

think one can make much of his *fidus Achates*). Curiously enough, however, one variant of the Aeneas legend emphatically associates him with homosexuality: the twelfth-century French *Roman d'Eneas*. In this poem, Queen Amata vociferously opposes her daughter Lavinia's proposed marriage with Aeneas on the grounds that he is a lover of boys! Whether Milton, for all his wide and profound reading, would have known this poem I cannot say, though it would be interesting to find out.

Apropos of the medieval background to Milton, I was intrigued by Bredbeck's narrowly limited discussion of Ganymede as an emblem of homoeroticism "within the vernacular of the Renaissance" (264). A brief mention, at least, of the similar symbolic use of Ganymede before the Renaissance would not come amiss. The medieval literary tradition of debate poems includes contests between homosexual and heterosexual love with titles like "Ganymede and Helen" and "Ganymede and Hebe" (see John Boswell's *Christianity, Social Tolerance, and Homosexuality*), and it seems clear that there is a continuity of tradition from the Middle Ages through the Renaissance. Incidentally, Boswell's study is extremely useful for obtaining an overview of the evolution of attitudes toward homosexuality in Western Christian society during the centuries leading up to Milton's time; the work may or may not have figured in the general background of Bredbeck's study, but explicit reference to Boswell could only enhance "Milton's Ganymede."

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To the Editor:

Gregory W. Bredbeck, in "Milton's Ganymede: Negotiations of Homoerotic Tradition in *Paradise Regained*," uses as "documentation of deviant sexual behavior" an attack against Elizabeth Cellier entitled *To the Praises of Mrs. Cellier, the Popish Midwife*. He alleges that this attack appeared in 1641, at the time of Milton's prose work *Of Reformation in England*, and argues from this supposed publication something about the sexual context of that era (263).

Unfortunately for his argument, Cellier flourished something like forty years after this date, in 1679–88, and could not possibly have been attacked in print in 1641 or even in Milton's *lifetime*.

This misdating is a reminder of the real risk involved in writing an essay with a strong ideological bent while using historical data chiefly for ornamentation.

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Reply:

Randi Eldevik's observations are absolutely fascinating and deserve to be worked up into a full article. In the book based in part on my essay, I touch briefly on some of the medieval traditions, and I am of course aware of Boswell's work—but I do not cover the issues in a way that precludes Eldevik's addressing them. As her letter so helpfully points out, there is much more that can be said about my topic—and I look forward to seeing others take up this task.

I thank Anne Barbeau Gardiner for the factual correction, particularly since it arrived in time for me to alter my book. There is indeed a broadside account of the Cellier controversy dated 1641, and this date has been transferred in pencil to two other accounts, all of which are bound in the British Library in a volume of broadsides inclusively dated 1600–50—hence my confusion. I am most intrigued by Gardiner's final sentence, for it addresses neither how one might write an argument without an ideological "bent" nor the ideology implicit in her own desire to keep the facts "straight."

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The Future of Grimm's Law

To the Editor:

I am greatly disturbed by Zacharias P. Thundy's reply to Edgar C. Knowlton, Jr. (Forum, 106 [1991]: 309–11). As though Knowlton's criticism of his former remarks (Forum, 105 [1990]: 1127) were not sufficient, Thundy now offers a number of considerations on the comparative method. Putting aside the origins of *cēosan* and *taste*, I would like to comment on the following statement by Thundy: "To me [Knowlton] seems to imply that we should accept past linguistic scholarship as authoritative and unquestionable. On the contrary, I hold that all scholarship, especially study of the origin of the language families, is very tentative. This qualification applies to the laws of Indo-European, particularly Grimm's law, which governs the reconstruction of the consonants" of many Proto-Indo-European roots. Thundy goes on to say that "[m]ost Indo-Europeanists cite the many laws of Indo-European as gospel truths even though scholars have fought and continue to fight over them, and there remain many honest doubts about them."

The reason Thundy is "skeptical" of many Proto-Indo-European roots based on Grimm's law "is that

most Indo-Europeanists apply Grimm's law universally and claim that there is no exception to it" (310). Against this background, he praises Joseph Greenberg for lacking dogmatism. "Traditional linguists attack as unscientific Greenberg's unconventional methodology of comparing common-sounding words across many languages without trying to formulate laws of sound shifts. However, what Greenberg did twenty years ago in the classification of African languages is now widely accepted" (311; the criticism by retrogrades, we are told, is directed at Greenberg's work in the classification of American Indian languages). Since Thundy is apparently not a traditional linguist, my remarks will be elementary, but it is nobler in the mind to "choose the deathbed" of triviality than to allow adventurism to pass itself off as scholarship.

