THE 1337 BURSA INSCRIPTION AND ITS INTERPRETERS

PRELIMINARY STATEMENT

The purpose of this Preliminary Statement is to place the present article in two disparate contexts, one of a piece of work which has its own history; the other of justification and explanation of its mediated appearance in this issue of Turcica.

The article, as it here appears, was sent to M. Vatin, in his capacity as one of the editors of Turcica, on 15 May 2003. It had started life as a short note, entitled ‘Wittek and the Ghazi Thesis: the Foundation Inscription of 738/1337-8 from Orkhan’s Mosque at Bursa’, which was intended to appear as an Appendix to the published version of a paper which I gave at a conference entitled ‘The Frontier in Question’, held at the University of Essex in 1995. As it turned out, my Essex paper eventually appeared some four years later in a format which, while entirely scholarly, was designed more for the general reader than as a dedicated record of the conference proceedings, and in which the Appendix was perforce omitted.1 I continued to work intermittently on the former Appendix during the next few years, and was able to benefit from some insightful observations made on its text at that time by Professor V. L. Ménage, for whose characteristic kindness I remain extremely grateful. I remained uncertain whether or not to publish the would-be Appendix as a separate article, but on 30 March 1998, following a telephone conversation between us, I despatched a copy of this ‘post-Ménagian’ version to Professor Heath Lowry, who was also at that time attempting to come to terms with the contradictions inherent in the 1337 inscription and Wittek’s use of it. As I wrote to him at the time,

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‘My purpose in writing… is to enclose… a print-out of my still unpublished (and, in footnote terms, partly unfinished) attempt to explain (or at least flag out) the still uninvestigated riddle of why Wittek made use of the 1337 inscription for purposes of wide-ranging historical explanation, yet, as a superb epigrapher, almost unbelievably both failed to study it in forensic detail and at the same time suppressed what he already knew in 1937 from his own research six years earlier, that one of its most crucial elements (for his ghazi thesis explanation of the Ottomans) was not only not unique, as he claimed, but led straight back to their [scil. the Ottomans’ CH 2004] antitheses, the Seljuks of Rum. Since you are grappling with the same problem, I would be very interested to have your comments on my piece, before I finally decide (or not) to publish it.’

Nothing immediate came of this overture, but my recollection is that Lowry and I discussed the problem further, in a desultory way, when I spent some time at Princeton in the spring of 2000, but by that time my current research interests had moved on and away both in time and place from fourteenth-century Anatolia, though not entirely from the Wittekian problem. Finally, as mentioned above, and as chance would have it, in May 2003 I decided that, in the absence of any publications to date dealing with the ‘1337 problem’, and on the urging of two colleagues for whose opinions I hold a high regard, I would venture to offer the article to the editors of *Turcica*.

Within a few weeks of doing so I became aware that in fact my study of the 1337 problem was not fated to exist in isolation. Firstly, I became aware of Mme Beldiceanu’s brilliant study in *Turcica* 34 (dated 2002, but published in 2003) entitled ‘Analyse de la titulature d’Orhan sur deux inscriptions de Brousse’. It was a great pleasure to me to discover that Mme Beldiceanu, while approaching the problem of the language and vocabulary of the 1337 inscription from an entirely different angle, also came to an essentially ‘demythologising’ conclusion regarding it. Shortly thereafter, through the kindness of Professor Lowry, I received an early post-publication copy of his long-awaited study of the early Ottoman state, in which I was glad to see that he had given ample space to a study of the 1337 inscription in which elements of the present article as well as some of my earlier work on Wittek as a historian had found a prominent place.

At this point the present article was still before the editorial board of *Turcica*. After an interval of some months, I was pleased to discover (let-

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2 Personal letter to Professor Heath Lowry, 30 March 1998.
5 Heath W. Lowry, *The Nature of the Ottoman State*, Albany, NY, 2003; cf. especially chapter 3, p. 33-44, ‘Wittek revisited: his utilization of the 1337 Bursa Inscription’. I should add that Professor Lowry was kind enough to furnish me with a print-out of the manuscript of this work in 2000; I felt, however (perhaps erroneously), that I should not make use of it before the work itself achieved publication.
ter of Mme Dupuy, 24 February 2004) that the article would be published, in the company of a further note by M. Kalus, who had served as the paper’s evaluator. It was not until June 2004 that I received the text of M. Kalus’ paper, from the study of which it became clear that he disagreed with my readings and interpretation on a number of vital points. At this juncture I was faced with a difficult decision, whether or not to amend my paper in the light of his criticisms. My first intention was to do just that, but on reflection I have decided that it should stand (with a number of minor typographical mistakes corrected) and should appear with and be read with M. Kalus’ alternative version. I take this position in the light of the undoubted fact that the present article has already become a historical document in its own right. Firstly, it reflects my own thoughts on the subject as they had developed over nearly a decade down to the submission of the manuscript to *Turcica* in February 2003; indeed, I feel strongly that M. Kalus’ criticisms of certain of my readings, valuable as they are, may still not be the last word on the subject. Secondly, I would hold that they leave unaffected the main, historiographical thrust of my article, which was to place Wittek’s use of the 1337 inscription under the microscope, as it were, and to place it equally in the historiographical and historical context of the period in which it was formulated. Thirdly, I would wish the text of my 2003 submission to stand, in the light of the use made of it post-1998 by Professor Lowry.

