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A refutation of Song's (2014) explanation of the 'stop coda problem' in Old Chinese

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1 Introduction

In a recent article Chenqing Song (2014) draws renewed attention to the problem of groups of Chinese words in which the character used to write one of the words has a stop final reading in Middle Chinese but the character used to write another of the words has an open syllable reading in Middle Chinese, although the two words in question either rhyme in the poems of the 詩經 *Shijing* or are members of the same phonetic series, in either case implying that they shared a rime in Old Chinese. She exemplifies this problem with the rhyme of 莫 MChi. *mɔk* / *maek*, *mâk* / *mak*, *muo*^C / *muH* 'end'¹ and 除 MChi. *djwo* / *drjo*, *djwo*^C / *drjoH* 'to pass' in poem 114 and the inclusion of 乍 MChi. *dza*^C / *dzraeH* 'suddenly' and 作 MChi. *tsâk* / *tsak* 'to act' (Song 2014: 99) in the same phonetic series.

Song reviews two previous proposals (1) 'the voiced stop coda hypothesis' and (2) 'the open syllable hypothesis' to explain these data and she finds them both wanting. She offers a new proposal that employs a the reconstruction of voiced and voiceless stop finals (exactly opposite in distribution to those in the system of Li Fang-kuei) in the ancestor of Chinese and Tibetan, with the lexical diffusion of the loss of the inherited voiceless series paradoxically both at the time of the script's invention and during the time of the 詩經 *Shijing* compilation, explaining the relevant oddities in phonetic series and rhyme practice. The comparison of Tibetan voiced finals with Chinese voiceless finals serves as a keystone to her argument.

Unfortunately, every step in Song's reasoning is faulty and nearly every claim she makes about Tibetan is false. André-Georges Haudricourt long ago solved the 'stop coda problem' in

1 I here give Middle Chinese (MChi.) in the system of Li (1971: 4-7; 1974: 224-227) and Baxter (1992) separated by a slash, and Old Chinese (OChi.) in the systems of Schuessler (2009) and Baxter & Sagart (2014), separated by a slash. Giving each form in two systems is cumbersome, but helps to draw focus to the fact that the argument nowhere relies on the details of a specific system.

a duly famous article (1954); Song's article contributes nothing and indeed risks propagating misconceptions both old and new.

2 The voiced stop coda hypothesis

Bernhard Karlgren noted rhyme and phonetic series contact between Middle Chinese closed and open syllables and reconstructed Old Chinese voiced stop finals as the source of the open syllable finals in the relevant words (1923: 27-28, cf. Baxter 1992: 332). Karlgren reconstructed many open syllable words in Old Chinese, although he decreased the number of them in his reconstruction during the course of his career (Baxter 1992: 326-328). On the basis of rhyme contact between words with Karlgren's voiced finals and Karlgren's open syllables, Li Fang-Kuei extends voiced stops (particularly *-g) to all of those cases in which Karlgren retained open syllables (Li 1971: 24-27, 1974-5: 247-252).

Song (2014) obscures the differences among various adherents of the 'voiced stop coda hypothesis'. Her single critique, that it is typologically aberrant for a language to have no open syllables, applies to Li's system but not to Karlgren's.² As a solution to the 'stop coda problem' the 'voiced stop coda hypothesis' is satisfactory. The demerit of this hypothesis is primarily that the 'open syllable hypothesis' is more elegant, more precise, and more predictive.

3 The open syllable hypothesis

Song mischaracterizes the views of those scholars she classifies as adherents to the 'open syllable hypothesis', claiming them to believe that all Middle Chinese open syllables descend from Old Chinese open syllables (2014: 102). In fact, these scholars posit two sources for Middle

2 Despite her explicit rejection of Li's system Song employs it for her own transcription of Old Chinese words. It is likely that this choice blinds her to the details of those who work with the 'open syllable hypothesis'.

