual readings cannot ignore the question of the general value of the witnesses in which these readings are found (e.g., it is generally admitted that the text of Antioch (*L*) is not a strong witness for the Old Greek (OG) of Samuel in the *non-kaige* portions of these books, to which 2 Sam 7 belongs; evaluation of the Chr LXX must take into account that the Greek text of Chr has been partly aligned on Sam LXX, etc.). As a matter of fact, Rückl's analysis of individual readings consistently presupposes a certain understanding of textual witnesses, which however often becomes clear only toward the end of the analysis. Moreover, even the final discussion of the main witnesses is somewhat wanting. For instance, the presentation of 4QSam^a reflects the state of the discussion some 20 or 30 years ago. Rückl does not seem to be aware of the many studies that have recently argued that 4QSam^a is not the oldest witness to the Hebrew text of Sam (as Ulrich and others have assumed) but rather an excerpted text already combining readings from the textual traditions of LXX and MT (Aejmalaeus, Himbaza, etc.); this is all the more surprising since that interpretation of 4QSam^a actually tends to lend support to the view espoused by Rückl that MT is actually a better witness to the ancient text of Sam than Ulrich and others had thought. In the case of the relation between Sam and Chr, I am not certain that Rückl has fully appreciated the complexity of the issue. Rückl is entirely correct to state against McKenzie that Chr cannot be placed exactly on the same level as other ancient witnesses to Sam (p. 134), and that in assessing a textual difference between Sam and Chr ($S \neq C$ pattern) we need to take into account the possibility not only of textual accidents and tendentious changes, but also of punctual changes made by the Chronicler to his *Vorlage* yet unrelated to the Chronicler's tendency. From this, Rückl concludes that, "in the passages where $S \neq C$ and, at the same time, no specific tendency or scribal mistake is apparent in S, [...] we should, more or less automatically, prefer the reading of Samuel" (p. 134). As a matter of fact, this rule is consistently applied by Rückl in his analysis of textual differences corresponding to the pattern $S \neq C$ (or variations of this pattern) in 2 Sam 7, even in those instances where a reasonable case could be made for the priority of Chr against Sam (for instance, the plus **יְהוָה צְבָאוֹת אֱלֹהֵי יִשְׂרָאֵל** in 2 Sam 7:27, which is missing from the parallel passage in 1 Chr 17:25 LXX^B, cf. p. 99-100). However, when it is acknowledged that the Hebrew text of Sam used as a source by the Chronicler was identical neither with Sam MT nor with the Hebrew *Vorlage* on which Sam LXX was translated (although it was probably closer to the latter than to the former), the situation becomes necessarily more complex, and we need to count with the possibility that, in some cases, the ancient text of Sam is better preserved in Chr than in the ancient versions of Sam that have come down to us. Admittedly, every instance of the pattern $S \neq C$ needs to be judged for itself, and in many instances it may be difficult to reach a solid conclusion; but in my view the application of an almost blind preference for Sam against Chr to every instance of the $S \neq C$ pattern is methodologically problematic.

2.3. There are other general issues with the text-critical discussion by Rückl that may be mentioned. I must admit that I remain a little puzzled by the classification of each and every

variant reading into “non-intentional” (n), “intentional” (i) and “tendentious” (t). First, in many instances Rückl must acknowledge that such classification is problematic, or even arbitrary; this is reflected by the repeated presence of question marks after the signs “n,” “i” and “t.” More importantly, however, I must say I do not understand how it should be possible to decide whether a given reading is “tendentious” (and not intentional) without having a general idea of what general tendencies can be observed in the textual witness in which that reading is found. However, the question of what tendencies can be observed in the MT and LXX of Sam is not addressed in a comprehensive way by Rückl; even when he occasionally refers to observations by other scholars on such issues, it is unclear whether these observations are consistently applied in his discussion of individual readings. For instance, Rückl refers favorably to the view of Tov and Hutzli (and, actually, several other scholars as well, such as, e.g., Rezetko) that in MT the image of David was secondarily improved in several passages (p. 118); but if so, why does he fail to consider the possibility that MT’s *plus* לַדָּוִד in 2 Sam 7:9 (against both LXX* (minus *L*) and Chr), which uniquely attributes to David a “great name” whereas this attribute is otherwise reserved to YHWH, is a typical example of this tendency in MT, as has already been suggested by some scholars (e.g., Rezetko)? In addition, the way in which the term “tendentious” is used by Rückl is not entirely clear to me. Normally, this term should be used to refer to a variant reading introduced in one of the textual forms of 2 Sam, and which can be related to a set of variant readings elsewhere in the same textual form; this is certainly how Rückl understands the term “tendentious” in the case of variant readings in Chr and Sam. But occasionally, Rückl seems to imply that this term may also apply to variant readings within 2 Sam 7 that are closely related (compare, e.g., his discussion of variant readings in 2 Sam 7:25, p. 89-93), which I find somewhat problematic (a deliberate change to the text may trigger another change for reasons of coherence, without implying the existing of a general “tendency” underlying this sort of revision). All this raises some questions about the value and the thoroughness of Rückl’s classification, and suggests that there are methodological issues involved that may not have been thought through as fully as one may have wished.