It is in the context of the above observations, therefore, that I would request MM. the editors of *Turcica* to allow the following short study to appear in the form in which I had originally submitted it.

Colin Heywood  
Hull, 30 November 2004
In a now not so recent paper I offered a reexamination of the question, fundamental to our understanding of early Ottoman history, as to whether the *uj* (*Tu.∞: uc*), the frontier zone of the Ottoman state, as it existed in the fourteenth and first half of the fifteenth centuries, was more — or possibly less — than what historians have made of it. Critical to any re-evaluation of the Ottoman frontier is the continuing debate over the existence and true nature of the *ghāzī*, and whether the Ottoman state was, or was not, a ‘*ghāzī* state’. Our starting-point thus must be what has come to be termed the ‘*Ghāzī Thesis*’, put forward more than sixty years ago by the late Professor Paul Wittek (1894-1978). Wittek, as is well known, held that the Ottoman Empire, in its most essential form, was a *ghāzī* state, founded on the principle (and practice) of Holy War (*ghāzā*), waged by warriors for the faith (*ghāzīs*) against the Infidel. Wittek based his formulation on two Ottoman sources. These were, respectively, an inscription dated 738/1337-8 on the Şehādet Mosque at Bursa; and an Ottoman verse-chronicle inserted into his reworking of the Alexander romance by the late-fourteenth/early fifteenth century Turkish poet Aḥmedī. Although the existence of both the Bursa inscription and the verse-chronicle of Aḥmedī was known to scholarship at the time Wittek wrote, neither source was regarded as being of particular significance for understanding the history and, more importantly, the ideology of the early Ottoman state. Subsequently, as a direct result of Wittek’s forceful and apparently convincing advocacy, this situation was to be completely changed, and for half a century and more both Aḥmedī and to an even greater extent the 1337 Bursa inscription came to be regarded as the two pillars which supported the imposing arch of the Wittekian canon regarding the early Ottoman state and its essential nature.

Our concern in the present paper is only with the Bursa inscription of

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8 On Aḥmedī, in the present context, see Wittek, ‘Deux chapitres’, *op. cit.*, p. 304-305, 312.
1337, on which, as being what he termed ‘the oldest epigraphic document we have from an Ottoman ruler’, Wittek leaned the most heavily to support his ghāzī thesis. Wittek referred to the inscription in passing in several of the works which he published between the late 1920s and the eve of the Second World War, but most significantly he gave it considerable attention in the lectures which he delivered at the University of London in 1937 and which represent both a summary and the culmination of his historical formulations during these years. More specifically, Wittek referred to the 1337 inscription in the final paragraph of the first of his three London lectures to provide the clinching evidence for his assertion of the essential ghāzī nature of the Ottoman state. In a footnote to his first lecture Wittek supplied the text of the relevant section of the inscription, in Latin transcription:

Sulṭān ibn sulṭān al-ghuzāt, ghāzī ibn al-ghāzī, Shujā‘ ad-daula wa’d-dīn, marzbān al-āfāq, bahlavān-i jihān, Orkhān ibn ‘Othmān.

Wittek’s translation of this section of the 1337 inscription, which he inserted into the body of his lecture, and which has been utilised subsequently at second hand by most commentators, omits the third and the final (sixth) members of his text, and reads as follows:

Sulṭān, son of the Sulṭān of the Ghāzīs, Ghāzī, son of Ghāzī, marquis of the horizons, hero of the world.

It is clear that by 1937 Wittek was possessed of absolutely no doubts about either the integrity of his text, or the correctness of his reading of it, or its specific historical significance as supplying unquestionable proof of his ghāzī thesis. Raising, in order to answer it, the question as to whether or not Ahmedī’s ghāzī testimony, previously mentioned, should be dismissed as ‘mere literary form’, he responds that a glance at the 1337 inscription ‘will dissipate all such doubt’. Referring to the translated members of the inscription, he describes them as ‘an ensemble of titles absolutely unique in the Ottoman protocol (my italics, CH),

9 I would like to express my gratitude to Professor V. L. Ménage, for his insightful observations some years ago on an earlier version of this paper. My grateful thanks are also due to Colin Imber, Rudi Lindner and Caroline Finkel who at various times have read and responded to the present version. For the form in which it has finally appeared I accept full responsibility.