Sound laws are formulas of correspondence. Some of them affect hundreds of words, others a mere handful. Etymology is not algebra, and no lexicon can be reconstructed only with the help of such formulas. There are borrowings, analogy, *Mischformen*, folk etymology, rhyming slang, sound symbolism, onomatopoeia, and so on. Despite all the laws, the history of many words may be beyond recovery, but Thundy is deeply mistaken in believing that, according to the Indo-Europeanists' assumption, Proto-Indo-European "was a simple, self-contained pidgin language with no dialects and no interaction with other language families" (310). Exactly the reverse is true. For more than a century Indo-European dialects have been at the center of comparative linguistics, and the same holds for contacts between the Indo-Europeans and the population they allegedly subjugated. We may never learn whether the ancient language of the Indo-Europeans formed a relatively monolithic whole or whether it emerged as a language union whose common features are the result of later development. The truth can lie on unpredictable paths, but no one reconstructing Indo-European believes in "a simple, self-contained pidgin language" existing in a vacuum (and how could it become pidgin if it was self-contained?).

Etymology is useless without sound laws. In the absence of such laws we can only compare words having approximately the same meaning and sounding alike. This is what etymology did from the days of ancient Egypt to the nineteenth century, and this is what Thundy wants it to do now. There is no need to mock at the gospel truths of the comparative method. Thundy derides philologists who refuse to connect Latin *habeo* and Old English *habban*. Anyone can see that these verbs are cognates, and only a group of entrenched conservatives refuses to "follow the hermeneutics of skepticism" (311). But Latin *h* does not normally cor-

respond to *h* in Germanic, so why should a special dispensation be made for *habeo-habban*? During the many centuries of coexistence, the Romance verb could have influenced its Germanic synonym, but they are not cognates. Neither are English *heath* and *heather*, Russian *moloko* 'milk' and *milk*, and a host of others like them.

Sound laws are circular. We first agree that *pater* and *father* are related and then formulate the rule, "Latin *p* || Engl. *f*." As a result of this inherent flaw, we are faced with hundreds of words that may or may not be related. Etymologists seek a correspondence that will ideally cover all cases; when the formula fails, alternative hypotheses are tried. Pairs like *pater-father* are few, and eighty percent of etymological work consists of looking for respectable cognates. It is one thing to accept the law that prohibits Latin *h* from corresponding to *h* in Germanic and then attempt to account for *habeo-habban*, and it is something quite different to treat the law lightly and allow any two seemingly eligible candidates to be related.

Of all the laws of historical phonetics, Grimm's law is easily the best. Exceptions to it are famous. Is it possible that Thundy never heard of an article entitled "Eine Ausnahme der ersten Lautverschiebung"? Thundy quotes Collinge, in whose opinion Grimm's law is about to expire. I hasten to assure Thundy that this law will survive all of us. Whatever the status of *media aspirata*, correspondences like *pater-father* will forever remain a cornerstone of Indo-European etymology, and no theory will abolish the fact that *moloko-milk* does not belong with *quod-hwæt*.

"We can and we must try to discover some provisional underlying order in the aberrant behavior of languages because doing so is fun," says Thundy (310). Fun indeed when a professor of Old English appeals to his colleagues to "be creative at the risk of being wrong . . . and challenge 'established' scholarship in comparative linguistics"! As a coeditor of *A Bibliographical Dictionary of English Etymology*, I know that numerous English words have no connections outside English, and anyone's (intelligent) guess on their origins is welcome. Other words have cognates everywhere, and they were discovered only because the basic principles of etymology were observed. If we follow Thundy's advice to be "creative" at the expense of the "sickening" and "expiring" laws, we will end up in the company not of Gamkrelidze and Ivanov but of their compatriot Marr, another great skeptic who was ready to challenge the establishment. *Dixi et animam meam salvavi*.

ANATOLY LIBERMAN
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Reply:

Anatoly Liberman defines the comparative method and defends its importance in historical linguistics. He also makes a strong case directly for Grimm's law and indirectly for Verner's law while seeming to imply that critics of these laws are trivial adventurists and that Grimm's law will endure.

