More on Japanese /r/

A Response to Pellard (2016)

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Abstract

This paper is a response to “Why /r/ is not a special, empty consonant in Japanese” by Thomas Pellard, published in the JEAL 25 (2016). It addresses the major criticisms raised in Pellard (2016), providing new evidence or alternative analyses on the following questions: distribution pattern and frequency of /r/ and /'/ at various stages of the history of Japanese, comparison of /t/ with /h/ and /b, d, g, z/, typological evidence pertaining to word, stem or root initial liquid prohibition in the world languages, role of /t/ in the verbal morphology, difference between derivational and compositional environment for /t/ epenthesis, cross-linguistic evidence on /t/-epenthesis, the fallacy of positing a feature “liquid” as a phonological feature, and several other phonological, morphological or formal issues. This paper also discusses two novel issues. The first one concerns the dating of the emergence of /t/ in Japanese. It is proposed that /t/ developed before the split between Japanese and Ryukyuan, before the 3rd c. CE. The second issue is whether some so-called “Yamato” Japanese /t/’s may have an external source. While admitting that some /t/’s may be the reflexes of prehistorical loans from Korean or other languages which would have occurred after the phonologization process of /t/ within Japanese proper began, this paper defends the position that since Japanese has not been convincingly demonstrated to be genetically related to any language other than Ryukyuan, it is safer to adopt a strictly internal approach for the investigation on the nature and status of /t/ in Japanese. Explaining some of the characteristics of Japanese /t/ through a tentative Altaic origin should thus appear premature.
1. Introduction

Thomas Pellard’s article “Why /r/ is not a special, empty consonant in Japanese” offers a critical rejoinder to my paper “The phonology of Japanese /r/: a panchronic account,” which appeared in the *Journal of East Asian Linguistics* in 2014. Pellard alleges that my paper is based on a “logical fallacy” (P 353), makes “opposite claims one after the other” (P 356), provides “overstated”, “inaccurate” or “wrong” evidence (P 352), and resorts to a methodology and argumentation that suffer from “limitations” and “inadequacies” (P 352) or to “unfalsifiable tautology” (P 356). Some of my arguments are said to be “logically flawed”, or my analysis is “flawed in several aspects” (P 373, 375), and I also allegedly “argue at the same time that /r/ is epenthetic but also not epenthetic” (P 373).

In this response I show that Pellard’s argumentation suffers from a variety of biases and errors. His allegations are based on an incorrect representation of my ideas, distortion and oversimplification of my arguments, as well as misunderstandings of theories and data. They also contain incorrect or unsubstantiated claims as well as irrelevant or even misleading data, thus failing to provide a fair and constructive criticism.

The paper begins with a restatement of the basic hypothesis of Labrune (2014) dubbed Hr. This is followed by a discussion of the major criticisms raised in Pellard’s paper, which will

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1 References to Pellard’s paper are indicated with P, followed by the relevant page number. References to my 2014 paper are made through L. So (P 351) should be understood as Pellard (2016:351).
be addressed in turn. 2 A couple of new issues, not discussed in either my original paper or in Pellard’s, are also considered.

2. The Hr

The key-points defining the Hr as developed in my 2014 paper can be restated as follows. First, /r/ behaves as a special consonant in the Japanese phonological system which motivates analyzing it as a default, unmarked consonant. Second, its present-day characteristics reflect the history of its relatively late development in Proto-Japanese as a default epenthetic consonant in intervocalic position.

The starting point of my research on /r/ in Japanese was originally an attempt to synthesize the results of two different studies which had independently addressed, each with its own method and approach, the special status of this consonant. The first was Kuginuki Tooru’s (1982) paper, “Jôdai Nihongo ra-gyôon-kô” [remarks on r in archaic Japanese] which was grounded on a thorough philological study of OJ texts and materials, and proposed that /r/ originally developed relatively late in the history of Japanese. The second paper, Armin Mester and Junko Itô’s (1989) “Feature predictability and underspecification: palatal prosody in Japanese mimetics”, was a theory-oriented paper which argued for the unmarked and underspecified nature of (modern) Japanese /r/ within the framework of underspecification theory. I wondered whether these two analyses could be combined and, in some way, complement each other in order to obtain a more comprehensive account of /r/’s particularities in present day Japanese. I thus started to collect and investigate the phonetic, phonological, and morphological characteristics of /r/ in order to achieve a bird’s eye view of

2 Another, briefer response to my paper was also written by Shigeto Kawahara (Kawahara, 2015). A number of Kawahara’s arguments are taken up in Pellard’s paper, so some passages of the current reply can be understood as an indirect response to Kawahara (2015).
the status of this consonant in Japanese. The core of my argument thus comes down to a synthesis of these two studies, namely that /r/ primarily developed as an epenthetic consonant in intervocalic position in Proto-Japanese (Tooru Kuginuki’s idea), through default feature filling (Armin Mester and Junko Itô’s idea) before acquiring full phonemic status. The various characteristics of Modern Japanese /r/, I argue, reflect this emergence process. The so-called Hir thus constitutes a powerful explanatory principle which makes it possible to relate the numerous seemingly independent peculiar properties of Modern Japanese /r/.

3. Distribution pattern and frequency

One of the main arguments of the Hir rests on the observation that /r/ is overwhelmingly frequent word-initially in Japanese but absent word-externally, while being in complementary distribution with /'/ (the zero consonant or zero onset) in both positions. Another important observation, originally made by Kuginuki (1982), is that /r/ is more likely to occur late in words. Addressing these two issues, Pellard takes painstaking efforts to demonstrate that /r/ is not the most frequent consonant word-externally in Yamato words (sections 3 and 4 of P). In his opinion, this fact would suffice to seriously undermine the analysis that /r/ developed in Japanese as a default, unmarked consonant. However, Pellard actually offers no general contradicting conclusion on the frequency issue which would invalidate the Kuginuki (1982), Kokuritsu Kokugo Kenkyûjo (1984), and Irie (1996) evidence, on which the Hir is based. His discussion is flawed with misinterpretations and selective use of the evidence.

First, Pellard casts doubts on the reliability of Kuginuki’s data. However, I want to insist that I see no reason to believe that Kuginuki’s study was not based on a coherent and honest scholarly approach.
In his discussion of the frequencies, Pellard relies on two novel recent sources, Irie (2012) and Miyajima et al. (2014). He also applies new statistical tests to some of the data. What do these new results tell us? They tell us that:

a) /ʔ/ (the zero consonant) is the most frequent initial in Yamato nouns (Irie 2012:22, 37, 62, 82)

b) /ɾ/ is the most frequent word-internal consonant in two-mora Yamato nouns (Pellard Fig.2, based on Kokuritsu Kokugo Kenkyûjo 1984)

c) /ɾ/ is the second most frequent word-internal consonant in three-mora Yamato nouns (Pellard Fig. 1, based on Irie 1996)

d) /ɾ/ is the second most frequent word-internal consonant in Yamato nouns of any length, including compounds (Pellard Fig. 3, based on Irie 2012).

e) /ɾ/ is the most frequent consonant in the final mora of Yamato nouns of any length and all periods except in the Man’yōshû corpus where it ranks second after /m/. (Irie 2012).