10 Wittek’s London lectures were published as The Rise of the Ottoman Empire, London, 1938. For the background to the lectures and a more extended discussion of their intellectual consequences, see my ‘Wittek and the Austrian tradition’, op. cit., p. 7-8, 11-12.


12 Rise, op. cit., p. 53, n. 27.

13 Rise, op. cit., p. 15.

where, as he observes ‘generally the classical and quite different formulas of the Seljuk period are used’. Historically, too, the inscription raises for him no uncertainty: with the affirmative and confident assertion that ‘we can… be sure that this strange formula is the expression of an historical reality, of the same reality which dominates the chapter of Ahmedi’, Wittek concluded his first lecture.

Recent scholarship has criticised, often in strong terms, many of Wittek’s historical formulations. Criticism has also been levelled in particular at his utilisation of Ahmedi and the 1337 Bursa inscription as the two ‘legitimating’ sources for what may be termed his ghâzî thesis. In the course of preparing my 1995 paper on the Ottoman frontier it appeared to me all the more remarkable therefore, that not only did Wittek never supply his own edition of the inscription in question, but that recent Wittek criticism, including that published since 1995, has failed to address itself adequately to the inscription and to the justificatory use which Wittek made of it. The present paper represents a first and — it must be stressed — a provisional attempt to remedy this deficiency.

From the foregoing it will be obvious that the public confidence in the 1337 inscription and his interpretation of it which Wittek displayed in 1937 still needs to be accounted for. At the outset it must be emphasized that neither Wittek’s skills as an epigrapher nor his familiarity with the Bursa inscription are in doubt. In the late nineteen-twenties, as a staff member of the German Archaeological Institute at Istanbul, Wittek had travelled extensively on expeditions in western Anatolia, on occasion in the company of the noted Ottomanist and epigrapher Fr. Taeschner, engaged in collecting epigraphic material. The published results of the two scholars’ journeys are contained in a pair of richly documented articles, both of which were published in *Der Islam*. Also, Wittek’s own editions of, for example, the Islamic inscriptions of Milet, which he also

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15 *Rise, op. cit.*, p. 15.
undertook at about this time, are equally models of clarity and scrupu-

losity.18

The reasons why Wittek failed to publish the 1337 Bursa inscription in full still remain obscure. In an introductory note (‘Gandarlyzade’, p. 60) with which Taeschner prefaced their joint article on the monuments of the Çandarlı family, he spoke of the intention of the proposed ‘Beiträge’ to deal with, in the case of Bursa, the inscriptions of Orkhan, the Yeşil Câmi’ complex, and a number of other specified monuments and inscriptions. When the ‘Beiträge’ appeared, in 1932, the 1337 inscription was not published, although the other promised Bursa inscriptions received the full treatment. In a note (‘Gandarlyzade’, p. 63, n. 2), which is appended to their preliminary discussion of the Yeşil Câmi’ inscriptions, the authors refer to the 1337 inscription being of ‘fräglichen Ursprungs’, and take issue with the view put forward by the pioneer Ottomanist Ahmed Tevhid19 that the 738/1337-8 inscription and the 820 rebuilding inscription on the adjacent mosque of Orkhan, which was apparently founded in A.H. 740, both derive from the ‘original’ Orkhan mosque at Bursa. An extended treatment of the problem was remitted to a future publication. However, in the ‘Nachträgë’ to the Çan-
darlı article which Taeschner published in Der Islam in 1935 he expressed a revised opinion, to the effect that the 1337 inscription belonged ‘in the highest probability’ to the Şehâdet mosque on which it is found, thus making the mosque the oldest surviving dated Ottoman structure. Taeschner stated also that he had dealt with the present and probable original form of the mosque ‘in [scil.: in an appendix to ?] an article prepared by Wittek dealing with the inscription’.20 This article never appeared. Presumably, like its author, it had become a victim of the times. The Nazi régime had come to power in Germany in 1933; Wittek speedily resigned from the German Archaeological Institute in Istanbul and found political and academic asylum in Brussels.21 Taeschner continued to live and work in Germany under the Third Reich. Had there been a personal as well as a political sundering between the two scholars? Whatever the answer may be to this last

question, the failure of Wittek and Taeschner’s joint article on the Şehâdet Mosque to appear leaves us unable to judge on the basis of his own detailed work how justified Wittek was in his reading of the inscription and, by extension, in the historical conclusions which he reached largely on the strength of it.