2.4. Another general issue, in my opinion, concerns the systematic or almost systematic preference given by Rückl to the MT, except in those cases where its revisional character is too apparent to be disputed. Overall, as noted above, I think Rückl makes a good case for the general value of MT as a textual witness to 2 Sam 7. However, in several instances the criteria he uses seem questionable. In some cases, the reasons for preferring MT are simply not clear to me. E.g., why should the idea that the systematic repetition of prepositions in MT is an original feature of the text be “more plausible” than the opposite view explaining this device as the result of later stylistic revision and improvement (see p. 27)? In other cases, the criteria are not used consistently. For instance, Rückl sometimes prefers the MT on the basis that it preserves the *lectio difficilior* (e.g., on p 26 regarding 2 Sam 7:3, p. 53 on 7:15, etc.), but frequently the reason that MT should be retained against other witnesses is that it preserves a smoother text. Similarly, the principle of *lectio brevior* is applied on various occasions to support the antiquity of a MT reading, but much less often in the case of LXX, and virtually never in the case of Chr (in the latter cases, minuses are almost systematically interpreted as omissions by the Chronicler; the same remark applies to those instances where Chr preserves

the *lectio difficilior*, which apparently can never be an argument for Chr's priority in this case). Most problematic, in my view, is the argument sometimes raised that the MT should be preferred because its reading is closer to the "deuteronomistic" style (p. 20, 24-25, 34-36!, 58). Not only Rückl never defines precisely what he means by that term, nor which range it covers exactly (admittedly, this is a difficult issue); but more importantly, several critics would, on the contrary, regard the alignment of a given passage with dtr language and terminology as signaling later revision, whereas non-alignment with dtr would witness to an earlier, pre-revisional text. Significantly enough, Rückl himself acknowledges one such case of secondary dtr revision of the text of 2 Sam 7 (p. 29), which emphasizes the methodological issue involved here. Overall, these various issues tend to significantly weaken the general case made by Rückl for the value of MT in 2 Sam 7, as they suggest a certain bias for MT against other textual forms.

As mentioned above, these various remarks are mostly qualifications to Rückl's text-critical discussion, which do not invalidate his general conclusions in this section. They do suggest, however, that Rückl might usefully rethink some of his methodological assumptions in that section, as well as his decision to offer an exhaustive (and, in many respects, problematic) analysis of each and every textual differences in 2 Sam 7, instead of focusing on a selection of significant differences in which it seems possible to build a strong, solid case for the chronological priority of one textual form.

3. Another central aspect in which I find Rückl's argument in need of some qualifications concerns his interpretation of the dynastic promise in 2 Sam 7. As mentioned above, I find rather convincing his case for the relative unity of 7:1-17, as well as the general thesis that the function—or, at least, one central function—of this text is to reinterpret and resemantize the traditional connection between temple and kingship after the end of the kingdom of Judah. There are three elements, however, where I do have some issues with Rückl's demonstration.