It is important to note that the Irie (2012) and Miyajima et al. (2014) data used by Pellard also include compounds, whereas my study is concerned only with non-compounds, so the two sets of data are not directly comparable as Pellard incorrectly assumes. The word-internal

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3 /ɾ/ represents 13.8% of all consonants in the final mora of Yamato nouns in the Man’yōshû (Irie 2012:20), 17.1% in the Genji Monogatari (Irie 2012: 37), 17.8% in the Nippo Jisho and 20% in the Shinchô Dictionary (Irie 2012:83).

4 Nouns exceeding three moras in Yamato Japanese are likely to be compounds, which creates a bias against /ɾ/ when compounds are included in the samples since /ɾ/ is never going to occur at a nominal compound juncture, unlike most other consonants.
/r/ frequencies that Pellard relies on based on Irie (2012) and Miyajima et al. (2014) are likely to be under-estimated due to the presence of compounds. As a complement to this set of data, I would like to bring into the discussion the Sakakura data (Sakakura 1968:280) which, unlike the Miyajima et al. (2014) or Irie (2012) data, is based on a corpus of 992 words containing all non-compound Yamato nouns occurring in the Man’yōshū. According to Sakakura, /r/ ranks first as the final mora onset (19.5%, n=194) before /k/ (17.2%, n=171), /l/ (14.4%, n=143), /h/ (14.3%, n=142), /m/ (12.8%, n=127), /s/ 12.6 %, /n/125, /n/ 5.3%, /y/1.8 %, /w/ 1.8%, n=18, /'/ 0.1%, n =1. Total n = 992). Sakakura does not distinguish voiced and unvoiced obstruents, so that the actual figures for /k/, /l/, /s/ and /h/ are presumably lower than indicated, but this has no real bearing on the global ranking.

Be that as it may, it is not the absolute figures and ranking of each consonant which are at stake in my analysis, but the discrepancy between their word-initial frequency and their word-internal one. The Hr does not crucially depend on /r/ being the most frequent. Rather, it is the disparity in distribution that is critical: /r/ is virtually absent word initially, whereas it is overwhelmingly frequent in the other positions. /r/ being the first, second or even third most frequent consonant does not really matter. The new data adduced by Pellard do not invalidate the claim that /r/ “is the most frequent, or one of the most frequent of all [consonants], in intervocalic position” (L 6), and that “/’/ happens to be the most frequent type of onset in the word-initial position” (L 6). Pellard fails to understand that the fact that /k/ could putatively rank as the most frequent consonant word internally is not really significant, because /k/ is also extremely frequent word-initially (it ranks second after /'/). Similarly, the fact that /z/ is rare (and actually absent in OJ) word-initially, is not really relevant because /z/ also has a very low frequency word internally. It is the correlation between /'/ and /r/’s unusually high
frequencies in the positions in which they appear within words, and the fact that their
distribution is complementary\(^5\), that demands our attention.

Pellard also asserts: “[t]he frequency of word-internal /t/ seems to increase with time, but
this contradicts the Hr’s view that r-epenthes was a pre-OJ phenomenon” (P 361). I do not
see any contradiction here. First, we have to bear in mind that the data under discussion here
concern only Yamato nouns. The language sample for Modern Japanese (MJ) that Irie (2012)
uses has been extracted from the *Shinchô Gendai Kokugo Jiten* (2\(^{\text{nd}}\) edition, 2000), and
contains as many as 13,015 Yamato nouns, whereas her OJ data, consisting of all Yamato
nouns occurring in the *Man’yôshû*, sums to 3,310 nouns. The EMJ data (*Genji Monogatari*)
and the LMJ data (*Nippo Jisho / Vocabulario da lingua do Japao.*) reach a total of
respectively 3,570 and 8,811 nouns. Clearly, the further one goes back in time, the smaller the
lexicon available to us. This is because our sources are more limited both in number and in
type: for OJ, only *Man’yôshû* poems are used in Irie (2012), for EMJ, the sample comes from
one literary work, the *Genji Monogatari*, written by a single author, Murasaki Shikibu, a
woman of the Kyoto aristocracy. In contrast, the two most recent sources used by Irie (2012),
the *Nippo Jisho* and the *Shinchô Gendai Kokugo Jiten*, are dictionaries. To the extent that as

\(^5\) As for the comment that /’/ and /t/ should not be considered as being in complementary
distribution (P 354), the argument is not only false, it is also totally trifling. /’/ and /t/ are in
complementary distribution because they never occur in the same phonological context. The
fact that /’/ is also in complementary distribution with other phonemes changes nothing to this
state of affairs. From a structuralist point of view, which is the one adopted for my analysis,
the issue is whether this complementary distribution should lead us to consider that these two
elements should be assigned to the same phoneme or not, on the basis of acoustic or articulatory
similarity. It is only on this latter point that interpretations can diverge.
far as /t/ is concerned, native nouns have not been affected by any significant phonological change since the OJ period, one should consider, contra Pellard, that statistical evidence based on data extracted from the Nippo Jisho and the Shinchô Gendai Kokugo Jiten are more likely to provide representative and statistically reliable data. This is because, first, the number of Yamato nouns included in these two dictionaries is significantly higher than that of the Man’yōshū and the Genji, and second, the language samples they provide are more comprehensive and balanced since they cover more diaphasic and diastratic varieties.

On page 359, Pellard writes that “[a]s for three-mora words, no statement that /t/ is the most frequent consonant can be found in the source (Irie 1996) quoted by the Hr (Labrune 2014:4)”. This observation is correct. The fact established by Irie (1996) is that /t/ is the most frequent consonant in the final mora of Yamato three-mora nouns, whereas it is the most frequent consonant in the final mora of Yamato two-mora nouns according to Kokuritsu Kokugo Kenkyûjo (1984:25). What we learn from Irie (1996) is that /t/ is the second most frequent consonant after /k/ word internally in three-mora long Yamato nouns, including compounds. Concerning the important issue of /t/’s frequency in the final mora of Yamato nouns, one would have expected Pellard to mention Irie’s (2012) frequency results in a comprehensive manner, since they undeniably demonstrate that my claim about /t/’s high frequency in that position receives full validation. In the surveys conducted by Irie on all Yamato nouns occurring in the Genji Monogatari (Irie 2012: 37), in the Nippo Jisho (Irie 2012: 63), and in the Shinchô Gendai Kokugo Jiten (Irie 2012: 82), /t/ ranks first as the onset of the final mora of Yamato nouns of all lengths. In the Man’yōshū survey (Irie 2012: 20), /t/ ranks second after /m/. But Pellard just chooses to ignore these data.