But we may still ask the question, how justified was he? The furnishing of a reliable answer to this question is not unimportant, given the weight of interpretation which this short inscription subsequently has been obliged to bear. In the first place, what of the inscription itself and its epigraphic history? In a footnote to ch. 1 of The Rise, Wittek observed somewhat cryptically that ‘a first attempt’ to read the Bursa inscription had been made in 1914 by the Ottoman scholar Ahmed Tevğid.22 Wittek does not comment on Tevğid’s reading, but we are left with an impression, unspoken but strongly perceived, that we are to suppose Wittek’s own later reading improved on and superseded it.

An inspection of Tevğid’s edition reveals that in almost all particulars it corresponds not only to Wittek’s rendering, but also to the readings offered by later editors and discussed below (sect. III). On three points, two of them critical to his interpretation, however, Wittek’s readings materially differ:

[a] For Tevğid’s reading of Orkhan’s laḳab (Shujā‘ al-Dunyā wa’l-Dîn), Wittek offers Shujā‘ al-Dawla wa’l-Dîn;

[b] Wittek adds the word marzbân to Tevğid’s reading, to create the crucial phrase marzbân al-âfâq (‘marquis of the horizons’);

[c] In the succeeding element of the inscription, Tevğid’s reading of bahâdur-i zamân, ‘hero of the age’ failed to recommend itself to Wittek, who substituted instead the reading bahlivân-i jîhân (‘hero of the world’), which thus is made to form the second of what he termed an ‘ensemble of titles absolutely unique in the Ottoman protocol’.

Thus far we have remained within the Wittekenian orbit. Our concern in this present note to explore the problem further is based on the unexpected discovery that not only has recent criticism of Wittek’s use of the 1337 inscription failed to investigate, as a matter of first principle, any of the epigraphic or contextual problems connected with the inscription or Wittek’s reading of it, but that not one of the editors of the several editions of the inscription which have appeared subsequent to the publication of the Rise has made any reference to the fundamental historical assumptions derived from Wittek’s reading of it.

There has, however, been no shortage of second-hand and speculative controversy. Most notably, the authenticity of this inscription has been questioned on several grounds by the late Ronald Jennings.23 His first

objection, on the grounds of style, was that ‘the date attributed to the
inscription [sic]’ ‘seems impossibly early for an Ottoman ruler to have
called himself sultan, never mind ghâzî. Secondly, he objects on histori-
cal grounds, in that, according to ‘another inscription’, Mehmed I
restored Orhan’s mosque in 1417 after the beg of Karaman had burned
it in 1413 while pillaging the suburbs of Bursa. Jennings concludes that
the 1337 inscription is ‘just one more evidence of later Ottomans trying
to upgrade the image of their past’.25

It must be said that Jennings’ analysis, both of the inscription and of
its architectural and historical context, is to some extent simplistic, and
that the reality is in fact more complex. As he correctly observes (‘Gazi-
Thesis’, op. cit., p. 154, n. 2) the 1337 inscription is today found not on
the existing Orhaniye câmi’, but cemented into the wall of the nearby
Şehâdet mosque. The mosque known today as ‘Orhan’s’, which was
burned by the beg of Karaman in 1413 and restored by ‘a certain’
Bâyezîd Pasha in 820/1417, was built in 740/1339-40, i.e. approximately
three years after the ‘original’ mosque of Orhan in Bursa. In the case
of the present Orhaniye câmi’ the circumstances of the rebuilding are faith-
fully recorded in the rededictory inscription;26 by analogy, therefore, it
is highly unlikely that the 1337 inscription would have been reused (or
simply copied) had it been utilised in the rebuilding of the earlier
mosque founded by Orkhan. In any case, we must admit a strong proba-
bility that the Karamanid invasion of 1413 is a red herring. Much more
significant is the fact that the Şehâdet mosque itself, occupying a prime
position within the citadel, was founded to commemorate the ‘martyr-
dom’ of Murâd I at the battle of Kosovo in 791/1389. To what extent
that event also made the Şehâdet mosque the effective site of an official
cult of the ‘martyr-sultan’ Murâd I, the existence of which Stephen
Reinert has convincingly suggested in a recent brilliant study, remains
undetermined.27 In this context the observation, already made by a num-er of scholars since Ahmed Tevhîd, that the Şehâdet mosque occupies
the site of the ‘original’ (and in all probability, one must observe, the
relatively humble) Orhaniye mosque in the citadel at Bursa would appear
to have much to recommend it. The Şehâdet mosque itself, Jennings
notes, was destroyed by lightning in 1855 and rebuilt in 1892;28 his

