

REPLY TO JAMES D. PRICE

By John Wheeler (Version 2006-08-02)

On the bulletin board called **BaptistBoard**, a member asked his fellow board members if they had heard of Suzanne Haik-Vantoura's work, *The Music of the Bible Revealed*. One of those who participated in the thread (which you may find [here](#)) was Dr. James D. Price, Professor of Hebrew and Old Testament at Temple Baptist Seminary in Louisville.

In the days before easy Internet access (at least for me!), Dr. Price and I exchanged several lengthy letters by post regarding Haik-Vantoura's work. When an interviewer asked me for an "opposing point of view" regarding that work, he was the man I suggested. While he noted (correctly) that most Hebraists have not accepted Haik-Vantoura's (SHV's) arguments, he added (also correctly) that most Hebraists do not have the musical knowledge to follow them. Given what I saw on Dr. Price's (JDP's) side of the debate during our mutual letter-writing campaign, I have to agree. I also agree with him that more needs to be done to address specific issues raised by Masoretic scholars, and I am attempting to do this.

A member of the **Music of the Bible Revealed Group** on Yahoo.com (who is also a member of BaptistBoard) provided JDP's comments to me. My (JHW's) reply to those comments is inserted among them with increased indentation and italics.

Long post -- Posted with permission

The following was provided to me as appearing in a letter written by Dr. James A. Price to the translator of Haik-Vantoura's book.

The translator of SHV's book was Dennis Weber, now with a doctorate in music of his own. I was not aware that Dr. Weber had been in contact with JDP, although that should not surprise me.

Dr. Price is probably one of the leading experts in the Massoretic [*sic*] accents, having published a four-volume analysis of such throughout the entire OT. Dr. Price also mentions that someone at Southern Seminary in Louisville has written an entire doctoral dissertation on Haik-Vantoura's claims.

The assessment of JDP's status is correct. JDP is a member of the International Organization of Masoretic Studies, one of the older teachers in the field, and one of several who have carried out computer-aided studies of the accents as interpreted by the Masoretic paradigm. (Another scholar, now deceased, who carried out similar research and who vigorously debated SHV's conclusions was Gerard E. Weil of CRNS in Paris – hereafter, GEW.)

The "someone" mentioned is Dr. E. Wray Bryant (EWB), who is likewise a member of the IOMS. I met him at the Annual Meeting of the IOMS (after I became a member myself) a few years ago. He has been trying to publish his dissertation for several years through [BIBAL Press](#) (the same publisher who co-published SHV's book in English). I have been looking forward to reading that dissertation for all those years, and I gave EWB specific advice concerning how SHV's melodic paradigm and the Masoretic syntactic paradigm compare.

JDP's comments and my reply to them follow.

...I appreciate the information you provided in an effort to clear up possible misunderstanding of Suzanne Haik-Vantoura's work on the music of the accents of the Hebrew Bible. There are certain aspects of her work that I regard as correct

from the perspective of accents. She is obviously a gifted musician and composer. I am not sufficiently trained in music to criticize the musical aspects of her work, except as intuition and common sense dictate.

*This begs the question: is “intuition and common sense” sufficient to criticize a serious musical decipherment of an ancient notation? (Apparently JDP does not think SHV’s “intuition and common sense”, with which she was plentifully endowed, was sufficient to qualify her to critique the Masoretic paradigm. We shall see.) One of the MBR Group members, a conservatory-trained musician and music teacher, told me that many of the concerns JDP voices below **reminded him of questions asked by his first-year music students**. It takes more than “intuition and common sense” to appreciate music, let alone to critique it, on a technical level. It takes an understanding of music as a **tonal language** – which after all is what the cantillation of the Bible is meant to be.*

*To say that the late SHV was “a gifted musician and composer” is a considerable understatement (though no doubt an unintentional one). Her **curriculum vitae** as given on the cover of her book (which reflects her characteristic combination of modesty and self-knowledge) does not begin to cover her accomplishments. (A more complete review of them, in English and French, is found on my Web site [here](#).) The classical singer and voice teacher Mira Zakai in Israel¹ has noted SHV’s exceptionally keen ear for modal patterns (that is, the use of scale types and accidentals) – something vital for dealing with ancient musical notation, which virtually never indicated the mode openly.*

SHV was a specialist in her own field, but also interdisciplinary and highly independent in her way of thinking.² Obviously, no one can “specialize” in everything – including SHV, JDP and this author (JHW). This last factor unquestionably lent some fundamental strengths and weaknesses to SHV’s arguments concerning both the Masoretic paradigm and the melodic interpretations associated with it. But it also unquestionably lends even more fundamental strengths and weaknesses to JDP’s arguments against SHV’s thesis.