Similarly, after having discussed at length the MJ data provided by Irie (2012) for the word-internal frequencies in Yamato nouns of all lengths in MJ, Pellard writes (P 360) that “it is also interesting to examine OJ to [sic]/[…]/”. For this purpose, he uses Miyajima et al.
(2014). Why doesn’t he go on using Irie (2012), who provides very rich and precise data on OJ, and whose work he has been continuously using to make his point on MJ? In the Miyajima et al. (2014) data, /t/ turns out to be the fifth most frequent consonant. But had Pellard also used the Irie data, /t/ would have turned out to be the most frequent consonant in the *Genji Monogatari* corpus, and the second most frequent in the *Man’yōshū* corpus. By ignoring the Irie data and emphasizing the Miyajima and al. ones, Pellard misleads readers into believing that Irie (2012) deals only with MJ and that Miyajima et al. (2014) is the only available source for investigating OJ phoneme frequencies. Here, I believe that in order to prevent any suspicion of cherry-picking the evidence, Irie’s data should have been discussed alongside Miyajima et al. (2014)’s, and even more so since her data have been used by Pellard in his discussion on Middle and Modern Japanese, thus allowing a more homogeneous picture of Japanese phonemic frequencies throughout different sources and periods of Japanese history.

Concerning the co-occurrence restrictions that I discussed in my paper, stating, on the basis of Kuginuki’s account, that two /r/’s cannot occur twice in a stem, especially a verb stem, Pellard brings forth a number of counter-examples (P 356). However, it is necessary to point out that *arapare-*-, *karamar-*-, and *muragar-* are not stems in the sense of Kuginuki. The stems of these verbs (what other scholars like Martin 1987 would call “roots”) would be *arap-*-, *karam-* and *murag-*-. Furthermore, four of the alleged counter-examples given in (1) (P 356) are not OJ forms, which is why Kuginuki did not mention them, his study being focused on OJ. These forms should be considered as having developed at a later stage. There are actually only two true counter-examples: *hiroru* ‘to spread’ (cited as *piro2r* in Pellard) and *arare* ‘hail’. *Hiroru* is actually cited by Kuginuki as the unique OJ counter-example to the co-occurrence restriction, while I have suggested in another paper (Labrune 1998) that *arare* could be analyzed as containing the frequent -*re* suffix (the stem of this word is then *ara*-).
This leaves us with one, possibly two, counter-examples. Of course, exceptions should be reported and, if possible, explained, but one has to be careful not to ignore the forest for the trees. Abstracting away from data that can be considered exceptions is basic to linguistic analysis. I actually know very few linguistic phenomena with absolutely no counter-examples, so if their existence was enough to dismiss assumptions in linguistics, our field would remain singularly poor in hypotheses.

Pellard also wonders (P 356) why examples such as the mimetics rerorero ‘grumbling’ and rorirori ‘restless’ are not more numerous. This is a good question, which was already brought to my attention by an anonymous reviewer and mentioned as such in footnote 10 of my paper. All I can say is that whether one adheres to the Hr or not, this issue would have to be raised since rerorero and rorirori stand out as exceptions to two well-known structural phonotactic constraints: the bans on initial /t/ and on identical consonants in mimetic roots.

To conclude on the frequency and distribution issue, it turns out that Pellard does not provide any seriously contradicting statistical results, even though he claims to do so. The new data adduced by Pellard converge with mine, allowing us to validate the statement that “/r/ is the most frequent or one of the most frequent consonants word-internally”, while is it “absent or rare word initially”. And conversely, that ‘/’ exhibits the opposite pattern.

4. The comparison with other Japanese consonants

A recurrent criticism in Pellard’s paper is that the properties which are held to be unique to /t/ actually are not, because they are also characteristic of other Japanese consonants. The first of these consonants is arguably /h/ (P 353, 364) because /h/ has more contextual allophones than /t/ in Modern Japanese, and also because it cannot undergo gemination. These are well-known facts. But the reasons why it should be so are rather clear and uncontroversial in

Second, the voiced obstruents /b/, /d/, /z/, /g/, Pellard argues, also share their distributional asymmetries and their co-occurrence restrictions with /r/ (P 353, 356-357, 364, 374) since they do not occur at the beginning of Yamato words and cannot occur twice in a root. They also resist gemination. This is also correct and well established. However, here again, although the issue is a complicated one, there exists an uncontroversial historical explanation which accounts for the special status of medial voiced obstruents and their rarity in word-initial position in modern Yamato Japanese words: they are known to be of secondary development in Japanese (Martin 1987:20-31, Frellesvig 2010:43).

While explanations of the distributional asymmetries, frequency data and allophonic variation are available for /h/ and the voiced obstruents, no such account is available for /r/. The analytical merit of the Hr is precisely that it provides an interpretation for such otherwise unaccounted facts.

Concerning the voiced obstruents again: since a number of scholars including myself (Komatsu 1981, Itô and Mester 2003, Labrune 2012) have characterized them as marked, basing this appreciation on a number of properties among which some are actually also displayed by /r/, Pellard accuses me of “logical fallacy, both circular and self-contradicting” (P 353-354). But here again Pellard oversimplifies and distorts my analyses. Voiced obstruents display other characteristics which they do not share with /r/, notably their extremely low frequency word internally. Moreover, the reasons for their idiosyncratic status find a straightforward explanation in the fact, already mentioned above, that they result from a secondary development.

5. The (un)markedness issue
Pellard writes (P 353) that « [t]he distribution pattern of /r/ in Japanese is noteworthy for its well-known restrictions. Though the Hr interprets these constraints as evidence for the unique unmarked status of /r/, distributional restrictions are usually considered to target marked segments rather than unmarked ones […] so that in this case the data would suggest to the contrary that /r/ is marked in Japanese”.

Certainly, most phonologists would assume at first sight that distributional restrictions usually target marked segments. But is it really so? Our job is to account for the empirical facts, not to dismiss them on the grounds that they contradict a vague and unreferenced expectation. What is certain is that the markedness issue is not a black and white one, as Pellard seems to assume. I am not sure that distributional restrictions alone should be considered to target marked segments on a wholesale basis. To exemplify the difficulty of (un)markedness judgment with regards to distributional restrictions, consider the case of the French schwa. Van Oostendorp (1998) mentions the following properties of French schwa: frequency, orthographic variety, phonetic character (shortest of all vowels, central, phonetic variability), epenthetic, easily deleted, interaction with other segments, no occurrence before another vowel, no occurrence after \( h \) aspiré, no occurrence word-initially. A number of these properties are strikingly reminiscent of /r/ in Japanese. Now, most phonologists would agree that schwa in French can be considered an unmarked, default vowel. Some would even argue that it is an empty vowel, the materialization of a phonological zero. However, the fact that schwa does not occur word-initially does not fit into the picture and appears as contradictory to those who assume, like Pellard, that “distributional restrictions usually target marked segments”. So what shall we do? Try to find an explanation which would account for the seemingly contradictory status of the French schwa, or just dismiss the issue of the French schwa because it does not correspond to what is expected?
The Hr aims precisely at reconciling apparently contradictory facts. If Pellard wants to argue that /r/ is a marked consonant in Japanese, contra a number of other previous studies (Mester and Itô 1989, Nasukawa and Backley 2011), that would be a perfectly respectable stance. But he should propose a fully developed analysis of the problem, not just content himself with a sweeping and unreferenced statement about distributional restrictions being usually supposed to target only marked segments.