23 R. C. JENNINGS, ‘Some thoughts on the Gazi-Thesis’, WZKM, lxv, 1986, p. 151-
161, at p. 154-155.
24 See infra, n. 21.
26 Ekrem Hakki AYVERDİ, Osmanlı mı’ârûsinin ilk devri 630-805 (1230-1402), i,
İstanbul, 1966, p. 80-81 (with photograph of the inscription).
27 See Stephen W. REINERT, ‘From Nis to Kosovo Polje. Reflections on Murâd I’s
Final Years’, in Elizabeth Zachariadou (ed.), The Ottoman Emirate (1300-1389) (Halcyon
Days in Crete, I: A Symposium held in Rethymnon 11-13 January 1991), Rethymnon,
1993, p. 167-211, at p. 207, n. 95.
observation that ‘although it is not impossible that the inscription is the exact original from Orhan’s mosque, there was every opportunity, whether consciously or inadvertently, to change the original’ is a valid one, but should not lead us into generalised acceptance or rejection of the inscription as it stands. We are in fact greatly in need of the study promised but never delivered by Wittek and Taeschner, but in default of that, a more detailed analysis of the inscription’s constituent elements and their treatment by successive editors is a minimum prerequisite for any further discussion of the subject.29

In the more than six decades since Wittek wrote the 1937 University of London lectures which achieved their well-known and highly influential published form in the following year, the Bursa inscription has been published on a number of occasions. In 1942 the Turkish scholar Halim Baki Kunter brought together a number of inscriptions from Bursa, including both the 738/1337 and 740/1339-40 inscriptions attributed to Orkhan, as part of a rather heterogeneous selection of Anatolian epigraphy.30 Some ten years after Kunter the Arabic inscriptions of Bursa were published in a scholarly edition, but largely without historical commentary, by Robert Mantran.31 A further decade or so later both inscriptions, as already noticed, were published by Ayverdi in the course of his extensive survey of Ottoman architecture.32

What is most noteworthy about these editions subsequent to Wittek’s

29 I hesitate to comment on matters perhaps appertaining more to the history of Ottoman art, but in this context it is perhaps worth noting that the indisputably authentic gravestone at Iznik of Çandarlı Kara Halil, known as Hayreddin Paşa, dating from A.H. 789 (A.D. 1387), which was published by Taeschner and Wittek (‘Gandarlyzde’, op. cit., p. 60-62 and pl. 2), possesses the same ‘boxy’ style and pattern of interlocking / interwoven characters (cf. the dâl at the end of its l. 2) as the 1337 inscription. On chronological and stylistic grounds it is unlikely to be the work of the same craftsman, but the similarities are sufficient to establish the 1337 Bursa inscription firmly within the cadre of fourteenth-century Ottoman epigraphic practice. Even more can this be said of the inscription from the long-since demolished Hacı Hamza mosque at Iznik (AYVERDI, Ilk devri, op. cit., i, p. 162-3; clear photograph at p. 163), recording its construction by Hamza b. Erdemîn in 746 (1345), and the tomb inscription from the Kargizlar türbesi (AYVERDI, op. cit., p. 180) where the similarities with the 1337 inscription are so strikingly apparent that the possibility of all three inscriptions being the work of the same craftsman must be considered.
30 Halim Baki KUNTER, ‘Kitâbelerimiz’, Vakıflar Dergisi, ii, 1942, p. 431-455 plus 60 unnumbered pages of plates. The Şehâdet câmi’i inscription is at p. 437-438 (photo. 26); that of the Orhaniye câmi’i at p. 438 (ph. 27).
32 For the 738/1337 inscription see AYVERDI, Ilk devri, i, op. cit., p. 58-59 (with pho-
reading offered in *The Rise of the Ottoman Empire*, is that no common

ground exists amongst them which would permit a resolution of the very

real palaeographic problems inherent in the disputed readings. The vari-

ant readings of the disputed elements supplied to date may be tabulated

as follows, utilising the sigla employed above:

In view of the above irreconcilable differences between the readings

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<th>Kunter</th>
<th>Ayverdi</th>
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<td>[c]</td>
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of its various editors (with the exception of Ayverdi, who slavishly fol-

lows Kunter), it is tempting to agree with Jennings’ strictures on the

whole inscription, and reject it out of hand. The inscription as a whole is

on the small side, measuring only 0.5 m — little more than eighteen

inches — square overall, including the modern cartouche.33 Into this

limited space, carved in a small and crude neskhî (the dimensions and

style perhaps both pointing to an early date), is crammed the conven-

tional inner inscription in four lines (1∞: *basmala*; 2-3∞: Qur’an cxii, 1-4; 4∞:

*târîkh*) and a peripheral line which contains the problematic laudatio

for the founder of the building to which it was originally (and may still

be) attached. With the practical problems of distance inhibiting any

physical examination of the inscription in situ, the excellent and clear

photograph supplied by Kunter (reproduced below in Plate I), together

with a photograph taken from a different angle supplied by Ayverdi,

may serve as a basis for a further discussion of Wittek’s readings and

their implications.