*Myself, I (JHW) stand somewhere between SHV and JDP. I am largely self-taught both in music theory and practice and in the intricacies of the Masoretic reading tradition, although I have had some formal training in both music and Hebrew. (Indeed, I am largely self-taught in just about everything I do; I seem to learn best that way.) Due to financial and health issues, I have not been able to pursue my dream of obtaining a doctoral degree combining both music and Hebrew – a dream that JDP encouraged me to follow. My actual profession involves a dream that is deeper still, founded on the one degree I possess (a B.A. in theology). I work as a research assistant – effectively, as an **apologist** – for my church. This allows me to apply my love of multidisciplinary studies and of writing; it also has allowed me to integrate SHV’s seminal work into my research.*

These are some aspects of her work that I regard as correct:

1. The accents represent very ancient tradition along with the other signs the Masoretes added to the Hebrew text, such as the vowel points, the **niqqudim**, and Masoretic notes. The accent signs represent the ancient oral cantillation of the text.

Apparently, JDP means that the cantillation was transmitted orally and was only written down in the time of the Masoretes. If so, this comment reflects

certain lines of rabbinic tradition and (since the Renaissance) scientific thinking alike, but it is not what the next-to-last of the chief family of Masoretes – Moshe ben Asher – said about the accentuation.

SHV partially addressed this issue in her French book; only in the appendices to the English translation could she address it more fully. Her citation of ben Asher's colophon to his Codex of the Prophets (895 AD) prompted me to look for the book in which it was published.³ In it I found other remarks by ben Asher, in a poem called "The Song of the Vine". I passed them on to SHV, who put them with her thanks in her English book.⁴

*The poem claims that a family of Second Temple priests called the Elders of Bathyra (in the Babylonian Talmud, the Sons of Bathyra) "as delights...**have established (hitqīnu)** the accents of Scripture, giving [the] sense and interpreting its word." As I pointed out to JDP in one of my letters, no one "establishes" a musical tradition save in **writing!** One uses similar language even today when discussing the "fixing" of the Hebrew Masoretic Text itself. Oral tradition, even that memorized by a **yeshiva**-trained mind, is simply inadequate to "establish" a tradition of such size and complexity. Only a written tradition can do this.*

*But there is another and even more important indication, and that concerns how the Masoretes and their successors understood (or rather did **not** understand) the features, layout and meaning of the written accentuation. I will come to that in due time. I will merely point out here that even were it possible for a trained memory to pass on the tradition intact, the simplest and only adequate explanation of the facts is that the **meaning** of the tradition was not preserved along with it. JDP finds this reality a stumbling block to his understanding, but it need not be so.*

2. There is a system of laws that govern the sequences of the accents. My computer research has verified that fact, and formalized the laws. The verification has now been extended to the entire Hebrew Bible.

SHV likewise has written that her deciphering key applies to the entire Hebrew Bible⁵ – as indeed it does in her base text (the Letteris Edition, a revision of the Second Rabbinic Bible by Meir ha-Levi Letteris). This leaves aside the matter of typographical errors that, once corrected, make her case stronger, not weaker.⁶ So far as I am aware, JDP's verification extends to another base text (the BHS, which unlike Letteris is available in computer-searchable formats of various types). I will make clear why this distinction matters in due course.

*JDP overlooks the fundamental issue, here and elsewhere: **we are dealing with two mirror-image paradigms – methods of analysis that address the accentuation vis-à-vis the Hebrew verbal syntax in exactly the same way, but from opposing perspectives ("the accentuation is primarily melodic" versus "the accentuation is primarily syntactic").** Viewed from another angle, the simplest and only adequate explanation of what the early Masoretic sources say and do **not** say is that the "tables of accents" and the "grammatical rules" accepted by JDP and his peers comprise **a medieval "deciphering key"** to the ancient accentuation – **not** an authoritatively transmitted tradition of the original meaning of the accentuation. It then becomes a matter of discovering which "deciphering key" – that of the Masoretes and their successors or that of SHV – actually explains all the features of the notation and its layout. One*