3.1. First, it seems to me that the close reading of 2 Sam 7 (p. 162-207) sometimes lacks balance. Rückl is clearly willing to emphasize the coherence and artistic design of 2 Sam 7—mostly in the form preserved by the MT, see above—but this leads him sometimes to be perhaps too selective in his discussion of the evidence. One fine illustration of this problem is the discussion of vv. 6-7. Rückl is well aware of the long-standing scholarly tradition that interprets either part or all of these verses as a later interpolation, but in the end nonetheless wants to maintain that they are original in 2 Sam 7 (see p. 196-197). However, his arguments against the literary-critical separation of this material are weak, to say the least. For instance, I simply fail to understand why the fact that vv. 6-7 are presupposed in Chr (p. 195-196) should be a problem for regarding these verses as secondary in 2 Sam 7, especially if we accept a dating of Chr around 300 BCE (!). It is true that 2 Sam 7:6-7 appears to be presupposed in 1 Kgs 8:16 (p. 196); but several scholars (including already Veijola) have also argued that 1 Kgs 8:16 was presumably a later interpolation in its context, and there are some arguments for this view. At least, one would have expected that Rückl discusses this possibility. Above all, Rückl's discussion leaves entirely unexplained the classical crux raised by the language used in 2 Sam 7:6-7, which contains several features that cannot be regarded as dtr; this does not

only concern the combined use of *משכן* and *אהל*, as Rückl states on p. 195, but also the use of the participle of *הלל* Hithpael with YHWH as subject (cf. Gen 3:8!; Deut 23:15; for the use of *הלל* Hithpael with YHWH as subject, see also Lev 26:12, a text that has often been connected with 2 Sam 7:6-7, see Lohfink and many others). As long as a better explanation has not been offered, the view of those scholars who consider that the sudden occurrence of such late, non-dtr language in 2 Sam 7 is best explained by the assumption that these two verses are part of a later addition or have been significantly edited will have to be preferred. Very much as in the case of the evaluation of the MT in 2 Sam 7, Rückl's argument would have been much stronger, in my opinion, if he had been able to allow for some flexibility in his overall case for the relative unity of 2 Sam 7 by considering more seriously the possibility of *limited* secondary revisions in 2 Sam 7.

3.2. Second, I find it surprising that the discussion about the dating of 2 Sam 7, which recurs at several places in the dissertation (especially p. 192-194, and again p. 289ff. and 308-309), does not include a discussion of the various passages *outside* of Sam that appear to presuppose the promise of an everlasting dynasty to David—such as Isa 55:1-5; Am 9:11-12; Zech 6:12-13?; Ps 89, etc.—especially since these texts have been the subject of several studies recently (see especially Pietsch, also Schniedewind). To be sure, these texts can only provide us with a *relative* chronology for the composition of 2 Sam 7, not an absolute one; however, I do not see how it would be possible to ignore this sort of evidence in the discussion on the dating of 2 Sam 7. If, for instance, we accept that the oracle in Isa 55:1-5 (which transfers the promises to David to the people as a whole) already presupposes the promise to David in 2 Sam 7—as Rückl seems to briefly acknowledge in one passage (p. 237)—, this tends to make it more difficult to accept the second, later dating advocated by Rückl for 2 Sam 7, since a majority of scholars would date Isa 55:1-5 no later than the first half of the 5th century BCE. Admittedly, this sort of evidence can also be evaluated differently; but it needs to be discussed in any event, and it is certainly more comprehensive and solid than an argument that is exclusively based on the relative chronology of the books of Samuel (or Samuel and Kings).

3.3. Third, I also have some questions regarding the explanation of 2 Sam 7 as seeking to reinterpret the traditional relationship between kingship and temple in ancient Judah/Israel in order to suggest that the legitimacy of the Davidic dynasty was not based on the temple (or, more precisely, on the king's capacity to act as patron of the temple in the traditional ANE way). To begin with, even in the text of 2 Sam 7 itself things seem to be slightly more complex, since after all Nathan's oracle does imply that the temple will be built by David's son and heir, Solomon. Already in this regard, I find it somewhat problematic to affirm that the traditional relationship between temple and kingship would be "rejected" by the dtr author of 2 Sam 7 (e.g., p. 192). It would be more exact to say that this traditional relationship is reinterpreted in the sense that the building of the temple now *follows* from the establishment of an everlasting house/dynasty for David instead of justifying it. But the connection between temple and kingship is *not* severed or abolished; it remains on the contrary quite central, even in 2 Sam 7. This becomes even more clear when one considers 2 Sam 7 not just for itself, but in its narrative context, where 2 Sam 7 follows immediately after 2 Sam 6. Whatever the textual and literary problems there may be in 2 Sam 6, I do not see how it would be possible