[a] *Shujâ‘ al-Dawla* Wittek’s reading would appear to be correct on

palaeographic grounds: the wâw’ of *dawla* may be clearly discerned,

written directly above the *dal* and compressed between the uprights of

the two bracketing *lâms*. In any case, and what seems to have escaped

Jennings’ notice, there is confirmatory evidence from a numismatic

source in the shape of an undated silver coin of Orkhan struck at Bursa,

the authenticity of which cannot be questioned.34 The *lakab* Shujâ‘ al-

Dawla wa’l-Dîn is also employed in a Persian foundation charter of

tograph); for the mosque of Orhan and the rebuilding inscription of 820/1417 see *Ilk devri*, i, op. cit., p. 61-90 (*kitâbe* at p. 80-81).

33 The dimensions of the 1337 inscription are given by *Ayverdi*, *Ilk devri*, op. cit., p. 59.

34 The coin in question was published in a pamphlet by Şerâfettin Erel, *Nadir bir kaç
Orkhan’s dated I. Rebi ‘I 724 / 8-17 March 1324. Thus, in the face of variant readings for the 1337 inscription and of alternative lakabs for Orhan encountered elsewhere, epigraphic, numismatic and documentary evidence concur. This is certainly an indication of the essential genuineness of at least this element of the 1337 inscription, and may well testify to its basic authenticity.

[b] Marzban al-āfāk Wittek’s reading is clearly untenable, however attractive its first element may appear to be as a ‘learned’ synonym for uj-begi, rather than the meaningless ‘lord of the horizons’. Certainly, if we read, in order to avoid an Aralo-Persian macaronic, not marzbān ‘a warden of a frontier’ (Redhouse, s.v.), but (as an Arabic loan-word) marzbān (pl. maraziba; Redh., s.v: ‘lord of a frontier district’). What, however, are the āfāk? Ostensibly they are the pl. of Ar. Ufḵ, ‘horizon’, and are so translated by Wittek. Cf., however the (literary?) term āfākī, with its sense of ‘[one] who belongs to any distant region; foreign; vagrant; vagabond’ (Redh. s.v.). It may possibly be suggested, with very great reservations, that the use here of āfāk[ī] may imply a veiled reference to those vagrant and vagabond elements of the frontiers, largely under the control of the uj-begis/marzbāns, who are better known as ghāzīs.

It may also be observed that Wittek’s reading of marz[u]bān appears to be understandable only if the alif of the definite article in al-āfāk(i), the first element in the ‘inverted’ bottom strip of the peripheral inscription, is read (on its side) as a ‘flattened’ nūn and taken in conjunction with the kesrā’-like symbol which is joined to its foot and the two dots just to the latter’s right and directly under the dāl as forming together the final and penultimate letters of the word al-dīn. This reading would then free the actual yā’-nūn of –dīn to serve as the first element (MR) of Wittek’s marzbān, although his reading of the remaining elements (ZB’N) of that word from the convoluted but rounded and continue form compressed into the bottom left-hand corner of the inscription defies com-


36 For alternative, pseudo-historical lakabs attributed to Orkhan see ARTUK, ‘Early Ottoman coins’, op. cit., p. 457.
prehension. Mantran (‘Inscriptions de Brousse’, op. cit., p. 89) settles for a simple copulative wāw’, which seems both palaeographically and textually suspect, in providing Orkhan with a highly dubious triple lakab, to be translated as ‘valiant [defender] of the state and the religion and the horizons’, a phrase which lacks coherence: Mantran himself offers no translation of the phrase. Equally, Kunter and, following him, Ayverdi’s MNFK / al-afāk, although epigraphically just within the bounds of possibility, is meaningless. A possible alternative reading, which the unaccounted-for and apparently unpointed remaining corner element equally permits, would be muttaṣīk, in the sense of ‘ally’, i.e. of the ‘frontier people’, if we accept the tentative reading of afākī offered above. This would at least have the merit of making a certain sense, albeit one as unconventional in its way as Wittek’s ‘lord of the horizons’.

