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The MARCS Institute for Brain, Behaviour and Development

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To the Habillitation Committee:

This letter presents my evaluation of **Dr. Kamil Kaźmierski**'s application for the degree of *doktor habilitowany* [habilitated doctor] in linguistics. My assessment is based on the following understandings about the candidate: that he received his PhD 8 years ago (2013, University of Vienna), that he has held an *adiunkt* [~Lecturer] position at Adam Mickiewicz University since then[1], and that he has selected a subset of his publications to represent his core research theme, *Phonological implications of variation*, as the evidence of his scholarly quality and independence that I am to evaluate. Please note that I do not know the candidate, and was previously unfamiliar with his work (see next paragraph).

Contextualizing this review

I must preface my review with two important caveats. In light of these caveats, I request that you take the following points into account when considering my evaluation in your deliberations about conferring *habilitation*. Firstly, my PhD training was in developmental cognitive psychology, with a specific specialization in psycholinguistics, and I have gained substantial post-doctoral expertise in phonetics. My theoretical and empirical work primarily address the effects of linguistic experience on first and second language speech perception (and, to a lesser degree, production). In contradistinction, the candidate's training in linguistics, with a specialization in theoretical phonology. His research focuses on phonological and morphophonological aspects of language variation and change, albeit showing an increasing uptake of "laboratory phonology" techniques and logic that take phonetic contributions into account to a minor extent. In other words, I do not share the same major field or linguistic sub-field, nor most of the specific research foci as Dr. Kaźmierski. Nevertheless, we do have overlap in the area of second language speech processes, the relationship between phonetic and phonological processes, and aspects of language variation and their theoretical implications. Moreover, I can certainly offer a well-informed psycholinguist's perspective on his work, given my reading knowledge of phonological theories and familiarity with laboratory phonology, both through my collaborations with sociophoneticians and laboratory phonologists, and in my role as Editor in Chief of *Phonetica*.

Secondly, I've had extensive experience with the diverging academic progression requirements in the USA and Australia, but almost none with the *habilitation* system. Therefore, I have done a modicum of online research (which turned up debates about its pros, cons, and changing EU distribution) to get some sense of it and its practice in Poland (relative to, e.g., Germany, where it has changed from a federal mandate to a regional option). Though I have thereby gained superficial insight into *habilitation*, it is not in depth knowledge – I haven't experienced it and admittedly lack a full grasp of its processes and purposes. Thus, as an international reviewer I will contextualize my review of Dr. Kaźmierski based on my grasp of *habilitation*, by comparison to academic promotions within the US and Australian systems with which I am much more familiar.

[1] He also calls himself a "postdoctoral candidate" in his notes about his contributions to each selected publication. I will interpret this to mean *habilitation doctor* candidate who currently holds an *adiunkt* appointment at the university, and not that he is a postdoctoral fellow in the sense that the term is used in the USA and Australia.

US universities employ the tenure system, for which the most comparable step appears to be the mandatory tenure review. That process is typically conducted at ~7 years post-PhD, based on extensive external and internal assessments to determine whether both tenure (permanent position) and promotion from Assistant to Associate Professor should be granted. The outcome is “up or out” -- if the decision is negative, the candidate’s position at the university is terminated. If it is positive, continuation of their position is protected but there is no new second doctoral degree to bestow, and a second major review is conducted 6-8 years later to determine whether or not full Professor status should be conferred. The comparability to *habilitation* is loose/weak, at best, and the correspondence between an *adiunkt* and an Assistant Professor in the US is notably more distant than the candidate implies. I do not intend this observation as a value judgment, my intention is simply to underline that they are quite different processes and systems, making the correspondence like “comparing apples to oranges.”

The Australian system is, in turn, different from both the US and Polish systems. Australian universities lack tenure, instead hiring academics into a mix of term-limited contracts (1-, 2- or 3-5 years) or “continuing” positions for which the probationary period (usually 2 years) involves a mid-probation review and a final review to determine whether the candidate’s position will continue indefinitely or be terminated. This does not constitute de facto tenure: the continuation decision occurs much earlier, and is much less onerous, but importantly the university can nonetheless end the position at any time by designating it “redundant.” Early stage academics are Lecturers appointed at and/or promoted through three levels: A (junior), B and C (senior). Subsequent Associate Professor and Professor promotions involve more rigorous external/internal reviews, but both are optional, i.e., one can request those promotion reiews at any time or decide to simply remain at Lecturer level indefinitely. Thus, the correspondence to *habilitation* is even looser than with the US system, but the most equivalent step is a continuing decision at level C (not usually linked with a decision about promotion to Associate Professor).