Chinese open syllables. One source is indeed the direct descendent of Old Chinese open syllables, but the other, originally proposed by Haudricourt (1954), results from the simplification of clusters, e.g. *-ks > *-s > -H. Haudricourt's hypothesis not only solves the 'stop coda problem' but also accounts for the origin of the Middle Chinese 去聲 *qùshēng* tone. Song appears unaware of Haudricourt seminal contribution, although her bibliography includes works that describe Haudricourt's hypothesis and add further evidence on its behalf (Bodman 1980: 52, Baxter 1992: 308-319, Baxter & Sagart 2014: 196-197³). It is easy to cite further researchers who also accept Haudricourt's proposal (e.g. Jaxantov 1978-79: 38, Starostin 1989, Zhengzhang 2000, Schuessler 2007: 29). Aside from these and many other publications, such easily accessible sources as the Wikipedia article 'Old Chinese phonology' (accessed 25 October 2014) provide lucid explanations of Haudricourt's proposal. Although not everyone accepts this proposal (e.g. Gong 2002[1995]), its opponents ignore or briefly dismiss the hypothesis rather than providing a sustained argument against it.

Turning to the two concrete examples of the 'stop coda problem' that Song presents, the solution to 乍 MChi. *dʒa^C* / *dzraeH* 'suddenly' and 作 MChi. *tsâk* / *tsak* 'to act' appearing in the same phonetic series is that they descend respectively from OChi. *dʒrâkh / *[dz]ʳak-s and OChi. *tsâk / *[ts]ʳak,⁴ and the solution to the rhyming contact of 莫 MChi. *mɔk* / *maek*, *mâk* / *mak*, *muo^C* / *muH* 'end' and 除 MChi. *dʒwo* / *drjo*, *dʒwo^C* /

3 Song incorrectly cites this work as published in 2013.

4 An anonymous reviewer protests that I do “not offer any further evidence to prove that the -s in the -ks would not break the *xiéshēng* connection”. Many characters have two readings in different tones (e.g. 王 *jwaŋ* / *hɟwaŋ* < *waŋ / *G^waŋ and *jwaŋ^C* / *hɟwaŋH* < *waŋh / *G^waŋ-s) and a great majority of phonetic series contain readings of various tones (e.g. 垠 *lâŋ^C* / *langH*, 朗 *lâŋ^B* / *langX*, 糧 *lâŋ* / *lang*, etc.). The reviewer's apparent proposal that phonetic series must include only readings in the same tone would require a rethinking of historical Chinese phonology from the ground up.

drjoH 'to pass', as pointed out by Baxter (1992: 327), is that with the Middle Chinese readings 莫 *muo*^C / *muH* < OChi. **mâkh* / **m^hak-s* and 除 *djwo*^C / *drjoH* < OChi. **r-lah* / **lra-s* we have here a perfect rhyme of *-ah / *-as with *-ah / *-as, since this poem dates to after the simplification of **akh* / **-aks* to **ah* / **-as*.⁵

Because the 'stop coda problem' was solved in 1954 and lucid descriptions of its solution are available in numerous publications, some of which Song claims to have consulted, in principle there is no need to consider her own proposed solution. However, because many of her views about Tibetan risk spreading from her article to other publications, the arguments for her own solution to the 'stop coda problem' merit detailed refutation.

4 Orthographically voiced Tibetan finals

Concern that Song may lack sufficient familiarity with Tibetan to execute her goals begins at her selection of minimal pairs to exhibit the language's voicing contrast. A pair like བླ *ba* 'cow' and བཤ *pha* 'father' is more indicative than ལྒྱ *gwa* 'white forehead' versus ལྒྱ *kwa* 'alas'. The word ལྒྱ *gwa* was unknown to me before reading Song's article. She presumably takes it from the well known dictionary of Zhang, which defines it as ལྒྱགས་དང་རྩ་མོགས་ཀྱི་དབྱུང་བ་ནས་ ལྒྱ་ཙུའི་བར་དཀར་པོ་ *gnag phyugs-dan rta sogs-kyi dpral-ba nas sna rtsehi bar dkar-po* 'the white between the forehead and tip of the nose of yaks, cattle, horses, etc.' (1980: 379), not a prime candidate for core vocabulary. Both ལྒྱ *gwa* and ལྒྱ *kwa* contain the medial *-w-*, which is exceedingly rare in Tibetan vocabulary; the word ལྒྱ *kwa*, aside from being a clear case of onomatopoeia, begins with an unaspirated initial, a telltale sign that it does not belong to Tibetan inherited vocabulary (Hill 2007).