to reconstruct a form of this narrative that would not emphasize David's key role in the bringing of the ark to Jerusalem; more than that, the narrative concludes by presenting David in a cultic role (vv. 13ff.)! The only parallel to this in 1-2 Sam, as far as I can see, is in the (very late) story of 2 Sam 24, where David offers an holocaust on the site where the temple will be built by Solomon (24:18ff.). Since the base account in 2 Sam 7, as Rückl reconstructs it, clearly presupposes the ark narrative in 2 Sam 6 (and, presumably, 1 Sam 4-6 as well), the juxtaposition of these two chapters raises a considerable issue for the proposed interpretation of 2 Sam 7. As a matter of fact, several authors have argued that the introduction of 2 Sam 6 and 7 into the David traditions was intended on the contrary to associate David as much as possible with the building of the temple, thereby opening a perspective that will be continued later in Chronicles. For instance, this view has been recently argued by Rudnig, who concludes: "Im Verbund mit II Sam 6* und 24,18ff* verfolgen II Sam 7,1-17* das deutliche Interesse, Plan und Vorbereitung zum Tempelbau noch im Leben Davids zu verankern" (2006:16). To be sure, the juxtaposition of 2 Sam 6 and 7 can also be interpreted differently: the two chapters need not belong to the same layer in 2 Sam, and chap. 7 could also be a reinterpretation, or a qualification, of the cultic ideology of 2 Sam 6; on the other hand, the separation of chaps. 6 and 7 might not be so easy, as the ark narrative in 2 Sam 6 offers an excellent introduction to chap. 7, and one would expect in any event that the bringing of the ark to Jerusalem would be followed by some further cultic measures. In any event, the whole issue is central for the interpretation of 2 Sam 7, and necessarily requires a discussion. Rückl somehow sees the problem, but surprisingly offers no explanation for it. On the contrary, he concludes his interpretation of 2 Sam 7:1-17 with the following statement: "2 Sam 7 seeks to legitimize the Davidic dynasty without a relation to the temple, yet the location of Nathan's oracle after the end of the Ark narrative in 2 Sam 6 hints at the legitimizing, pro-Davidic aspect of this older cult legend" (p. 207). I find this aspect of Rückl's argument very problematic. As it stands, his interpretation of 2 Sam 7 ends with an aporia. At most, the quoted statement may be taken to imply that he regards the ark narrative as an older (pre-dtr?) tradition that would preserve a more standard conception of the relation between king and temple in ancient Judah, but this assumption is never justified and, moreover, it is largely disconnected from recent research on 2 Sam 6, which sees on the contrary this chapter as a rather late text in Sam, especially in the form preserved by MT (see Rezetko, etc.). And even if we should assume that there *is* an ancient cultic legend behind 2 Sam 6, the question would nonetheless remain of why 2 Sam 6 and 7 were juxtaposed in the redaction of 2 Sam, and what was the function of such juxtaposition for the ancient audience of that book. Here, the reader can only wish that Rückl would have devoted more space to such basic questions, instead of spending 120 pages to discuss every single textual detail in 2 Sam 7. As long as this issue has not been properly dealt with, Rückl's overall interpretation of 2 Sam 7 will remain problematic, and cannot be regarded as having been properly demonstrated.

4. The last remarks are related to a more general issue, which concerns the place of 2 Sam 7 in the composition of the books of Samuel (and, more broadly, of Sam-Kgs). As I already suggested, this is the part of Rückl's work that I find most problematic. In the end, Rückl is unable to suggest what the composition of the books of Sam of which 2 Sam 7 was a

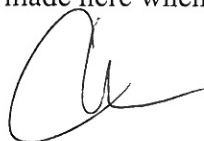
part may have looked like (as he himself acknowledges, p. 307). In his conclusion, he briefly suggests that 2 Sam 7 was part of a redaction designated as “Dynastic Redaction of Samuel 1” (= DRS1), which would have included 2 Sam 7:1-17 as well as 1 Sam 10:8; 13:7b-15a and 1 Sam 25, and which would have been followed by a “Dynastic Redaction of Samuel 2” (DRS2) responsible for 2 Sam 7:18-29, 2 Sam 22 and 23:1-7 (p. 290); however, he immediately considers the possibility that the two redactions are actually one and the same. At first, he seems to favor the latter position (p. 291), yet at the very end of this discussion he nevertheless appears to retain the distinction between DRS1 and DRS2 (see p. 308-309). The whole discussion is confuse and rather superficial, and the fact that Rückl himself seems unable to choose between his two redactional models does not make things better. Very little scholarly literature is quoted (apart one reference to the—in my view very weak and problematic—book of Wagner (2005) in a footnote (p. 307, n. 886)), which is all the more surprising considering the wealth of studies on the composition of 1-2 Sam that have been published in the last years. Otherwise, Rückl does not confront his model to any of the main models that already exist for the composition of Sam, nor does he make any attempt to indicate what other texts could have belonged to such “Dynastic Redaction(s);” this is all the more regrettable that the texts he assigns to his DSR1 apart from 2 Sam 7:1-17 often play an important role in the discussion on the composition of Sam (in passing, Rückl does not seem to be aware that the literary unity of 1 Sam 13:7b-15a has been questioned by several scholars, and not just by Adam, whose position he briefly mentions on p. 263, n. 750). Nor does Rückl discuss the (basic) question of the relation between 2 Sam 7 and the so-called “History of David’s Rise,” although there exists a considerable body of literature on that topic, especially on the place of chap. 7 within 2 Sam 5-8.