Despite their other differences, the US and Australian systems share several features that differ notably from the Polish *habilitation* system, if I have correctly understood the latter. In the USA and Australia, PhD training itself is expected to have produced an independent researcher and university-level teacher. Although postdoctoral fellowships can and frequently do augment one or both skills, postdoctoral experience is not a prerequisite for obtaining a faculty appointment. A new PhD can be directly hired into an academic position, where they are expected to already function as independent teachers and researchers. While early career academics do receive mentorship from senior colleagues (to varying degrees), at top tier institutions they are expected to quickly establish their own research programs while teaching classes, and they are evaluated negatively if they fail to accomplish both tasks satisfactorily. They usually carry full (or sometimes ¾ or even ½ time) teaching loads in their Assistant Professor or Lecturer roles, for which they are the primary instructor.

In contrast, what I infer from my reading is that a pre-*habilitation* academic appointed as an *adiunkt* is not yet considered or treated as a fully independent researcher or teacher, perhaps more like an apprentice (to one or more Professors) in both arenas. The *habilitation* doctorate thus serves as the formal process that assesses the *adiunkt*’s ability to undertake independent research and teaching, which is officially recognized with a second doctoral degree. This establishes eligibility for appointment to Professor and allows them to supervise PhD students. I could not find clear guidance re: whether *habilitation* actually entails a promotion to Professor, an important issue which could be another fundamental difference from the other two systems. These inferences about the *habilitation* degree are the basis for my evaluation of the candidate’s selected works. If I have misconstrued things, please hold that in mind when considering my evaluation, which follows next.

Review of the selected publications: Phonological implications of variation.

I address the submitted papers in chronological order, as this helps highlight topical and quality trends over time, i.e., evidence of growth as a researcher and an academic author. First, however, I note that Dr. Kaźmierski published his PhD thesis in 2015 as a monograph with De Gruyter Mouton, an internationally respected publisher in linguistics, but did not include it in his selected works for *habilitation.* I infer it was omitted largely because the contents had already served as the basis for the PhD degree. In the other two systems, such a monograph would be included in the roughly corresponding evaluation step in linguistics, where solo books "count" more heavily than a journal article, but its value would be down-weighted for the same reason it was left out of the *habilitation* corpus.

The earliest article in the corpus is Kaźmierski (2015) *Exaptation and phonological change,* a primarily theoretical analysis that extends the thesis of the PhD dissertation to consider how the evolutionary principle of exaptation may apply to historical shifts in the long and short vowel subsystems from Old to Middle to Early Modern English. In it, the likelihood and value of exaptation as a viable account of language change (in this case historical vowel shifts) is considered first as a superficial metaphoric extension of a biological concept, and then as a strictly evolutionary concept that applies directly to language as an evolving system. The metaphorical interpretation was found to be logically wanting, adding nothing to understanding the underlying causes of the vowel shifts beyond the adequately explanatory and non-biological concept of phonologization. The strict evolutionary analysis, however, is deemed to offer novel insights into the historical changes in English vowels, and suggests that language does indeed function like a biological system evolving over time according to the same basic principles. The core idea is an extension of Lass's introduction of evolutionary concepts to linguistics, i.e., not original, but the comparative analysis of metaphoric versus strict evolutionary application to phonological change appears to be novel and insightful, as far as my knowledge of phonological theory goes. It is a solo paper, i.e., entirely the candidate's work, which is a plus for a first post-PhD journal article.

On the other hand, the argumentation could have been tighter and clearer and the supporting examples more transparent, e.g., to effectively illustrate the vowel change principles being argued, the example words should have been consistent across tables 1-4. Worryingly, in addition, there were a number of errors in details that are central to the paper's core thesis, e.g., in (1) it is the final rather than the middle syllable that acquires secondary prominence; in Table 4 some vowel pairs do not align with the entries in earlier tables. The most glaring issue is the repeated citation, both in the text and in figure captions, of Lass 1999, which serves as probably the most central source for this paper. There is no Lass 1999 in the literature, and certainly not in the volume presented in the reference citation. The correct reference is almost certainly Lass 1992; the 1999 entry in the reference list is a garbled version of the correct Lass 1992 reference, which is also listed there. These errors are concerning in that they reflect poor attention to details that are fundamental to the core thesis of the paper. They should have been caught and corrected before the paper went to press (NB: this also implies that neither the candidate's mentors nor the journal's editorial staff and reviewers caught the problems either). Also, the journal is not top-tier -- it has a 2nd quartile ranking in Language and Linguistics (68%) and modest impact (IF .463), which may be contributing factors to the paper's low citation rate (1). But it has a fairly high weighting for the evaluation (140 points).