I have no quarrel with the basic theoretical model of historical linguistics; I have raised my objections only to its practice. There is, indeed, something wrong with the praxis of historical linguistics: the discipline has reached an impasse, a fact recognized or deplored by distinguished linguists like Hjelmslev, Allen, and Lehmann (*Language* 50 [1974]: 623). The decline of the once-popular historical linguistics as an academic discipline is evident from Watkins's observations that one can receive a PhD in linguistics at a number of fine American universities without taking a course in historical linguistics, that Newmeyer's influential work *Linguistic Theory in America* (1980) makes no mention of the comparative method, and that Newmeyer's hefty four-volume, 1500-page *Linguistics: The Cambridge Survey* (1988) devotes only about 110 pages to language change and does not focus at all on historical linguistics (*Language* 65 [1989]: 783, 798).

Though historical linguists like to consider their discipline a science, they should also bear in mind that, however scientific its method is, the field is different from the physical sciences in that Indo-Europeanists often deal with scant, unstable phonological data and with some lexical data that were never observed at the time for which the laws were formulated. Most of the factors that influenced language changes during that prehistoric period are unknown to us. So doubts and challenges will continue to plague the neat formulations of the laws of Indo-European. Consequently, Kant's scientific criteria of universality and necessity cannot be applied to the laws of Indo-European. On the contrary, historical linguists must—and do—rely on the reformable, subjective interpretations that historians thrive on. An example is the variety of exciting theories on the Indo-European homeland, which, I like to believe, is central Asia, the home of the Tocharians.

Let us briefly consider the two laws Liberman refers to: Grimm's law and Verner's law. (An English translation of Verner's paper "Eine Ausnahme der ersten Lautverschiebung" 'An Exception to the First Sound Shift' can be found in W. P. Lehmann, ed., *A Reader in Nineteenth-Century Historical Indo-European Linguistics*, Bloomington: Indiana UP, 1967, 132–63. Incidentally, the exception mentioned in the title happens to contain many exceptions.)

Though Collinge thinks that Grimm's law is about to expire, Hopper, another critic of the law, admonishes us that Grimm's law "will not be ceded without a struggle" (N. E. Collinge, *The Laws of Indo-European*, Philadelphia: Benjamin, 1985, 265; for bibliographies on glottalic theory and Grimm's and Verner's laws, see 71–76, 211–16, 265–69). Liberman is doing exactly what Hopper warns us about. Why not hang on to the canonized Grimm's law, which has been around for over 150 years? Liberman seems to forget that the exceptions to Grimm's and Verner's laws would render those laws invalid in other scientific disciplines.

Classical theorists like Grimm divide "stop" consonants into the three categories *p, t, k* (voiceless—*tenues*); *b, d, g* (voiced—*mediae*); and *bh, dh, gh* (voiced aspirates—*aspiratae*). By contrast, glottalicists like Gamkrelidze and Ivanov have developed the following scheme: *p', t', k'* (glottalized stops, in which the throat closes at the vocal cords to prevent the outward flow of breath); *b/bh, d/dh, g/gh* (voiced/voiced aspirated stops); and *p/ph, t/th, k/kh* (voiceless/voiceless aspirated stops). According to Gamkrelidze and Ivanov, among the glottalized stops, *p'* was suppressed in Proto-Indo-European (PIE), and *p', t',* and *k'* occur in language families like North Caucasian and South Caucasian (Kartvelian), though the glottalized stops tend to disappear in most language families. The glottalicists' conclusion that their reconstruction of the PIE consonant system is closer to the consonant systems of Germanic, Armenian, and Hittite than to Sanskrit rejects the classical theory that the former languages have undergone a systematic sound shift and that Sanskrit had preserved the original sound system. Gamkrelidze and Ivanov illustrate how the glottalic system affects the PIE reconstruction of *cow* (English) or *Kuh* (German). In Sanskrit the word is *gauh*, and in Greek it is *bous*. In the glottalic system the PIE word is **k'ou*, which is phonetically closer to Germanic than to the Sanskrit *gauh*; in the classical system followed by current etymological dictionaries, the PIE word is **gwou*. Gamkrelidze and Ivanov argue that the glottalic system makes better sense because it does not require Grimm's law and because it correlates Germanic with PIE (*Scientific American* March 1990: 110–16).