6. The typological issue

In my paper (L 22), I mention that Japanese is not the only language which prohibits rhotics - or more generally liquids - word, stem, or root-initially. I provided a short list of such languages belonging to different families. Regarding which Pellard observes (P 355) that “the typological evidence quoted is empirically questionable”. He also deplores that “only a list of languages is given, without any references”. I am glad to provide the required information in this response, leaving it up to readers to draw their own conclusions.

- Proto-Austronesian: “there are a few [Proto-Austronesian] reconstructions with initial *r but most are doubtful.” (Ross, 1992, endnote 16).

- Burushaski: “[r] is rare in initial position, especially so in non-loanwords.” (Anderson 1997: 1026)

- Proto-Indo European: “PIE must be reconstructed without initial /r/.” (Lehmann 1951, cf. also Kuryłowicz 1927, Bévéniste 1939).

- Australian languages: “/r/ cannot normally occur initially in most Australian languages” (Dixon 1972:3).

Actually, c. 200 additional languages or proto-languages which have been reported as exhibiting a certain degree of word or root-initial liquid prohibition could be mentioned,
among which are Slave (Na-Dene, Athapaskan): “a stem can begin with any consonant other than /h/ and /r/” (Rice 1989:143); Chechen (Nakh-Daghestanian): /r/ is “initially phonotactically prohibited” (Nichols 1997: 966); Susu (Niger-Congo, Mande): “la vibrante est absente en position initiale des lexèmes radicaux” (Houis 1963:27); Adang (Timor-Alor-Pantar): “while pAP [Proto-Alor-Pantar] *r did not occur in initial position, there are a small number of words in Adang with initial /r/ today” (Robinson and Haan 2014: 227); Dizi / Sheko (Afro-Asiatic, North Omotic): “/r/ does not occur word-initially except in the ideophonic word rururu” [call a cat] (Helltenthal 2010:54); Maipure (Arawakan): “In initial position, all the attested phonemes occur except /b/ and /l/ which, in our corpus, also do not occur initially in any stem” (Zamponi 2003:17); Timucua (Timucua): “all consonant phonemes except flap r can occur in post-junctural position (Granberry 1956); Barbacoan languages: “r cannot occur initially in any of the Barbacoan languages that have it as a phoneme” (Curnow and Liddicoat 1998); Hualapai (Hokan), “all segmentals, except /r/, occur initially” (Redden 1966); Proto-Keresan: “initial r is rare” (Miller and Davis 1963); Proto-Salish, “/r/ cannot be the first consonant of roots” (Campbell 1997:116), etc.

Space limitations do not allow me to provide a longer list, but I think that this sample of unrelated languages should suffice to convince Pellard of the cross-linguistic reality of a word (or root / stem) initial liquid prohibition.

Further, contrary to what Pellard seems to assume (P 354) when he says that “the Hr presents typological parallels for the ban of /r/ from word-initial position in Japanese (Labrune 2014, p. 22) but such evidence is irrelevant in trying to determine whether /r/ is a unique featureless consonant in Japanese” [italics by Pellard], I never claimed that the existence of typological parallels for the ban of /r/ in word-initial position is relevant in trying to determine whether /r/ is a unique featureless consonant in Japanese, nor even that something could be inferred from that about the specific characteristics of Japanese /r/.
Pellard oversimplifies my arguments and distorts my ideas. My point was simply to report a puzzling typological fact about the word-initial liquid prohibition, which seems to have passed altogether unnoticed in the field of typological phonology. Another reason for bringing this typological fact to the attention of readers is that the ban on word initial $r$ has been repeatedly mentioned as a characteristic of the so-called Altaic (or Ural-Altaic) family for more than one hundred years. The fact that the initial liquid prohibition is widespread cross-linguistically in languages belonging to different genetic families and geographical areas shows that the importance of this criteria to establish a possible Japanese–Altaic relationship has been overestimated.

7. Phonological processes

Concerning the statement by Pellard that “the resistance of /r/ to gemination is not as strong as stated by the $H_r$, and it thus does not support the special status of /r/” (P 365), the use of “thus” in this sentence seems problematic. Even if the resistance of /r/ to gemination were not as strong as stated by the $H_r$, it is still undeniable in a number of specific contexts, as Pellard acknowledges, and this calls for an explanation. Moreover, even if the rarity of geminated /r/ in Japanese may proceed from universal phonetic properties, and if other segments than /r/ also resist gemination, as Pellard observes (P 364), I still think that /r/’s reluctance to geminate in Japanese is definitely a feature that should be considered in a paper dealing with the phonological characteristics of this consonant in Japanese.

Pellard also writes that “geminated /r/ is amply attested in both mimetic […] and non-mimetic emphatic forms […] ” (P 365). No, geminated /r/ is not “amply” attested. One can object that such geminated /r/’s are quite recent and still marginal in Japanese, in addition to being confined to a specific pocket of the lexicon, and so their occasional occurrence does not really affect the $H_r$. 
8. The morphological issue

On page 367, Pellard writes that “the epenthesis hypothesis misses an important generalization and cannot account for all alternations in a unified fashion, in contrast with other approaches”. Readers would certainly like to know how the other approaches account for the alternations in a unified fashion. Actually, no approach, including the epenthetic one, can account for all the verbal alternations in a unified fashion (otherwise there would be no controversy at all!). But I believe that the epenthetic approach is able to account for a good number of otherwise puzzling facts. In contrast, the r-deletion hypothesis which Pellard seems to favor misses an important generalization, which is that the long alternant of many inflectional endings begins with /rl/. Furthermore, the epenthetic approach is not a marginal one, as Pellard suggests. Two very recent papers recently published by Brent de Chene even bring new experimental and statistical data to support it in Japanese and in Ryukyuan (de Chene 2016a, 2016b). In the Shuri Ryukyuan dialect moreover, de Chene argues (de Chene 2016b), r-epenthesis is a living process that is still being extended.