It is important to note that such carelessness with details has largely dissipated in subsequent papers. But still, none of the outlets for the next three articles (all 2016) was top-tier; one does even have a rank in the field. For career progression, strategically one should aim for increasingly more top quartile journals, which also increase the visibility and impact of one's contributions to the field. That better strategy does, in fact, does appear with the more recent papers, a positive sign.

The candidate's evolutionary theoretical framework continues to be utilized and built upon in the next five papers (2016-2019), nicely reflecting the coherent theme designated for the selected *habitation* evaluation corpus. They provide a systematic and progressive evaluation of a core theoretical issue: the role of morphology-phonology relationships in language change. They all rely on the following principles that Kaźmierski and his collaborators have extrapolated from the Dressler and Dziubalska-Kołaczyk (2006) [henceforth D&D-K06] theory of morphonotactics: 1) that although consonant clusters are phonologically dispreferred across the world's languages they nonetheless arise in some languages, posited to be due to their informativity about morphemic composition of words (semiotic utility), and 2) that the distribution of specific clusters in a language across lexical (mono-morphemic) vs. morphotactic (bi-morphemic) words, i.e., their "lexicality", shapes their evolution, stability and use in that language. As far as I understand, this is an original interpretation of the theory, which Kaźmierski and his collaborators systematically test.

Baumann and Kaźmierski (2016), *A dynamical-systems approach to the evolution of morphonotactic and lexical consonant clusters in English and Polish*, apply contemporary laboratory phonology modelling techniques to existing corpora to estimate and evaluate whether these principles are supported diachronically and synchronically, respectively, in English and Polish. They compare the modelling outputs to the actual distributions of the clusters in the corpora of these two languages, which differ in their tendencies to express grammatical functions via inflectional morphology (synthetic languages, e.g., Polish) versus analytical functions (analytic languages, e.g., English), to evaluate two competing hypotheses: 1) their extension of the D&D-K06 Semiotic Utility Function, in which they argue that the density distribution of a cluster throughout the lexicon of a language should be V-shaped across the purely lexical to purely morphonotactic continuum for synthetic languages (Polish) but ^-shaped for analytic languages (English); 2) the analogy-based hypothesis is that there is a bidirectional mutual relationship between morphonotactic and lexical clusters in a language which should result instead in a ^-shaped distribution for both types of language. Their models for Polish are globally consistent with their Semiotic Utility hypothesis (H1) except that the estimated distributions are clearly very U-shaped rather than V-shaped. This, in my view, weakens their argument for a *graded* effect of Semiotic Utility along the lexicality continuum, which instead is clearly a highly *categorical* function, i.e., very high preference for the pure lexical and pure morphonotactic cases and very strong dispreference for *any* mix of lexical and morphonotactic occurrences. The English case is even more problematic, in that an ^-shaped density distribution is predicted from both H1 and H2, i.e., cannot differentiate between them, and in that the modelling for English actually shows a very similar distribution to the Polish models with only a very small increase in density near the middle of the continuum that the authors call a W-shaped distribution. The latter is a wild overstatement, and there does not appear to have been any attempt to statistically compare the Polish and English distributions to assess whether they differ significantly, nor to assess whether the shape of either set of distributions differs significantly from the much more apparent U shape. Their interpretation of the findings is essentially that the Polish distributions/models adhere to H1, while the English ones instead reflect a mixed influence of H1 and H2, seems superficially logical. However, the paper does not address (nor acknowledge) the apparent (predominantly) U shape of distributions for both languages and its theoretical implications. Thus, it fails to consider the possibility that the findings may actually better support the original D&D-K06 Semiotic Utility Function, which they say does not distinguish among varied possible density distributions along a lexicality continuum, and which they imply does not distinguish between morphologically synthetic and analytic languages. The essentially U-shaped function for both languages suggests that Semiotic Utility is more nearly categorical and is also relatively impervious to the morphological type difference between these languages (possibly because of its categoricity).