[c] Bahlivân-i jihân The basic defect in Wittek’s reading, as in Tevhīd’s, by which Wittek appears to have been influenced, is its acceptance of a Persian izāfet construction and a word of Persian secular origin in an Arabic and patently Islamic inscription. Wittek has gone part of the way to Arabize the verbal element through spelling pehlīvān with bā‘ rather than with pā‘. Indeed, as bahlawān, the term does appear to exist (but since when?) as a loan-word in Arabic.37 The same criticism may be levelled at Tevfik’s reading of bahādur. Yamīn al-imān, offered by Kunter and, following him, Ayverdi, we may reject out of hand. Clearly the choice is between bahlawān, also favoured by Mantran, who refrains from committing himself on the reading of the second element, and bahādur. If only for its pseudo-Arabic linguistic legitimacy, the balance would seem to incline in favour of Wittek and Mantran’s reading of bahlawān. But what of the second element? The izāfet, as a Persianism in an Arabic inscription, should be rejected. Zamān, as an alternative to jihān, is palaeographically unacceptable, and in any case we need a definite article. If a second, stumpy alif may be read to the left of the equally stumpy alif of bahlawān, the letter to the left again of the former may be read as the lām of the definite article, complete with the mandatory right-pointing ‘spur’ at the apex of the upright. We may then read, provisionally, and with a rudimentary jim-hā’, al-jihān, legitimising Wittek’s reading with the requisite definite article. Jihān, however, is a Persian word, and thus also will not serve. A close inspection of the photograph furnished by Kunter reveals that the left side of the nūn, although merged in part with the alif of the first letter of Orkhan, and bisected vertically by the preceding alif, is nonetheless clearly considerably shorter than the right side, is undotted, and needs no special pleading to be read as dāl. Thus, in place of Persian jihān we have the contextually and linguistically satisfactory Arabic jihād. This element

37 But with the modern meaning ‘acrobat, rope-jumper, juggler’. Hans Wehr, A Dictionary of Modern Written Arabic, Ithaca, NY, 1976, s.v.
therefore, reconstructed in fig. 1 p. 214, might be seen to read in full *bahlawān al-jihād*, ‘hero of the Holy War.’

To what extent are the revised readings suggested above supported by other internal and external evidence, or, even, how far should the inscription be taken at its putative face value? In a stimulating and important article which appeared some years ago, the late Richard Ettinghausen was at pains to draw a distinction between what he termed the communicative and the symbolically affirmative aspects of Arabic epigraphy. In pursuit of his theme Ettinghausen suggested that time was ripe for what he termed ‘a reconsideration of the role of [sic] inscriptions played [sic: ? read ‘placed’] on monuments or objects and of the assumption that this form of public communication was unfailingly trustworthy’. Ettinghausen provides a catalogue of highly convoluted and ornamented, and in certain cases grossly misspelled or incomplete, Arabic inscriptions gathered from non-Arabic speaking parts of the medieval Muslim world from Zanzibar to India and Anatolia. These he well describes as ‘puzzle[s] which could be solved only by [a] few’, and he concludes that although difficult to prove, ‘there are some indications that inscriptions remained mostly unread’ (p. 303). One such example is taken from the thirteenth-century Küllük Mosque in Kayseri, where an inscription, not only incomplete in itself but mistranslated by its editor on the basis, convincingly argued by Ettinghausen, of a local ‘reading’ supplied with the eye of faith and not of inspection. In other words, inscriptions may be regarded as acts of symbolic affirmation, rather than of communication. In the light of Ettinghausen’s observations it is therefore not surprising that the clear and unequivocal elements of the 1337 Bursa inscription include the name — if not the full

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39 *Id.*, *ibid.*, p. 299.

ascribed titles — of the building’s founder, together with the date of construction. But who wrote it, and for whom was it intended? Following Ettinghausen, the inscription, including its controversial elements, may naturally be attributed to some putative shaykh who provided the formulae; to the unknown kātib who wrote down the sacred and affirmative texts; and to the equally unknown mason who set them in stone. But who, in Bursa within ten years or so of its conquest, in a Turcophone border area far from the main centres of Islamic civilisation, could have read a small, in part convoluted, and possibly defective inscription in Arabic? As Ettinghausen concludes, ‘there is no absolute a priori certainty about an inscription’s reliability and… each case should be judged on its own merit.’ Ultimately, in the case of the 1337 Bursa inscription, neither the readings of [b] nor of [c] offered by Wittek, by his predecessor Ahmed Tevhid, or by his successors, nor the tentative revisions offered above can claim much in the way of epigraphical supporting evidence. Epigraphically, I feel the reading offered of ‘jihād’ in [c] is fairly secure; that of [b] presents what appear to be insoluble problems. Overall, an indictment of Wittek’s readings, with the exception of [c], would be difficult to sustain: it may not be inappropriate to suggest that a jury might bring in the good Scots verdict of ‘not proven’. How far, of course, the revised reading of [c], if accepted, actually helps to support rather than demolish the Wittekian ‘ghāzī thesis’, may merit further discussion beyond the limits of the present article.