As for what Pellard assesses as a “strong argument against the epenthetic approach” (P 357), that is, the fact that other consonants, namely /ls/ and /ly/ also alternate with zero in the causative suffix -sase / -ase and the hortative -yoo / -oo, I view it as a strong argument in favor of the epenthesis approach. This is because the initial /ls/ and /ly/ in -saseru and -yoo are actually to be analyzed as epenthetic. The basic, underlying form of the factitive is -ase, and the initial /ls/ of the post-vocalic allomorph -sase results from the default copy of the /ls/ already present in the /-ase/ suffix. Concerning -yoo / -oo, it is well known that the /ly/ originated as an epenthetic consonant after -i stem verbs, (kami-ichidan and kami-nidan verbs). The epenthesis of /ly/ after a front vowel was actually not uncommon in OJ or pre-OJ: one could also mention the case of the imperative suffix -yo2 which has been accounted for by Ohno (1953) as the result of the suffixation of an *-o2 imperative mark to -i and -e stem verbs.
(kami ichi-, kami ni-, shimo ichi- and shimo ni-dan verbs), and which also happens to have a -\textit{ro}_2 allomorph. The obvious explanation for why \textit{y}/ was inserted in these two cases is that the first of the two vowels in hiatus was a front vowel. This interpretation does not contradict the general scenario that I developed in my paper, because it rested on the assumption that the basic mechanism of \textit{t}/ insertion is precisely an assimilation process (renamed Featural Agreement).

The \textit{s}/ and \textit{y}/ epentheses cases discussed above confirm that consonant epenthesis was a device available to avoid a VV sequence in Pre-OJ verbal morphology, and that the epenthetic consonant could inherit some of the features of its environment, or even all of its features, as in the \textit{-asu} / \textit{-sasu} case. The surface consonant is different in all three cases (\textit{r}, \textit{s} or \textit{y}), because it results from different default filling processes, but I view this difference as a difference in degree, not as a difference in kind (this degree being measurable in terms of the number of features copied from the environing segments). What is significant here is that the process at work was epenthesis. Why \textit{t}/, \textit{y}/ or whatever consonant already present in the suffix was not always chosen as an epenthetic consonant remains to be accounted for, although the causative pair \textit{-asu} / \textit{-rasu} is actually attested, as well as \textit{-ro}_2 as an alternant of \textit{-yo}_2 for the imperative. But this should not bother us unduly: variation is a fundamental property of human language, and no linguistic phenomenon is totally variation-free.

On page 367, Pellard gives examples of verbs in which a segment other than \textit{t}/ has been used as the base-final consonant (\textit{kozik-}, \textit{mokurom}-), and argues that these forms cannot be explained in the same way as other verbal neologisms. However, such examples are extremely low in number and marginal, so I do not think that they are likely to jeopardize the general hypothesis.

Pellard then introduces what he considers a “decisive counter argument to the epenthetic hypothesis” (P 367), that is, the fact that innovative \textit{r}-conjugation verbs of contemporary
Japanese do not belong to the vowel-conjugation class but to the consonant-conjugation class. This is obviously true, but the relevant point is that such verbs add the \(-ru\) suffix rather than any other \(-Cu\) suffix. This is all I wanted to point out. I fail to understand how innovative Modern Japanese data could prove to be a “decisive counter argument” to a hypothesis developed for the pre Old Japanese stage. Certainly, Modern Japanese materials can bring interesting insights, but to view them as a “decisive” counter argument for a 1500 years older language stage seems misguided.

Finally, in section 6.2 (P 368 sq), Pellard expresses doubts regarding my claim that \(/t/\) is “extremely frequent at the beginning of several […] suffixes”. In order to countercheck this claim, he compiled a list of all OJ suffixes listed in Vovin (2005, 2009). He undertook two different approaches to conduct his verification. Under the first approach, \(/t/\) ranks second after \(/k/\) as the most frequent initial in OJ suffixes. Under the second approach, \(/t/\) and \(/k/\) tie in ranking. I fail to see how these statistical data contradict my initial claim. The fact that \(/t/\) is the most frequent initial in suffixes on a par with \(/k/\) or the second most frequent initial after \(/k/\) does indeed mean that \(/t/\) is “extremely” frequent. And here too, what distinguishes \(/t/\) from \(/k/\) is that \(/t/\) does not normally occur at the beginning of independent words, whereas \(/k/\) does.

9. The worogamu / ‘orogamu issue

My paper proposed a re-analysis of examples initially discussed by Unger (1977) who argued that the OJ verbs \(\text{worogam- ‘revere’, ‘ir- ‘scord’ and ‘urutape- ‘appeal’ were originally} \ *\text{rorogam- from *rogam-, *riri- from *ri- and *rurutape- from *rutape-}. \) These examples are important because if Unger’s hypothesis is correct, it would mean that \(/t/\) could occur at the beginning of autonomous words in pre OJ, thus contradicting the H\(\text{r}\) claim. Pellard criticizes my re-construction of \(\text{worogam- as *örögam-, from *öögam- (or *o}_2\text{ro2gam-from o}_2\text{o2gam- following the transcription adopted by Pellard).} \)
But Pellard makes three incorrect or misleading assertions in his discussion of the *worogam*-example. First, when he states that “both OJ *wogam* and *worogam* clearly had an initial glide w- distinct from the zero onset, and that consonant cannot be considered to be an artifact of transcription” (P 369). This is false. The reality of the supposed w-initial before o is controversial. Moreover, the orthographic problem is real. Contrary to Pellard’s assertion (P 369) many confusions between the moras represented by the modern kana お~オ and を~ヲ and their *man’yōgana* correspondents are encountered in OJ and EMJ texts in word-initial position (Arisaka 1955:645, Mabuchi 1971: 33-34, 62, Mabuchi 1999: 149, Konno 2001:458, Seeley 1991:111, etc.). Second, Pellard claims (P 369) that “in the *Kojiki* (712) OJ *wo* is always transcribed by Chinese characters with an initial *w*- and OJ ‘*o* by characters with an initial *ʔ*.” However, *Kojiki* evidence is irrelevant here, because the lemma *worogamu*/*’orogamu* is not attested in this source. Its oldest attestation in *man’yōgana* spelling is found in the *Nihonshoki* (720), where it is written 鳥呂餓瀰. It is crucial to note that the initial of the character 鳥 has been reconstructed as *ʔ, *qʕ or *ʔʕ in Old Chinese, not as *w* (Starostin, 2005; Baxter and Sagart 2014). Thirdly, Pellard (P 370) misunderstands and distorts my proposal, which rests on the assumption that the initial mora of what Unger transcribes as *worogam*- must be reconstructed as the non-labialized vowel o₂, not as o₁. This is because, considering the fact that in 鳥呂餓瀰 the呂 mora is clearly ro₂, the preceding vowel can also be nothing other than o₂ by virtue of Arisaka’s Law, which prevented o₁ and o₂ to occur in the
same root. Thus, the hypothesis of \( *o_2gam \)- yielding \( *o_2o_2gam \)- by initial vowel reduplication (as proposed by Unger), and subsequently \( o_2o_2gam \)- with \( r \) epenthesis, is perfectly plausible.\(^7\)