5 The 詩經 *Shijing* does provide examples of *qùshēng* words rhyming with *rùshēng* words, but these are cases in which *-*Vkh* / *-*Vks* rhymes with *-*Vk* / *-*Vk* or *-*Vts* / *-*Vts* with *-*Vt* / *-*Vt* (Baxter 1992: 318-319). The one example of 詩經 *Shijing* rhyming that Song discusses is not a case of this type.

Despite their exoticness, the pair ལྷ གྲཱ and ལྷ ཀྲཱ exhibit the fact that Tibetan has a voicing contrast in onset position.

Song's treatment of Tibetan continues with the claim that we “do not have historical records that describe the phonetic value of these sounds” (2014: 112). In fact, Tibetan sports a long and lively indigenous grammatical tradition that includes detailed discussions of phonetics (Miller 1976, 1993, Verhagen 1993, 2001). Untempted by the structuralist view that the underlying representation of a segment in a position without contrasts is itself a meaningless question, Song decides to 'take the position' that the orthographically voiced stops of in coda position were underlyingly voiced (2014: 112). As internal evidence in support of this view she cites the behavior of “a closed set of 'dependent particles', which include the sentence final particle, and the 離合 *lihe* particle” (2014: 112). She continues in a footnote with the remark that the “term 'dependent particles', is used by Tibetan grammarians because these particles are somewhat dependent on the neighboring phonological environment” (2014: 112). With her ignorance of the grammatical tradition's discussion of phonology in mind, it is gratifying to here see an acknowledgement that the Tibetan grammatical tradition indeed exists. Nonetheless, her view that this tradition writes in English and Chinese is baffling. I know of no Tibetan equivalent for the English word 'particle'; the Tibetans are lucky to lack such a vague and unhelpful notion. The '離合 *lihe* particle' (a term that is new to me) is called by the Tibetans འབྱེད་སྡུད་ *hbyed-sdud* and when teaching Tibetan in English I call it an 'interrogative converb'. Song notes that this and the རྩོལ་སྐྱོག་ *rdzogs-tshig* morpheme are written with the same letter than ends the preceding word. Normally this suffixation results in an apparent voiced geminate, e.g. བཅད་ལྷོ་ *bcad-do* 'cut'. Although the basic facts are right, Song misses a few important points. First, the ད་ལྷོ་ *da-drag* 'strong *d*' appears as a voiceless consonants in these contexts, e.g. ཕྱིན་ལྷོ་ *phyind-to*. If 'd' is underlyingly voiced, one may legitimately ask why it does not voice here. Second, she writes that when

“the preceding syllable has no coda” the need to have some onset “is satisfied by inserting the default onset in WT [ʔ] as the onset” (2014: 113). The letter used in this context is the 23rd of the Tibetan alphabet ལ, which Song herself posits as having the value /fi/ just two pages earlier (2014: 111).⁶ This blatant inconsistency suggests a lack of familiarity with the Tibetan alphabet.

Song suggests that because final stops are geminated as initial voiced stops they are underlyingly voiced as finals (2014: 115). She entertains another possibility, writing that those “who support setting the underlying form of <-g> as /-k/ may argue that there is an intervocalic voicing process in the derivation that changes the voicing of the stop” (2014: 115). One of the criticism she offers to this view, namely that “no other instance of intervocalic voicing is found in WT” is simply false. She has yet to learn of such doubles as ཡིག་ *yig* /jik/, ཡིག་གེ་ *yi-ge* /jige/ 'letter', ལུག་ *bug* /buk/, ལུག་གེ་ *bu-ga* /buga/ 'hole', and ཐག་ *thag* /thak/ 'weave', ཐག་པ་ *tha-ga-pa* /thagapa/ 'weaver'.

Song writes that gemination “apparently ceased to be active in a slightly later stage as documents from a later period no longer show signs of gemination” (2014: 116) whereas the “data used in this section comes [sic] from early WT text” (2014: 113 note 20). I am eager to consult the documents Song has access to; my own readings suggest exactly the opposite tendency. Newspapers and novels of our own day slavishly maintain gemination of these morphemes, whereas early literature often omits gemination.

Examining an Old Tibetan document from Dunhuang, Berthold Laufer long ago solved the mystery of apparent gemination.