In a counter-argumentative sense, however, further discussion may be redundant, and the historical significance of the Bursa inscription may be seen to have been considerably over-exaggerated. As noted above, Wittek referred to its significant elements (Rise, op. cit., p. 15) as ‘an ensemble of titles absolutely unique in the Ottoman protocol’, in contrast to what he describes, without illustration, as ‘generally the classical and quite different formulas of the Seljuk period’. It is therefore perhaps worth concluding this short note with a reference to the Islamic inscriptions which Wittek published, in exemplary fashion, in 1931, in R. M. Riefstahl’s monograph on Turkish architecture in southwestern Anatolia. Document 2 in Wittek’s collection is an inscription which had once adorned a tower in the sea-walls of Antalya, and which had already been published some years earlier by Ahmed Tevhid (in Türk Tarihi Encümeni Mecmu'ası, xv, p. 175). The inscription, which is dated 642 (beg.

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41 ETTINGHAUSEN, ‘Epigraphy’, op. cit., p. 317. KREISER, ‘Über einige Einigschaften’, op. cit., p. 104, formulates certain critical criteria for the differentiation between ‘early’, ‘high’ and ‘late’ Ottoman epigraphic styles. Applying these, in all particulars the 1337 inscription can be seen to be characteristic of its period.

42 Despite the epigraphic justification for reading jihād rather than jihān, in this context doubts must remain as to the actuality of the phrase.

9 June 1244), commemorates the builder of the tower, the Rūm Seljuk sultan Kāyhusrev, who is apostrophised by it not only as ‘Sultan of the sultans of the world, master (maulā) of the kings of the Arabs and Persians, but as (in Wittek’s translation), no less than ‘boundary guard of the horizons’ (marzbān al-afāq). It would appear, therefore, that not only was at least one of the fundamental elements of the Bursa inscription of nearly a century later not unique to the Ottomans, but had actually been employed by the Rūm Seljuks, the very same Rūm Seljuks who were, according to Wittek, the archetypical representatives of the ‘High’ Islam of the Anatolian hinterland, and the equally archetypical antitheses of the march-dwelling frontiersmen gathered around ‘Osmān Ghāzī’. It may not be too much to suggest that in the spirit of Ettinghausen’s observations noted above, the rhetorical phrase ‘Marzuban al-ufāk’ (in whatever variant we accept the reading) has the sense simply of ‘Lord of all I survey’, and has nothing to do with the uj. It is the more to be wondered at that Wittek, in his commentary on this inscription, fails to make any mention of this element in it. The implications both of this omission, and of the less than apposite interpretation which may be placed on the term itself, and to other disputed elements in the inscription noted above, are considerable, extending either to a revaluation of both Seljuk and early Ottoman history, or to the final abandonment — or final acceptance — after more than sixty years, of the so-called ‘Ghāzī Thesis’.

44 For a more detailed discussion of this point see my ‘The Frontier in Ottoman History’, op. cit., p. 231, ff.
45 RIEFSTAHL, Turkish Architecture, op. cit., p. 81.
Colin HEYWOOD, *The 1337 Bursa Inscription and its Interpreters*

The present paper examines the well-known Arabic inscription dated 738/1337-8 from the Şehādet Cāmi‘ at Bursa, and the use made of it by the late Professor Paul Wittek in his publications from the 1930s and by other Ottoman historians before and since. The article examines the inscription’s canonic status in Wittek’s historical formulations concerning the ghāzī nature of the Ottoman state and seeks to determine the reasons why he failed to publish it in a critical edition. Finally, certain phraseology in the inscription is compared with that in a Rûm Seljuk inscription from the previous century, also edited by Wittek, which would appear to demonstrate that the 1337 Bursa inscription cannot now be regarded as ‘unique’. The article is preceded by a ‘Preliminary Statement’ which takes a position towards the critical study by M. Kalus published in this issue of *Turcica*, and also reflects recent studies by Mme I. Beldiceanu and Heath Lowry.

Colin HEYWOOD, *L’inscription de Bursa (1337) et ses interprètes*

Cette étude a pour but d’examiner l’inscription célèbre de la mosquée Şehādet de Brousse, datée 738/1337-8, dans un esprit historiographique et, en particulier, l’utilisation de cette inscription par feu M. le Professeur Paul Wittek, dans ses publications bien connues des années trente, et par d’autres ottomanistes alors et depuis. L’article présenté ici examine le statut canonic de l’inscription dans les formulations historiques de M. Wittek concernant le nature ghāzī de l’État ottoman, et cherche à déterminer les raisons pour lesquelles il n’a pas réussi à publier cette inscription dans une édition critique. Finalement, des éléments de la phraséologie de l’inscription sont comparés à ceux d’une inscription seldjoukide de Rûm du siècle précédent — éditée également par Wittek — qui paraissent démontrer clairement l’impossibilité de regarder l’inscription de Brousse de 1337 comme « unique ». L’article est précédé d’une « Note préliminaire », qui prend position sur l’étude critique de M. Kalus, publiée dans ce même volume de *Turcica*, et reflète aussi des études récentes de Heath Lowry et de Mme I. Beldiceanu.