10. The cross-linguistic evidence on \( r \)-epenthesis

Pellard writes that “[a]n important point not fully discussed by the \( Hr \) is whether \( r \)-epenthesis is a cross-linguistically common and natural phenomenon” (P 372). Pellard reproaches me for having mentioned “only” two cross-linguistic parallels involving \( r \)-epenthesis, the English and Dutch cases (L 6, footnote 6; L 22 footnote 17). In response, I shall now add the Basque and Ryukyuan cases, where the epenthetic status of /t/ is actually more conspicuous. In Basque (Hualde and Ortiz de Urbina 2003:175), an epenthetic /t/ appears at the beginning of several vowel-initial suffixes when they follow a vowel ending base, for instance \( Mikel-ekin \) ‘with Mikel’ vs. \( Kepa-rekin \) ‘with Kepa’, \( aran-ik \) ‘valley’ (partitive) vs. \( mendi-rik \) ‘mountain’ (partitive), \( Mikel-en \) ‘Mikel’s’ (genitive) vs. \( Kepa-ren \) ‘Kepa’s’ (genitive), etc. As for Ryukyuan, the evidence presented by de Chene (2016b) shows that \( r \)-epenthesis is a “living process that is still being extended” in the verbal morphology. Pellard relies on Hall (2013) to question the naturalness and frequency of \( r \)-epenthesis in languages. However, the issue is not settled and there has been ongoing disagreement on whether rhotics are frequent and/or natural epenthetic segments. Wiese (2011:725) for instance, writes that “[r]hotics are also found quite often as epenthetic consonants” and that “the phonetic variability of rhotics, ranging from a fricative to a glide, may make an r-sound particularly suitable as an epenthetic segment”. Undoubtedly, opinions

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\(^7\) Since space limitations do not allow me to provide all the details and references pertaining to this philological discussion, readers are referred to Labrune (2017) for the complete version of this section.
diverge, and additional research is obviously needed. But in any case, as regards the search for cases similar to that of Japanese, I think the focus should not be restricted to /r/ but widened to any consonant which could be considered as under-specified or defective in a given language. This is because the factors that allow /r/ to occupy the status of default or empty consonant in the phonological system of Japanese may be different in another language. So it is not /r/ in the absolute which should be investigated (even if coronal rhotics constitute obviously good candidates), but all epenthetic consonants. For instance, it has been known from Paradis and Prunet (1991)’s seminal volume that the feature coronal frequently works as a default place feature, but not all rhotics are coronal, so in a phonological system with a non-coronal rhotic, the default, epenthetic consonant might not be a rhotic but a non-rhotic coronal (on the issue of coronal special properties and their phonological representation, see also Avery and Rice 1989, Harris and Lindsey 1995, among other).

11. The historical scenario

On page 373, Pellard writes that “[t]here is decisive evidence that runs counter to the hypothesis of a late development of r and its predictions […]. If r were indeed originally an allophone of zero, there should be many cases where r alternates with zero. However there are none […].” Well, as Pellard himself acknowledges in footnote 42, alternations between r and zero can be found in the verbal morphology according to certain studies. Nevertheless it is true that there exist examples of alternation of zero with consonants other than r noun-initially but none with r. He mentions the oft-cited examples *harusame* ‘spring rain’ (the combination of haru ‘spring’ + ame ‘rain’), and *urusine* ‘glutinous rice’ (uru ‘?’ + ine ‘rice’) with a putatively epenthetic s. However, what Pellard does not mention is that the epenthetic nature of this s is not at all certain (see Martin, 1987:35-36 for alternative accounts).

However, whatever the origin of the s in *harusame* and *urusine*, Pellard is right to signal that there is “complete absence” of a single clear example of r-epenthesis comparable to the
*haru-same* example, that is, in the context of a compound juncture. But there is an obvious and straightforward explanation for this fact: *r* has settled as an epenthetic consonant in derivational contexts but not in compositional ones, probably under the pressure of analogy and through a process of paradigm leveling which occurred after the full phonologization of */r/* was achieved, requiring that nouns exhibit the same phonetic form under both their independent and non-independent usages. Basque can once again be mentioned as an illustration of the difference between derivational and compositional morpho-phonology, since *r* functions as an epenthetic consonant in derivational suffixes but is never found at a compound juncture (whereas several other consonants are). We could also mention French, in which */t/* is the default, epenthetic consonant in derivation (cf. *bijou* / *bijoutier* ‘jewel’ / ‘jeweller’, *sirop* / *siroter* ‘sirup’ / ‘to sip’, *psycho-* / *psychoter* ‘psycho-*’ / ‘to worry’) but not in compounds (*bijou-aimant* ‘magnet jewel’, *sirop-édulcorant* ‘sirup sweetener’, *psychanalyse* ‘psycho-analysis’ and not *bijou-t-aimant*, *siro-t-édulcorant*, or *psycho-t-analyse*).

In addition, Pellard’s remark (P 373) that “there isn’t any example of epenthesis of */b/*, */d/*, */g/* and */z/* either” is not totally accurate, because although not epenthetic in the strictest sense of the term, these consonants happen to be the exponents of *rendaku* insertion in Japanese, which explains why they can appear very frequently at the beginning of the second constituents of compounds. Their status is thus totally different from that of */r/*, because they do occur at compound juncture whereas */r/* does not, and they also play a morphological role.

Pellard also reminds us (P 373-374) of the fact that hiatus was resolved by vowel elision or crasis in OJ and in internal reconstruction of proto-Japanese. He even asserts that this was “always” so … but he nevertheless contradicts himself with the *harusame* and *urusine* examples. He also mentions two cases where hiatus was resolved by *r* insertion, which he analyzes as analogical leveling… whereas I analyze these latter examples as instances of *r-
epenthesis. What I have to say here is that languages are generally not exclusive in their morpho-phonological devices, so Japanese can very well have resorted both to vowel elision or crasis and to consonant epenthesis. Once more, Pellard’s reasoning is conducted on a black and white basis, but natural languages are more complicated than what he seems to assume. One should keep in mind that reconstruction of unattested stages of a language inevitably leads one to conflate several different diachronic stages. The exact chronology of the different processes in not easy to reconstruct, and it is quite possible that elision and crasis as hiatus resolution processes, as well as r epenthesis, were used at different periods. But Pellard’s reasoning is nevertheless flawed, because while dismissing the possibility that r epenthesis might have occurred in parallel to crasis and elision, he sees no problem in accepting that both crasis and elision could have been conjointly used at the same historical period.

Finally (P 375), Pellard mentions four possible explanations for the word-initial prohibition of /r/ in contrast with its high word-internal frequency (a fact that he finally acknowledges …). These four explanations are:

a) initial r-apheresis: *r > 0 /#_

b) initial vowel prothesis: 0 > V / # r

c) initial mutation: *r > X / #_

d) medial mutation: *X > r / V V

Explanation d) is the one I developed in my paper (′ > r / V V). The other three explanations are theoretically possible but I do not see any convincing grounds for them in Japanese. Analysis a) seems unlikely, even if there is at least one possible example of it in Japanese: the Chinese word for ’sulfur’ liu huang 硫黄 is rendered as ioo in Modern Japanese (? yuwa > yuwa > yoo > ioo) with loss of the initial liquid. The process described in b) is well documented in languages, and there are a few examples of initial vowel prosthesis before /r/
in Japanese. For instance the word *rosiya* ‘Russia’ was borrowed as *orosiya* during the Edo period, but such examples remain marginal. Pattern c) is very common cross-linguistically, initial rhotic in loans being often replaced by /d/, /n/, /l/ or other segments in languages which do not accept initial rhotics. However, I am not aware of any evidence which would support this scenario in Japanese. Explanation d) is the one which, in my opinion, is actually supported by both internal and external evidence in Japanese.