In the Tibetan alphabet is developed the principle of writing separately each syllable of a word and of any composite

6 I have argued that the letter reflects /y/ (cf. Hill 2005, 2009).

formation; this, however, does not mean at all that what is separated by the use of the syllabic dot in writing presents also an independent part phonetically. ... we find *rdzogs-so* written in two syllables, and *rdzogso* written in one graphic syllable; the pronunciation is not *rdzogs so*, but *rdzogs-o*. In other words, this is not a case of phonetic, but merely of graphic reduplication, caused by the principle of writing. Likewise it does not make any difference from a phonetic viewpoint whether the Tibetan spells *gyurd-to* or *gyur-to*; phonetically it is neither the one nor the other, but *gyurt-o*. (Laufer 1914: 58, Tibetan transcription adjusted).

Spellings such as བཅདོ་ *bcado* 'cut', བཅགོ་ *bcago* 'broke' with clear intervocalic voicing (in place of བཅདདོ་ *bcad-do* and བཅགགོ་ *bcag-go*) are quite common in Old Tibetan texts, as a search of Old Tibetan Documents Online confirms (otdo.aa-ken.jp/ accessed 25 October 2014). The gorgeous architecture of derivations from underlying forms that Song proposes is unnecessary. Voicing is neutralized in coda position, consonants are realized as voiced intervocalically and are otherwise voiceless. The underlying value of Tibetan stop finals in coda position is a meaningless question.

5 Evidence of early Sino-Japanese

Song points to three words in which a Middle Chinese final stop to a voiced stop in a very early Japanese borrowing (2014: 118).⁷

麥 MChi. *mək* / *meak*, Jp. *mugi* 'wheat'

筆 MChi. *pjet* / *pit* 筆, Jp. *fude* 'writing brush'⁸

7 In this section, I am indebted to the comments of Sasha Vovin (per litteras 17 April 2015), which he later posted in a comment on a blogpost by Guillaume Jacques (<http://panchr.hypotheses.org/192#comment-214>).

8 SEE Miyake 1997: 187, 192.

物 MChi. *mjwət* / *mjut*, Jp. *mono* < *modo* 'thing'

Song admits that the interpretation of these Japanese words as loans from Chinese is controversial (2014: 118 note 21). Indeed, in the *opinio communis* two of these three are reconstructible to proto-Japanese: Jp. *Mugi* < OJp. *muNki*₁ < Proto-Jp. **munki* 'wheat' (Martin 1987: 487), Jp. *mono* < OJp. *mo₂no₂* < Proto-Jp. **mənə* (Martin 1987: 485). Song's reconstruction *modo*, which she omits an asterisks from, appears to be motivated only by a desire to present the word as a Chinese loan. The third word, Jp. *fude* < OJp. *puNte* 'writing brush', although obviously not reconstructible to a period before writing, is built with native morphology or may involve a Korean loan (Martin 1987: 416). Song's brushing aside of these standard explanations, in favor of merely positing her interpretation of these words as Chinese loans without explanation, cannot convince. At a minimum, she should have explained how the vowel correspondences are accounted for in an analysis of these words as loans. Finally, even if these three words were loans, they argue not for Chinese voiced finals, but for pre-nasalized finals, as voiced stops in modern Japanese originate from Old Japanese pre-nasalized stops and proto-Japanese clusters of a nasal with a stop (Martin 1997: 20-26).

6 Sino-Tibetan correspondences and 'the solution'

Building on the mistaken conclusion that Tibetan stop finals are underlyingly voiced, Song turns to Sino-Tibetan comparison. She rosily claims that “scholars have found the correspondences” Tib. *-b* : MChi. *-p*, Tib. *-d* : MChi. *-t*, Tib *-g*, MChi. *-k*, Tib *-Ø* : MChi. *- Ø* (2014: 116). We would be lucky indeed if this account were correct. To start, if one states things as she does using Middle Chinese rather than Old Chinese (in the tradition of Haudricourt) a number of widely made comparisons become extremely hard to state, e.g. Chi. 晝 *tjəu*^C / *trjuwH* < **trukh* / **truk-s* 'time of daylight', Tib. ཀལུགས་ *gdugs* 'mid-day, noon', Chi. 夜 *jiə*^C / *yaeH* <

*jakh / *N.rAks 'night', Tib. རྟམ་ *zag* < *r'ag 'day, 24hrs', Chi. 護 *yuó*^c / *huH* < *gwâkh / *[G]^waks 'guard, protect', Tib. འགོགས་ *hgogs* < *hg^wags 'prevent, avert', etc. (cf. Hill 2012).⁹ There are also cases where Chinese has a clear stop that corresponds to nothing in Tibetan: Chi. 日 *ńzjet* / *nyit* < *nit / *C.nik 'sun', Tib. ཉི་མ་ *ñi-ma* 'sun', Chi. 百 *pvk* / *paek* < *prâk / *p^rrak 'hundred', Tib. བརྒྱ་ *brgya* 'hundred', etc. (cf. Hill 2012). One wonders how Song explains such examples.

