

## RESPŌNSIŌ AD DISPUTĀTIŌNEM

I am grateful to Mrs. Anastassia Brodetskaya (īnfrā = B.) for the time and effort she has devoted to the careful examination of some of my recordings.<sup>1</sup> I regret that I must disagree with most of her conclusions. The following are some of my reasons (I follow the pagination of B.'s article).

**Pp. 352–353. Methodology.** B. states that for her investigation she used (a) certain laboratory instruments, (b) an assembled audience, and (c) her own auditory response. B. does not make clear however *which* of her conclusions were based upon *which* of the above criteria, making it often impossible to evaluate the basis of her conclusions. Furthermore, it is axiomatic that no scientific experiment can be considered valid unless the original results can be replicated by other independent researchers. B. gives no indication that such replication was made.

**P. 354.** B. declares that she used for her investigation recordings of the Zhamajtian dialect of Lithuanian and recordings of Norwegian, but she does not establish their *necessary relevance* to ancient Greek phonology.

**P. 355.** B. speaks of my using a “twin-humped” structure in diphthongs (I am not acquainted with “twin-humped” as a linguistic term in English), but she does not give any specific reference as to *where* in my recording it is allegedly used. How can her claim be checked?

**Pp. 356–357. Accent.** B. states that the acute accent ascends only on the second mora of a long vowel. This is wrong. An acute accent glides upward in pitch over the *whole* long vowel, beginning in the first mora and reaching its *summit* at the end of the second mora, with the pitch then descending in the following syllable(s).<sup>2</sup> B. further states that I do not descend after the acute accent, giving a number of alleged examples (μετ-έειπεν, περίσχεο, χερειότερον, Εύρύπυλος). In each of the words cited, the acute accent is found on the antepenult. What B. fails to observe is that in each case (I listened carefully) the pitch *partially* descends from the antepenult to the penult, and then *fully* descends on the ultima. This is completely in accord with the description given by Dionysios of Halicar-

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<sup>1</sup> “Notes to the Reconstruction of the Ancient Greek Pronunciation...”, *Hyperboreus* 3 (1997): 2, 353–361.

<sup>2</sup> Cf. Allen. *VG* (1987) 122.

nassos as cited by Allen (*VG*<sup>3</sup>, p. 123), and is probably the phenomenon that certain Greek grammarians refer to as μέσος τόμος.

B. states that I produce many metrical mistakes and accentual inaccuracies caused by my failure to distinguish clearly between long and short vowels. She then gives four examples of allegedly mispronounced circumflex accents. I listened carefully to the four examples cited and found that these circumflex accents are *correctly* pronounced (highest pitch at the end of the *first* mora, descent in the *second* mora). Now it would be unconvincing for me to claim here that *my* ears or *my* auditory response is more accurate than that of B. To counter B.'s claim of metrical mistakes, however, I can offer the testimony of ears *other* than my own. In addition to my fifteen recordings of ancient Greek literature, many of which have been professionally reviewed in classical journals, I have given over one hundred recitals of ancient Greek in various universities throughout the world (including St. Petersburg and Moscow). Although some reservations have occasionally been made in writing, or orally, concerning certain aspects of my performance, *no* reviewer and *no* listener has ever raised the question of *metrical irregularity*. As a matter of fact, let me quote from a review of my recording of *Iliad* 1–6 by D. Seward of the University of Pittsburgh:<sup>3</sup> “Daitz has rendered the meter with such accuracy that one can easily “scan” each line with the ear alone. I venture to assert that a Greekless listener could make an accurate phonetic transcription of these recordings, so much care has been given to the accurate reproduction of the printed text”. (I subsequently learned that D. Seward was not only a classicist, but also a professional violinist, presumably a person of particular sensitivity to matters of pitch and rhythm.)

**P. 358.** B. states that my pronunciation of the letter *eta* is often indistinguishable from that of long *alpha*. The reason for this perception may well be that the phoneme for *eta* [ɛ̄] does not exist in the languages of those listening, and so the sound heard would be identified with the sound in the listener's language closest to [ɛ̄], namely [ā]. In other words, for such listeners [ɛ̄] would be perceived virtually as an allophone of [ā]. This is certainly the case with speakers of English where the phoneme [ɛ̄] does not exist. My own students would often at first not hear the difference between *eta* and long *alpha*, but after I carefully pronounced the words *νεῆ*, *θεῆ*, then they clearly perceived the distinction.