12. The formal analysis

Pellard considers the formal OT analysis offered in my paper “flawed in several aspects” (P 375). He begins his critique with specious considerations on the formal (mathematical) difference between zero (nothing) and the empty set, and the difference between the absence of onset and the existence of an onset position not linked to segmental material. I do not see how this point bears on the discussion.

It is difficult to validate Pellard’s claim that the “usual” or “basic” realization of Japanese /r/ is a tap (P 376, also 353). I am not sure I understand what “usual” and “basic” mean here. Japanese /r/ is indeed frequently described as a tap in standard Japanese, which, like all standard languages, is an idealized object, but this does not mean that the tap is necessarily the sole realization of /r/, nor even that it is its most frequent and common realization. For instance, according to Ohnishi (1987), who conducted dynamic palatographic experiments, the articulation of /r/ “has, first lateral- and secondly, fricative-oriented tendencies” (see also Matsuno, 1971, for a discussion on the many allophones of Japanese /r/). In actual usage, Japanese /r/ is also frequently a lateral, a trill, a fricative or even a stop, which I take as a clue that the feature [continuant] is not really relevant for Japanese /r/ (as explained in footnote 12 of my paper, contra P 376). But these realizations are generally not mentioned in introductory textbooks or general descriptions of “standard Japanese” like the ones cited by Pellard.
Similarly, Pellard argues (P 353) that what is assumed to be the invariant features of /r/, that is, voiced, apical, alveolar or post-alveolar, with short or weak closure “directly contradict the emptiness” argued for /r/. But it is actually just the opposite. Voiced can be argued to be unmarked in a voiced environment; apical, alveolar or post-alveolar come down to coronal, a place feature which, as already mentioned, can been argued to be the default, unmarked place feature for consonants; and it is obviously not very difficult to argue that a short or weak closure could be considered as the sign of an unmarked or defective segment.

Furthermore, if the class feature [consonantal] is not used in my formal analysis (P 376), it is because I take consonantality to be a positional or functional property derived from the prosodic position of segments, rather than an intrinsic property that segments inherently possess, as initially posited in the linear SPE approach. In other words, consonantality is more appropriately conceived in terms of prosodic constituency, so any segment occupying the onset position is phonologically consonantal.

Pellard also argues that the distributional asymmetry exhibited by /r/ requires a plausible historical explanation based on phonetics, and that the spontaneous emergence of /r/ posited by the Hr is not grounded in either perception or production (P 378). This criticism is unwarranted. First, one could once again quote Wiese (2011:725), who considers that the phonetic variability of rhotics may make r-sounds particularly suitable as epenthetic segments. Second, the scenario for the emergence of /r/ that is proposed in my paper is phonetically grounded, since it posits that /r/ emerged as the result of the propagation of environmental articulatory and acoustic features, which basically comes down to the action of assimilatory processes of the most common type. It is true that some of the resulting features are also assumed to result from processes which are more functional than strictly articulatory or perceptual in nature, but it would be too reductive and simplistic to assume that sound changes are uniquely grounded in phonetics in the strictest sense of the term. Systemic or
functional, that is, phonological motivations, are also active and have been amply documented in the evolution of languages. I am convinced that phonology is better capable of explaining the patterns and behavior of sounds. This is why my analysis relies principally on phonological grounds; but phonetic arguments are also taken in consideration, and cannot said to have been overlooked or neglected.

A recurrent tendency in Pellard’s argumentation is to appeal to the action of a “constraint” to account for a given particularity of /r/’s behavior (P 354, 364, 365, 372, 376, 378) with the fallacious implication that the existence of the alleged constraint should contradict my analysis. But to posit the action of a constraint, which only encapsulates an empirically observable fact, is no explanation for why this fact should exist in the first place. In an OT based approach, it is the interaction between the constraints which accounts for why things are the way they are, not the existence of constraints per se. So, for instance, appealing to the alleged *Nr constraint (as proposed by Kawahara 2015) to account for the fact that /r/ does not occur after /N/ in mimetics does not teach us anything that we do not already know.8

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8 Even if the analysis rests on constraints which are hierarchized in a manner that can properly account for the surface linguistic forms, we still need to understand why a given constraint exists the way it does. This is what Kawahara (2015) does by tentatively positing a *INITIAL/r/ constraint, which would be grounded on “the possible articulatory difficulty of initiating a ballistic gesture for /r/ word initially”. However, this does not fully convince me because not all rhotics are made with a ballistic gesture, and the supposed difficulty to initiate a word-initial ballistic gesture obviously requires further investigation. More importantly, in my view, such an articulatory constraint does not suffice to account in a unified manner for the other, apparently unrelated, phonotactic and frequency properties of /r/ in Japanese (or in other languages).
Constraints are useful and helpful if one wants to develop a formal account within a constraint-based framework, but such a formal account is not what is going to justify why a constraint exists as it does. For instance, one can posit the constraint No-Coda, and use it very efficiently in OT tableaux; but this has very little to do with trying to understand why many languages favor codaless syllables. It is because one was able to pose this question and found that yes, indeed, there are phonetic and phonological reasons for languages to dislike codas, that we are justified in positing the existence of the No-Coda constraint.

13. New issues: the relative chronology and the inheritance hypothesis

After the publication of my paper, I became aware of additional issues raised by a reviewer, which I feel deserve further investigation. These issues concern the dating of the ‘→r/V_V’ change, and the possibility that some Japanese /r/’s have entered the language through borrowing or language contact.

A blindspot in my paper was that it remained vague concerning the dating of the emergence of /r/ in Japanese. First, and in response to the reviewer’s comment, let me clarify what is meant by the term “Proto-Japanese” which was used in my paper. By Proto-Japanese, I mean the ancestral language from which all other later varieties of Japanese, including Ryukyuan, descend (this definition is a standard one, cf. Frellesvig and Whitman 2008:1).

Although I am unable to provide any absolute dating for the phonologization of /r/ in intervocalic position, I would propose that it occurred several centuries before OJ (700-794), that is, before what some scholars, including the reviewer cited above, refer to as “pre-OJ”, a stage that is generally placed between 500 CE and the appearance of the first continuous OJ records shortly after 700 CE. In any event, my view is that the emergence of /r/ in the intervocalic context occurred before the split between Japanese and Ryukyuan. It does not
postdate it since the epenthetic status of /r/ in the verbal morphology is also attested in Ryukyuan. Instead of “Proto-Japanese”, one may prefer to use the terms “Japonic” or “Proto-Japonic”, in order to explicitly include the ancestor of the Ryukyuan dialects in the discussion.