***must** understand this point before one can evaluate SHV's paradigm vis-à-vis the Masoretic paradigm properly.*

*I have often compared SHV's melodic paradigm and the Masoretic syntactic paradigm to the cosmological paradigms of Copernicus and Ptolemy, respectively. Obviously, both Copernicus and Ptolemy would have agreed that there are laws governing the motions of the heavens. The effects of those laws might be observed by anyone with the proper equipment and training. The only question (then) was whether the laws and their effects implied a sun-centered universe or an earth-centered universe. Copernicus' paradigm won out, against all opposition, **because it was at once the simpler and the more complete explanation of the available data.** Once the heliocentric nature of the Solar System was established, the doors opened to possibilities no one could have considered before.*

*In like manner, SHV confronts JDP and his peers with a "Copernican revolution" in how the accents should be viewed. In her paradigm, the hierarchical functions of the accents that JDP analyzes in his computer-based studies of the Masoretic paradigm are not only completely preserved; they actually are made more detailed and precise (especially in Psalms⁷). In other words, SHV's paradigm explains everything that the Masoretic paradigm explains and everything that it does **not** explain – even fine details of the accentuation (as found in various manuscripts and printed editions, no less) that SHV did not address during her lifetime. This should not surprise the qualified musician, because he knows that tonal music has a precise yet flexible hierarchy of itself – and only such a hierarchy can explain all the details of the accentuation's format and layout.*

*JDP continually evades and (as we shall see) even misrepresents this issue in his comments here, as he did in his letters to me. I trust that this is unintentional, but the fact remains that he is unable or unwilling to consider that there might be an explanation of the accentuation that is at once simpler and more complete **by far** than the Masoretic paradigm that so occupies him. But given the proper musical training, he would be able to **hear** the hierarchical relationships in SHV's deciphering key and its resulting melodies. Then it would be a simple matter of writing them down in tables or in scores on musical staff paper, just as SHV has done. On that level, and that **only**, can "intuition and common sense" apply to understanding tonal relationships.*

As SHV (using, let it be said, a great deal of common sense) remarked regarding the conclusions of the Masoretic paradigm, "their inanity will not escape anyone's attention since the verbal syntax is available to us all."⁸ Indeed, they should not. But if one takes that paradigm as an article of faith (as do JDP and GEW in their computer-based analyses of the accentuation), and then argues from that paradigm's authority, then no kind or amount of evidence will suffice to change one's mind.

3. The laws of the accents function in harmony with the natural grammatical syntax of the verses, except as overruled by the musical restraints.

*One might wonder how JDP can comment about "musical restraints" at all, since neither the Masoretes nor the grammarians ever gave any systematic musical interpretation of the accentuation. The best that the **Masorah** and the early "reader's manuals" could do is assign to **some** of the so-called "disjunctives" a value as **markers** of a very simple rising and falling melodic curve – that is, of the most "primitive" form of synagogue chant, such as*

was contemporary with the Masoretes and as is still used in many communities. Even the early sources disagree among themselves as to which “disjunctives” should serve in this quasi-melodic way.

*In all fairness, JDP, other grammarians such as Israel Yeivin, cantors, and musicologists such as Israel Adler, realize that certain features of the accentuation are unrelated to the accents’ “harmony with the natural grammatical syntax of the verses”, but must have a strictly **musical** basis. (Most of these scholars include in that basis, as JDP does not in this paragraph, the **melodic** aspect in principle.) That musical basis, however, has remained unknown, and there are definite reasons for it having become so. But, JDP cannot and does not address those reasons, because here as elsewhere, he fails to come to grips with the **core issue**: the inability of the Masoretic syntactic paradigm and its associated musical interpretations to explain **most** of the features of the accentuation and its layout relative to the verbal text.*

4. The accents are primarily melodic in their function and secondarily syntactical, because the musical restraints on the laws of the accents overrule the syntactical laws when the two systems are in tension. By musical here I mean the rhythmic aspects of cantillation, not the melodic aspects. I have not studied the melodic aspects of the restraints. The musical restraints, as recorded by the authorities on accents, are primarily determined by syllable count. Basically the restraints say that a certain number of syllables must exist between sequences of certain accents or substitution takes place. This does not mean that melody is not involved, but that the melodic reason for the restraint is not clearly stated.