**P. 359.** B. states that in many instances I omit the rough breathing (*spīritus asper*) on a number of initial vowels, giving five examples. I listened

<sup>3</sup> *Classical World* 85 (1991): 1, 66.

carefully several times on my cassette to these examples, and in each case I could definitely hear the aspirations, some louder than others, but all perceptible. The only explanation I can offer for this discrepancy between my perception and that of B. is that either the specific cassette or the audio equipment that B. used was somewhat defective.

**P. 360.** B. seems somewhat confused about the pronunciation of sigma before voiced consonants. According to Allen (*VG*<sup>3</sup>, p. 45–46), sigma before *all* voiced consonants becomes voiced, [z], in classical Greek. This is the practice that I have followed.

B. asserts that in my recording, *The Pronunciation and Reading of Ancient Greek*, the stops (occlusives) τ/θ, κ/χ, π/φ are *all* pronounced as aspirates. I listened carefully to all the passages cited, and in each case could hear a *discernible difference* between the aspirated and the non-aspirated stops. Again, the only explanation I can offer for this discrepancy of perception is that the cassette or the audio equipment used by B. was somewhat defective.

B. claims that initial rho should not be aspirated. But the ancient grammarians and linguistic evidence all indicate that initial *was* regularly aspirated (cf. *VG*<sup>3</sup>, p. 41).

Apart from my disagreement with most of B.'s specific conclusions, for reasons stated above, I regret deeply what seems to be the author's negative and sceptical attitude towards the attempt to restore as much as possible of the authentic sounds of ancient Greek literature. For well over a thousand years these authentic sounds were not heard and then were forgotten. Only in the past hundred years have international linguists been able to bring together all the ancient and modern evidence for an accurate reconstruction of the phonology of ancient Greek. And it is only in the last twenty years that some classical scholars, including myself, have attempted with the restored pronunciation, to apply the results of recent linguistic research to actual oral performance of ancient Greek literature. Although I disagree with most of B.'s specific conclusions, I am very much aware of the difficult challenge of such a pioneering attempt. It is for this reason that I wrote in the Preface of the booklet accompanying my recording of *Iliad* 1–6 (p. ix): "it is inevitable... that in this first recording of the entire *Iliad* in the restored pronunciation of ancient Greek, there be some slips of the tongue..." But I add the words of Cicero: "nihil est simul et inventum et perfectum", ending with "it is my hope that others will improve upon my efforts".

Instead of almost entirely emphasizing what she perceived to be the *negative* aspects of my attempt, I would have hoped that B. would mention

as well the *positive* achievements, precisely so as to encourage others to improve upon my efforts. The bottle of wine may be half empty, but conversely it is also half *full*. With continued research and improved oral performance by scholars who strive to recapture and to re-create the unique music of ancient Greek literature, the bottle will gradually become more and more full.

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*Moderātōrum arbitrium*

Публикацией *Respōnsiō* проф. Ст. Дайтца редакция твердо намерена завершить на страницах *Hyperboreus* полемику между ним и А. Бродоцкой. Редакция полагает, что публикация критической статьи г-жи Бродоцкой (*Hyperboreus* 3 [1997]: 2, 353–361) была несколько опрометчивой. У редакции вызывает сомнение правомерность самой постановки вопроса. В самом деле, что означает “древнегреческое произношение”? Идет ли речь о временах Гомера? Или о классическом периоде? Думают ли гг. Дайтц и Бродоцкая о произношении афинян V (IV, III, II etc.) вв. до Р. Хр. или о *выговоре* милетян (спартанцев, фиванцев, фессалийцев etc.)? Отсутствие исторической конкретизации усугубляется к тому же предлагаемой проф. Дайтцем живой реконструкцией, где артистическое поневоле начинает преобладать над научным. И если, прослушав записи проф. Дайтца, г-жа Бродоцкая утверждает, что “the stops (occlusives) τ/θ, κ/χ, π/φ are all pronounced as aspirates” (Daitz, *Resp.*, 3 f.), то в ответ проф. Дайтц настаивает на том, что “each case [I] could hear a *discernible difference* between the aspirated and the non-aspirated stops”. Большая часть разногласий оппонентов – того же рода. Редакции поэтому приходится сделать вывод, что речь идет по преимуществу об индивидуальных особенностях произносительного resp. слухового аппарата двух исследователей, что придает дискуссии чересчур специальный характер.