This being said, the problem is that the date of the separation of the Japanese and Ryukyuan branches is still debated. Following Uemura (1997:315), I tentatively advance the idea that Japanese and Ryukyuan diverged at a relatively early date, sometime during the Yayoi period (3rd c. BCE and 3rd c. CE). The process of /r/ phonologization that I hypothesize would have taken place earlier, during the early Yayoi period or even possibly the very late Jōmon period.

Another important issue that should be considered is whether all Yamato Japanese /r/’s developed according to the historical scenario proposed in my paper. The possibility that OJ /r/ has been inherited from an ancestral language, and that it would thus not be the result of a Japonic internal development, has been suggested by a reviewer, who observes that the proponents of the Altaic hypothesis would have a ready explanation for the high frequency of OJ /r/ word-internally, since Japanese /r/ could represent the merging of three distinct proto-Altaic liquids (*r, *r2, *l). However, the fundamental problem here is that Japanese has not been convincingly demonstrated to be of Altaic descent. The supporters of the Altaic hypothesis themselves cannot but acknowledge that there is no scholarly consensus on the hypothesis of an Altaic / Japanese relationship. As Robbeets (2007) puts it, the opinions expressed in the literature range from negation to a neutral or agnostic attitude to a positive stance. In my view, the Altaic origin of Japanese is, at best, a working hypothesis, so my stand would be that of an ‘agnostic’ in Robbeets’s terms. I want to make it clear that I did not start my research with the premise that Japanese is genetically related to Altaic. My approach has been entirely conducted following a strictly internal methodology, my position for more than twenty five years being that internal reconstruction is a prerequisite to any serious further
attempt to prove the genetic relationship of the Japonic languages to another language or language family through external comparison.

However, it is true that regardless of one’s position on particular claims of genetic relationship, the possibility that some /r/’s have been inherited ought to be considered. I do not exclude the possibility that some “Yamato” Japanese /r/’s may have had an external source, through language contact or borrowing, which would have occurred at the time when the phonologization process of /r/ in Japonic was taking place, or after it. The two origins, the internal and external one, are not antagonistic. One phoneme can have more than one single source. The /r/ in uri ‘melon’, hari ‘needle’, turu ‘crane’ or tera ‘temple’ could thus result from borrowing (likely candidates exist in Korean, see for instance Martin 1966). But as stated above, I think that the external comparison should be done only after the internal reconstruction work has been sufficiently advanced. Prior to searching for the ancestors of Japanese /r/ outside of Japan, one should first clarify its functions and characteristics within Japanese proper. This is what I have tried to do.

14. Conclusion

We are now ready to consider the conclusion that Pellard offers on Japanese /r/: /r/ behaves the way it does because it is “isolated” (P 379) in terms of the number of phonemic oppositions it enters into. Well, it could also be that /r/ is isolated because it behaves the way it does, so the argument is begging the question. But even if we were satisfied with this statement, it is worth observing that in many languages /r/ can be described as isolated, but it nevertheless does not enjoy the same special status as Japanese /r/ does. French or Italian are such examples.

In addition, Pellard states, /r/ “is underspecified since the single feature “liquid” suffices to distinguish it from all other consonants” (P 379). This argument, however, encounters a
serious problem: the unitary feature “liquid” does not exist in any of the major and most elaborated feature theories that I am aware of, be it the Jakobsonian feature theory (Jakobson and Halle 1956), SPE theory (Chomsky and Halle 1968), Feature Geometry (Clements 1985, Sagey 1991, Clements and Hume 1995), Dependency Phonology (Anderson and Durand 1987) and Element Theory (Kaye, Lowenstamm and Vergnaud 1985, Harris and Lindsey 1995, Backley 2011). Walsh Dickey (1997: 150) proposes that a new feature [liquid] should be posited but fails to provide a substantial definition of it. Even if the term liquid has been of common use since European Antiquity, it has received no formal status in modern phonological theory because it is not possible to strictly define “liquidness”. “Liquid” remains a convenient label for an indistinct class of intuitively easy-to-grasp sounds that share a “family resemblance” – the class of all “l” and “r” sounds. But saying that a segment is a liquid is not going to take one very far into phonological analysis. It is just as difficult to provide a coherent formal definition and representation of “rhotics”, because rhotics exhibit huge variability between and within languages (Lindau 1985, Wiese 2011). So Pellard’s conclusion does not really bring any new insight to the issue of /r/ in Japanese: labeling /r/ as a liquid is nothing more than a tautology in a system like Japanese which contains only one r- or l-like sound.

Pellard’s overall categorical assessment is as follows: “the hypothesis that /r/ is a special, empty consonant in Japanese has thus to be rejected and alternative accounts should be preferred” (P 352). The question is: what alternative accounts? Pellard does not offer any alternative account.

My basic claim was, and still is, that there exists a principled explanation (the Hr) that can relate four apparently unrelated properties of the Japanese rhotic (L 3): a) /r/’s distributional gaps; b) its overwhelming frequency word internally; c) the fact that it is more likely to occur late in words; d) its complementary distribution with zero. None of these four
properties has been shown to be inaccurate by Pellard’s countercheck of the facts. The argument offered by Pellard that /t/ is phonemically isolated, or that /t/ is a liquid, just does not account for any of these four central properties and neither does it account for the many other characteristics of Japanese /t/.

So we have come full circle.

From Pellard’s paper I shall actually retain two arguments which, in my opinion, require further inquiry. The first one concerns the acquisition of Japanese /t/: it is true that this consonant is acquired late by Japanese children (P 372; L 21 footnote 16; Ueda & Davis 2001; Arai 2013), and that this is not what we would expect of an unmarked segment. A possible line of explanation is proposed by Arai (2013): Japanese /t/ is difficult to acquire by children because there exists a wide variation in the pronunciation of the /t/ phoneme among adult speakers. The second interesting issue which deserves further theoretical research is the fact that positional restrictions are generally considered to target marked segments rather than unmarked ones (see my comments in section 5).

To these two issues, one should add two new ones, suggested by a reviewer and which have been addressed in section 13 of this paper. There is, first, the question of the dating of the ’ r/V_ V process in prehistoric Japanese, and secondly, the issue whether all Japanese /t/ developed according to the Hr scenario.

I conclude this rejoinder with another quotation from Marc van Oostendorp’s article, cited earlier in this paper. In his investigation of French schwa, van Oostendorp makes the observation that “[l]inguistic theory has to explain th[e] special behaviour of schwa: why is it exactly this segment which behaves in exactly this way […]”? The same remark applies to Japanese /t/: *linguistic theory has to explain the special behavior of /t/ in Japanese: why is it exactly this segment which behaves in exactly this way?* My paper was an attempt to answer
this question, and the merit of the Hr is precisely that it provides a plausible explanation for
the seemingly contradictory status of /r/ in Japanese.

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