Verner's law also suffers from inconsistencies, and genuine scholars have raised serious objections about its phonetic mechanics and chronological parameters (Collinge 207–11). For instance, the Gothic strong verb where we find the voiceless variant in the root-final position militates against Verner's law. If Sanskrit does not necessarily retain the archaic feature of PIE stress—there is no evidence that it does—Verner's application of shifting stress to Germanic raises problems. Stress

is not characteristic of spoken Sanskrit and modern Indic languages; the *udāta* (“raised”) and *svarita* (a combination of *udāta* and *anudāta*) discussed by Panini in his grammar are musical tones and not a matter of stress. Verner’s comparison of shifting Sanskrit syllabic lengthening to shifting Germanic stress is highly questionable simply because no voicing or unvoicing takes place in the Sanskrit verbs, such as *pat*, *cit*, *vṛt*, and *vep*, unlike in some Germanic verbs. Is it not possible that Verner’s law of voicing is due to some other factor—for example, the influence of another language on Germanic? Commenting on the many exceptions to Verner’s law and on the noted exceptions to Kuhn’s law, Bruce Mitchell, the distinguished author of *Old English Syntax* (Oxford: Clarendon–Oxford UP, 1985), writes: “If caution and acquiescence, rather than enterprise and independence, were the primary attributes for explorers or inventors or scholars, we might still believe that the world is flat and might live in a world without internal combustion engines and television sets” (*NM* 111 [1990]: 290). We must continue this questioning of Grimm’s law as well as of Verner’s. Then, only then, can we come up with new discoveries.

Finally, the defenders of sound laws assume that languages are too systematic and that they even exhibit rule-controlled mechanical changes. Languages are, however, as Dr. Johnson observes, “very often . . . capriciously conducted.” Of course, I would not dismiss summarily the old lawmakers of languages. I still teach my students Grimm’s and Verner’s laws, but I add that Grimm and Verner are not the final authorities and that linguists and scientists alike are fallible. So, as I carp at the empty promises of scholars and smile at the vanities of scholarship, I still foolishly cherish this Panglossian hope: the best is yet to come.

ZACHARIAS P. THUNDY
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Jonsonian Theme Parties

To the Editor:

I read with interest the letter of Gary Schmidgall and reply of Bruce Thomas Boehrer in the March issue (*Forum*, 106 [1991]: 317–19), not only because Ben Jonson’s “Inviting a Friend to Supper” is a poem in which I have long taken superficial and gustatory delight (and I would have *loved* to sit down to table with Jonson) but also because it is a pleasure to see two men accusing each other of lacking a sense of humor.

But the proof of the pudding is in the eating, and we ask of a poem whether and how it “works.” One of the ways Jonson’s poem works is—no surprise—as an invitation to a meal. As such I have tested it twice, and it served admirably both times.

Perhaps the most memorable of the events that Jonson’s “Inviting a Friend to Supper” helped to convene occurred in the spring of 1982, in Normal, Illinois. There were fourteen of us in all, adults and children, and I am sure all the ecumenical group would agree that it was a glorious yet secular Easter afternoon—“the forms and bounty of American holiday dining” indeed!

Surely Jonson knew what Schmidgall and Boehrer haven’t recognized—that no host or hostess ever occupies “an absolutist position . . . seeing all, controlling all, and defining all,” as Boehrer writes in his essay (“Renaissance Overeating: The Sad Case of Ben Jonson,” 105 [1990]: 1071–82; 1075). As for me, I never knew who broke the wineglass or what was done with the pieces; attempted conversational gambits were prone to being interrupted by bons mots like “Helene, is the mixer *always* supposed to emit smoke like this?”; and hospitality became the composing of such remarks as “*Of course* you may play with the teddy bear, darling, but try not to throw up anymore” and the deciphering of overheard comments like “He feeds her *Friskies*?” (part of a discussion of the new movie *Cat People*). We had touch football on the lawn among scatterings of crocus and late-spring snow, and we took group photos minus one (the cook). Though the excruciatingly expensive and arduously concocted homemade country pâté never “worked” (and it turned me off forcemeat for months), the bountiful meal was bountifully enjoyed (and a whole ham was surplus), and the archaism of the meal (and its invitation) provoked the responding archaisms of notes and flowers next day—in short, no little thanks to Jonson, a swell time was had by all.

It may seem trivial that I have responded to the quarreling of Schmidgall and Boehrer with my memoiristic application of Jonson’s “Inviting a Friend to Supper” to an occasion the poem inspired. It probably is. But an argument, too, is a meal, and such as Schmidgall and Boehrer, who would attempt to partake without properly observing the party clothes and spirit, I would send back to Jonson:

It is the fair acceptance, sir, creates
The entertainment perfect, not the cates.

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