I note as well that the candidate was not the lead author on the above paper. The publication outlet was even less optimal, a conference proceeding published in a series that is not indexed for impact or ranking in the field. Perhaps this is why it has a very low weighting for the evaluation (20 points). Nonetheless, it is accessible online, and has been cited 6 times (though in all cases by the authors or Dressler, a founder of the D&D-K06 morphonotactic theory).

The candidate is the lead author on the next paper, Kaźmierski, Wojtkowiak and Baumann (2016), *Coalescent assimilation across word boundaries in American English and in Polish English*. It is a peer-reviewed article in Research in Language, which at least has a 2nd quartile (56%) ranking in Language and Linguistics, but was given a fairly low weighting for this assessment (40 points). It presents an interesting and competent theoretically-motivated analysis of native-speaker allophonic realizations of the English assimilation process by which word-final alveolar obstruents /t, d, s, z/ merge with a following word-initial /j/ to become postalveolar /tʃ, dʒ, ʃ, ʒ/, applying a multiple modelling approach to existing conversational (casual) speech corpora of American English as compared to Polish L2-English. All four English models showed that coalescent assimilation is less likely when there is a syntactic boundary between the two words, and more likely if the second word is *you*. However, deletion rather than coalescence of the alveolar obstruent is likely when the first word ends in a consonant cluster. The first two patterns were observed as well in the Polish English data, but the final-cluster alveolar obstruent deletion effect was not. The Polish speakers avoided coalescence in that context, instead producing a full release of the alveolar obstruent. These findings are generally compatible with the authors' expectations, who go on in the Conclusions to discuss the implications of the (Polish vs. English) findings for teaching second language pronunciation. They argue that a describing the actual allophonic behavior of native speakers more accurately in textbook accounts could benefit L2 learners' acquisition of more native-like production patterns. It is commendable to see the practical implications of such research considered in this report.

The Ritt and Kaźmierski (2016) paper, *How rarities like* gold *came to exist: On co-evolutionary interactions between morphology and lexical phonotactics*, appears in a journal the just makes it into top quartile ranking (76%), another upward movement in outlet quality, and is weighted moderately heavily for the *habilitation* evaluation (70 points). The paper addresses the evolutionary hypotheses laid out in the three preceding papers, in a historical analysis of how cross-linguistically rare VVCC rhymes emerged in English. It is, in part, an expansion on the historical analysis of English long-short vowel shifts presented in Kazmierski (2015). The authors draw several related inferences from their historical analyses: 1) long vowels were retained, despite constraints against lexical VVCC, in intermediate forms of modern *friend* (now a short vowel) and *fiend* (still long) as they became partially lexicalized participles that were recognized as transparently related to the Old English verbs *frēogean* and *fēogean*; 2) homorganic lengthening in words like *child* coincided with schwa loss in highly productive morphological operations, making word-final [VVnd] and [VVld] frequent surface forms that facilitated not only their own recognition, but also that of lexical (non-morphological) forms, which thus came to be interpreted as expressing lexical underliers; 3) those shifts created a systemic gap in lexical phonotactics by establishing the "difficult" (dispreferred) rhyme types VV/nd/ and VV/ld/ before their more easily recognisable and transmittable lexical counterparts VV/nt/, VV/ns/, VV/lt, /VV/ls/ appeared, and that gap then filled. Thus they conclude that dispreferred sequences that frequently occur as surface word forms of morphological operations can emerge in lexical items even without lexicalisation processes, and that interactions of constituents from different levels of linguistic organisation can stabilise sound patterns that are otherwise dispreferred on the basis of their phonetic transmissibility or semiotic functionality. This is a theoretically interesting, careful and thoughtful theoretical and historical analysis of how typologically rare phonological properties can emerge in the lexicon of a language.

I see the candidate is not listed as the lead author of the paper, despite contributing 70% effort including preparation of the manuscript (30% attributed to the first author, who is an established Professor), and the fact that the study is designed around the evolutionary theoretic principles he presented in the preceding three papers (and presumably in his PhD dissertation/monograph). These circumstances would likely have been reflected in first authorship in the USA or Australia.