When the reader reaches the section entitled 'the solution', he witnesses the solving of a problem that was solved sixty years ago, fortified with a mistaken understanding of Tibetan phonology and a mistaken statement of Sino-Tibetan correspondences. The success of such an enterprise is doomed. The proposed solution is that Sino-Tibetan had a series of both voiced and voiceless stops that developed as follows: ST *-b > Tib. -b, MChi. -p, ST *-p > Tib. -Ø, Chi. -Ø. Song appears unaware that this explanation suffers from exactly the same criticism she leveled against the 'voiced stop coda hypothesis', namely that it yields a reconstructed language without open syllables. Other obstacles include the fact that -p is limited to 去聲 *qùshēng* syllables for no apparent reason. The suggestion that ST *-p > Tib. -b, MChi. -p, ST *- Ø > Tib. -Ø, Chi. -Ø would be a more neutral way to solve the same (faulty) correspondence, would solicit the answer we would need both *-p and *-b in early Old Chinese because *-p (equivalent to Li's *-b and Baxter's *-ps) has rhyme contact with *-b (equivalent to Li's *-p and Baxter's *-p) and *-b and *-p at times occur in the same phonetic series. We have come a long way to return to Li Fang-Keui's system in mirror image.

9 In the five Sino-Tibetan comparisons that Song gives (2014: 117) she uses the Roman letter 'j' for both ཅ་ /dʒ/ and ཇ་ /j/, cites the non-existent Tibetan verb form <chi> 'die' (intending either the present རྟམ་ *hchi* or the past ཇི་ *si*), and includes the word རྟམ་ *ḡag* /dʒak/ 'robbery', although this word, as an exception to Schiefner's law (*j- > ʒ-, cf. Hill 2014a), is unlikely to be inherited. Three mistakes in five words does not inspire confidence.

7 Lexical diffusion

Song begins the presentation of her own theory with the pronouncement that rhyme contact among words with Middle Chinese open and closed syllable readings “is not explicable under either of the two predominant opposing hypotheses” and “cannot be explained ... while firmly obeying the Neogrammarian law of sound change” (Song 2014: 106-107). Thus, she adds her name to the list of scholars who toll the knell of exceptionless sound change without first attempting to follow its principles (cf. Hill 2014b). In fact, as discussed above, both the 'voiced stop coda hypothesis' and the 'open syllable hypothesis' solve the 'stop coda problem' and do so without challenging Neogrammarian principles.

In place of regular sound change, Song puts her faith in 'lexical diffusion'. Without defending lexical diffusion, she writes “one fact is undeniable: lexical diffusion exists in Chinese, and its occurrence is far from rare” (p. 108).¹⁰ Lexical diffusion in Chinese is deniable; Pulleyblank and Egerod deny it. Pulleyblank points out that like the Neogrammarian hypothesis itself lexical diffusion is a research programme rather than a testable hypothesis, as “there is no possibility here of ever finally disproving the theory on empirical grounds” (1978: 184).

Although Wang may have originally formulated lexical diffusion as a refutation of Neogrammarian change, no scholar who supports lexical diffusion now outright denies the possibility of Neogrammarian regular sound change as one mechanism of linguistic change. So, if all linguistics agree that *pluralitas non est ponenda sine neccesitate*, the question becomes whether or not there is a need to add lexical diffusion as a new

¹⁰ Song claim that since “Neogrammarians first proposed this universal sound change mechanism, many other sound change types have been discovered, including lexical diffusion, borrowing, and analogy” (p. 107) is misleading. The Neogrammarians endorsed borrowing and analogy as explanations of language change (cf. Brugman 1879: 7-8). Lexical diffusion is the only newcomer.

mechanism of change. Pulleyblank believes the doctrine of diffusion is generally superfluous and that “there are many possibilities for rational explanation in terms of regular sound laws before one need be forced to accept that individual lexemes follow their own rules” (1978: 190). The doctrine is certainly superfluous in solving the 'stop consonant problem' in Old Chinese.