The situation is hardly better regarding the discussion of the relation between Sam and Kgs. The analysis of the passages in Kgs that appear to present a conditional understanding of the promise to David (p. 292ff.) is interesting, but here also there is surprisingly very little engagement with recent scholarly literature on the topic—even with those authors who would actually support parts of the argument developed by Rückl here (e.g., among many other possible examples, Pietsch 2003:45ff. regarding the late origin of 1 Kgs 2:4; 8:25 and 9:4-5)! I have much sympathy for the view that the literary connection between Sam and Kgs was somewhat fluid during the Neo-Babylonian and Persian periods and that some redactions may have sought to connect the two books while others may have treated them as distinct entities, as argued by Rückl toward the end (p. 306-308 especially); however, to substantiate this view would have required a much more detailed treatment than the one offered here, and several issues remain unanswered (and, unfortunately, unaddressed by Rückl): In particular, is it possible to reconstruct an early version of Kgs that did not (yet) presuppose the promise of an everlasting dynasty to David, and what would have been the content of that version? Such questions are heavily debated in the scholarly literature, and it is not possible to simply ignore them. The relationship of the second “Dynastic Redaction” postulated by Rückl to *dtr* is not entirely clear to me; but at some points, Rückl appears to regard it as “*dtr*”, at least in a broad sense of this term (this would also be logically implied, of course, by the possibility he raises that DSR1 and DSR2 are actually one and the same redaction). Yet this seems questionable in light of some of the passages that Rückl assigns to his DSR2, such as especially 2 Sam 22 and

23:1-7, which are not even found in Chronicles (!) and may be actually very late; after all, even Noth had already argued that the so-called “Appendix” in 2 Sam 21-24 was post-dtr, and few scholars have questioned that conclusion since. The idea that 2 Sam 7:1-17 and 18-29 could be of one piece, and that the promise to David in 7:1-17 could belong to the same layer as texts such as 2 Sam 22 and 23, seems also somewhat far-fetched. Rückl does not really consider some of the most basic objections against this assumption, such as, e.g., the fact that the presence of very late linguistic and ideological features—which Rückl does correctly observe—is much more obvious in 2 Sam 7:18-29 than in vv. 1-17; or the fact that texts such as 2 Sam 22 and 23 already seem to presuppose some sort of conceptual (and, presumably, material) separation between the books of Sam and Kgs, since they interrupt the transition from 2 Sam 20 to 1 Kgs 1-2, which does not agree well with Rückl’s view that the redaction responsible for the introduction of 2 Sam 7* was responsible for connecting the books of Sam and Kgs. The position of texts like 2 Sam 22 and 23 makes much more sense if these texts were introduced at a later stage, when Sam and Kgs were already considered as two discrete compositions.

Overall, these remarks (and possibly others as well) suggest that this part of Rückl’s dissertation is in need of extensive revision for the publication, in which the methodological assumptions underlying the suggested redactional model, as well as the redaction-critical implications of that model, will have to be addressed much more thoroughly, and in continuous discussion with the existing literature.

5. If the previous comments suggest that Rückl’s study leaves some room for improvement, it remains, as I mentioned at the onset, a solid, thorough and competent thesis, which represent a significant contribution to the research on the books of Samuel, the theme of the promise to David, and the construction of the discourse on kingship in postmonarchic Yehud. My recommendation is to accept the dissertation, but to invite Jan Rückl to consider the remarks made here when revising the manuscript for publication.



Christophe Nihan
Lausanne, Switzerland
September 6, 2012