The fifth paper, Baumann and Kaźmierski (2019; note that this is the correct publication year, not 2018 as listed in the application) *Assessing the effect of ambiguity in compositionality signaling on the processing of diphones*, picks up the reins from these co-authors' 2016 paper to continue their investigation of how language processes are affected by variations in how consistently consonantal diphone clusters signal morphological complexity, i.e., in their density distribution along the lexicality continuum. This time the collaborators apply psycholinguistic methods rather than statistical modelling and corpus/historical analysis, specifically testing perceptual processing speed in two discrimination studies with clusters that differ in lexicality. The results indicate that processing speed is slowed down with increasing ambiguity of cross-morpheme diphones, i.e., as the middle of their lexicality continuum is approached, but speed of decision does not differ between primarily cross-morphemic versus morpheme-internal diphones, i.e., those toward either end of the continuum. Thus, processing speed is a convex function of lexical probability. They interpret these results as only partially supporting the Strong Morphonotactic Hypothesis that they had they extrapolated from the D&D-K06 Morphonotactic theory in their 2016 paper. They conclude that when parsing consonant diphones listeners rely on detailed memories of previously encountered diphone instances, including whether or not they spanned a morpheme boundary; that ambiguity of a diphone is determined by token rather than type frequency; and that the discrepancies from the predicted results suggest that ambiguity about the morphological complexity of the item leads listeners to classify it as the more prototypical case, i.e., as morpheme internal. This is a well-designed study, using a converging method to address their hypothesis. The hypothesis was once again only partially supported but they provide a thoughtful, solid logical analysis of the theoretical implications of their results.

The critical points that I would make are that the organization of the background could have been better structured, and that the original concepts drawn into their hypotheses should have been attributed to the originating sources (e.g., D&D-K06, D&D-K10) by citation from the start of the paper, including in the abstract and early pages. It is also rather unfortunate that they jettisoned the accuracy results in both experiments based on an inappropriate approach to data processing and analysis. They should have applied one of the Signal Detection Theory transformations of the hits and false alarms in the raw data, such as d' or A' etc. (see Verde, Macmillan, & Rotello, 2006, Perception & Psychophysics), which yields a demonstrably more sensitive index of accuracy by controlling for response bias than raw data. It also avoids the statistically problematic use of SAME vs DIFFERENT as a factor in the analysis. Additional insights may well have been gained from analysing such transformed accuracy data, even though performance overall fell near ceiling. Nonetheless, the discrimination results and the discussion of them is very good and makes a novel theoretically-driven contribution.

I note with some disappointment that this and the 2016 paper are both led by Baumann rather than Kaźmierski, making it difficult to attribute the primary theoretical and empirical contribution to the candidate. The ordering of authorship, by convention, implies that Baumann made the primary conceptual and writing contributions. On the plus side, the journal choice is excellent, sitting quite squarely in the top quartile in ranking (86%) within Language and Linguistics. This reflects a good rising trajectory in quality of the venue and reach of the article. Accordingly, it is weighted more heavily for this assessment (100 points).

Kaźmierski, Kul and Zydorowicz (2019), *Educated Poznan speech 30 years later,* is a corpus-based examination of how key phonological/phonetic features of Polish speech produced by educated speakers in current-day Poznań differ from the representation of those features in corpus collected 30 years prior by a predecessor. The candidate is the lead author and originator of the study, which is well designed and extensive, and there appears to be an adequate sample of speakers and tokens at each time period. As the authors report, based on their extensive analyses of this recent corpus in comparison to the previous one, the results indicate a number of changes in educated Poznań speech over the 30 year span that point towards dialect levelling. The report thus provides a useful documentation of shifts in the speech of this one language community. But puzzlingly, it is never made clear who developed the recent corpus; no reference is cited, only a URL is provided, and the web site does not provide much information about its development. The only hint is a footnote on the first page acknowledging funding support for the corpus, which implies that the authors were the corpus developers. It is also disappointing that the actual research they report in the paper is self-described as "preliminary" -- no final version appears to have been published. In addition, the presentation does not seem to go much beyond describing and documenting this single community's speech shifts as examples of types of dialect change, rather than providing a deeper/broader examination of the potential theoretical implications of the findings re: evaluating core/universal principles of dialect change. The paper appears in a barely 2nd quartile journal (51% in Language and Linguistics) and is weighted only 50 points for this evaluation.