Egerod (1981) firmly rejects the touchstone example of lexical diffusion, namely the description of a Cháozhōu tone split given in Wang 1977. The force of Egerod's comparative discussion of French shows that the traditional Neogrammarian explanations offer more subtle and insightful accounts than lexical diffusion is capable of.

No linguist would dare to state that because Latin *correctum* becomes French *correct*, but *directum* becomes French *droit*, and because *-ent* and *-oit* both follow *r-* and are followed by zero, the only explanation is a sound change caught midstream, having reached *droit* but not *correct*. The existence of another form, French *direct*, immediately guides us toward a better explanation. Nor would he say that *validus* → *valid* and *calidus* → *chaud* allows us to draw any inferences about lexical diffusion. But this is precisely the logic followed in the Cháozhōu case. (Egerod 1981: 169-170)

Egerod's conclusion is that lexical diffusion is a methodological slight of hand, the dressing up of an aversion for toil as a theoretical insight.

there is no “massive split” involved, but an error of methodology in accounting for tones. Cháozhōu like other languages in China or outside of China has a complicated history with migration waves, loans and analogical formation. The conscientious historical linguist has to account for these before he resorts to a *deus ex machina*” (1981: 173).

Pulleyblank concurs with Egerod's explanation of the Cháozhōu tone split and more widely suggests that it “is not difficult to show that the various alleged examples of lexical diffusion in Chinese dialects are vitiated by the failure to take into account this ubiquitous fact of heterogeneity” (1982: 401). Pulleyblank regards the theoretical formulation of lexical diffusion as presented by Hsieh in Wang 1977 as “so manifestly at odds with any realistic picture of how dialects are inter-related and how innovations spread spatially through a language as to make them totally untenable” (1982: 408). Mazaudon & Lowe conclude a robust critique of lexical diffusion in a similar vein, remarking that “a detailed study of the history of the language can disentangle the reflexes from different sources, and it is not necessary to renounce the principle of regular change for the sake of such cases” (1994: 11).

Most textbooks of historical linguistics also reject diffusion as a mechanism of sound change distinct from regular phonetic change, borrowing, and analogy (Hock 1991: 649-652, Campbell 2004: 222-224, Ringe & Eska 2012: 79-83). Even sympathetic authors such as Crowley & Bowerman, who write of “lexical diffusion as being a major mechanism in language change” (2010: 216), mollify this commitment with the comment that “there is considerable debate on the extent to which diffusion is a mechanism of language change” (2010: 216 note 4 on p. 351). Bybee (2012), a theorist of *parole's* affect on *langue*, makes frequent mention of lexical diffusion as a factor in language change, but puts no stress on its distinctiveness from borrowing. If one mechanically substitutes Bybee's 'lexical diffusion' and 'change in progress' respectively with 'inter-dialectal borrowing' and 'synchronic variation' her presentation and her findings would accord with Neogrammarian doctrines.

Song touches on the famous example of Philadelphia accented /æ/ tensing as an example of lexical diffusion *par excellence* (2014: 108-109), yet even this case is consistent with Neo-grammarians principles (Blevins

2004: 270, Hill 2014b: 217). Lexical diffusion “is never an instance of sound change conditioned by phonetic factors alone” (Blevins 2004: 278); it is borrowing by another name. If Song believes that lexical diffusion is a type of sound change contravening Neo-grammarians principles which is distinctively essential to the theoretic arsenal of historical linguistics, she must answer in detail the many authors who disagree. To simply assert that that rhyme contact among words with Middle Chinese open and closed syllable readings cannot be explained except by lexical diffusion, is to abandon *Ausnahmslosigkeit* without giving it a try. Her claim that lexical diffusion in Chinese is 'undeniable' is simply false. Like so many others who would jettison exceptionless sound change in favor of exceptional sound change, Song will have her cake and eat it too. Lexical diffusion gives her a sound change (-p > Ø), but also let her whisk it away when it proves inconvenient. A magic wand to waive away exceptions, giving us a sound change in this word but not in that one, is not an explanation; it is an admission of failure.

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