In sharp and refreshing contrast, the final three papers in the selected series are very good indeed, all of them in clearly top-tier journals. They also display further expansion of methods that are now applied to a wider range of issues. These observations all represent a strong upward trajectory in the most recent publications. Baumann, Kaźmierski and Matzinger (2020), *Scaling laws for phonotactic complexity in spoken English language data*, examines whether and to what extent two well-known mathematical scaling laws, Zipf's and Heaps' laws, apply to phonotactic sequences of consonants in English. Compatibly with Kaźmierski and colleagues' previous papers, they find that the laws do apply well but only if cross-morphemic sequences are included in the statistical models, suggesting that languages and their speakers make use of not only phonotactic (lexical, within-morpheme) information but also morphotactic information in utterances. Thus, the phonotactic system is governed by similar laws of self-organization as other complex systems both within and outside of linguistics. This paper provides a well-designed and executed,novel contribution to the literature. It appears in one of the top journals in spoken language research, Language and Speech (89%ile in Language and Linguistics), and is more heavily weighted for the evaluation (140 points).

Schwartz and Kaźmierski (2020) *Vowel dynamics in the acquisition of L2 English – An acoustic study of L1 Polish learners*, is another excellent paper reporting an interesting and well-designed study of Polish L2-English learners' acquisition of English-like vowel formant dynamics, which differ from both the formant frequencies and dynamics of the most closely related vowels in Polish. The results indicate that as learners progress in acquiring English, the degree and temporal location of formant movement in their English vowels shifts more toward the dynamics found in native English (Southern British English). This paper is published in another top journal, Language Acquisition (89%ile in Language and Linguistics) and is even more heavily weighted in this evaluation (200 points).

Finally, we come full circle to a second solo-authored journal article, Kaźmierski (2020), *Prevocalic t-glottaling across word boundaries in Midland American English*. This reports a careful, thorough and well-conceived study of surface phonetic glottal realizations of /t/ in prevocalic and preconsonantal positions within words and across word boundaries, in a well-known corpus of Midland American English speech. Kaźmierski argues convincingly that the contextual and socio-indexical effects he finds on t-glottaling are consistent with hybrid models of phonological storage and processing, in which both abstract and detailed exemplar representations are employed. This paper appears in another top-tier journal of even slight higher ranking (91%ile in Language and Linguistics), Laboratory Phonology. The paper has a weighting of 140 points.

<u>*Comparative evaluation*</u>
My evaluation focuses primarily on Dr. Kaźmierski's scholarship as evidenced by the quality and number of research publications. However, in addition there are other signs of maturing scholarship and independence. Dr. Kaźmierski has had some grant success, and has forged a number of international collaborations/connections including conducting a research project while on a Fulbright research fellowship at University of Massachusetts/Amherst and establishing a productive ongoing collaboration with a colleague at University of Aarhus in Denmark.

As for his teaching and professional service contributions, they are difficult for me to assess as there is little information listed about them (e.g., there are no student evaluation reports, as there would be for such a promotion case in the USA and Australia). I can say, though, that both appear to be notably more limited in number and scope than they would be for a candidate at the most comparable s of evaluation in the USA and Australia. Substantially more teaching and departmental service are expected in those two systems at the roughly comparable stages (if I have inferred the correspondences correctly).

For what it's worth, if I compare Dr. Kaźmierski's record to those two other systems that I understand better, it would very likely satisfy the requirements for confirmation of continuing appointment in Australia, as well as promotion to the next step within the Lecturer level, though not to Associate Professor. Some concerns would likely be raised in an Australian university, though, about the dearth of first-authored publications, the inconsistent quality of the papers and the chosen publication outlets, and the apparently light teaching load. As for the system in the USA, his record of publications and teaching would probably not meet approval for the award of tenure and promotion to Associate Professor, at least at first-tier universities. More first-authored publications of consistently high quality and mostly in top-quartile journals would be expected, along with more substantial independent teaching and positive student evaluations.

With respect to *habilitation* in the Polish system, nonetheless, to the extent that I have understood the role of an *adiunkt* in the Polish university system and the expectations for *habilitatio*, I believe Dr. Kaźmierski clearly does meet *habilitation* standards, particularly in light of his most recent set of high-quality papers and his other signs of maturing scholarship.

I hope these comments are of use, and that my contextualization of them is helpful in your deliberations.

Sincerely,

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