*The melodic reason for the restraint JDP mentions – and for much else which JDP does not address here and never addressed in our correspondence – is not “clearly stated” in the early sources because to my knowledge **it is not stated at all** – for it was **unknown** by that time. Indeed, the **prima facie** evidence from those very sources is that their authors tried to “make the best of a bad situation”. In effect, their authors sought to reinterpret (and not very successfully) the accentuation according to the musical norms of their own time. Many would-be decipherers of the notation since at least the Renaissance have followed suit, arbitrarily imposing (as JDP puts it later, “**mapping**”) an extra-biblical and usually contemporary musical standard on the notation.*

*SHV did not detail how the deciphered melodic system dictates the sort of phenomena that JDP describes above. Evidently, she did not feel the need. For a composer, such data would be largely irrelevant, since one would sense them intuitively. Few who recite or listen to rhymed metrical poetry or song take the trouble to count the stressed and unstressed syllables in the metrical feet as they go; and still fewer do so with blank or free verse, let alone with prose. Such a thing is the affair of specialists in **prosody** (in the modern sense, not in the ancient Greek sense that SHV cites in her book), some of whom (regarding the scriptural texts) I work with on BIBAL.net. In like manner, an examination (however technically profitable) of how the reconstructed melodic system relates to the verbal prosody is unnecessary for most people, and especially for most trained musicians and composers. Again, such an examination is the affair of specialists. Nevertheless, I would like to make such an examination myself, for my sake and for non-musicians (including grammarians such as JDP) who may never understand the foundations of SHV’s analysis any other way.*

Along that line, I am making (as I have the time) a **cadential analysis** of the Psalms and prose texts. Derenbourg pointed out that five accents would be sufficient to mark the punctuation and phrasing of a Hebrew verse.⁹ In fact he was right: in prose texts (not counting the minor cadences within clauses) there are five kinds of melodic-verbal cadences, no more, no less. (In Psalms, Proverbs and the body of Job, there are four such cadences, at least insofar as I've examined the texts.) The reason there are some 19-20 accents (in SHV's paradigm) and not five or six accents is that the accents are indeed a truly melodic notation. Different combinations of accents and words mark different melodic-verbal cadences, because the accents also express four factors other than (yet interwoven with) disjunctive and conjunctive force.¹⁰

JDP is close to the mark here, yet apparently he does not realize that SHV's paradigm implies that the accents are primarily **melodic**, then **rhythmic**, then **syntactic** – rather than the **reverse** as the Masoretic paradigm implies. Considerably more is involved in the formation of biblical chant than the number of "conjunctive accents" that may occur before a particular "disjunctive accent", or the overall rule that JDP states ("a certain number of syllables must exist between sequences of certain accents or substitution takes place").

With the Hebrew accents (as with so many notations of like genre: the liturgical "**neumes**"), the position of the sign (whether it be – under SHV's paradigm – a sublinear degree of the mode or a superlinear melisma) determines its "syllabic time". The same sign, when it has a different position relative to a word or syllable in one place than it has in another, indicates that the note or melisma it represents is sung at a different syllabic time. As a corollary, a given pair of signs which the Masoretes and the early grammarians defined as distinct (e.g., **mehuppakh** and "**yetiv**", **pashta** and "**azla**", **tifha** and "**dehi**") is merely one melodic accent found in two different rhythmic positions and thus serving two different syntactic functions. Single notes and melismas of one note can do this easily; the same is not true of the complex "tropes" that some synagogues attach to the various prose accents.

A special case is the pair of signs called **silluq** and "**ga`ya**" (or "**meteg**") by the Masoretes and grammarians. The latter has no syntactic function at all, and thus the Masoretic paradigm classifies it as not an accent at all; yet even so, it has a **pausal** function just as **silluq** does. SHV's key makes clear why and how this is so. Both signs represent the same value: the **tonic degree** or "**final**" of the mode, which is naturally found at the end of every verse yet is just as naturally associated with every other sign, used to mark secondary stresses upon words, and repeated two or even three times at the end of a verse (e.g., Psalms 96:13, Letteris Edition). All this is **normal** and even **expected** for tonal music and its inherent hierarchy. It is no accident that **silluq** itself means "end"; that is the ancient name for the tonic degree of the mode, and in more than one language.

Now as we will see, JDP objects that **silluq** and "**ga`ya**" cannot have the same value and therefore cannot be the same accent. Under the Masoretic paradigm, they do not and are not. But this begs the question of whether the Masoretic paradigm starts from the right premise in the first place. SHV's discovery that **silluq** and **ga`ya** are in fact one and the same **melodic sign** – the tonic degree – was the **key** that unlocked the entire melodic, syntactic and rhythmic structure of the accentuation to her. I will go into more detail on this matter later.

Here I should discuss several related matters. One of the great conundrums of Masoretic studies is that so many grammarians (and with them cantors in general) insist that the accents really are **primarily melodic** in their function, yet they accept “tables of accents” (and/or melodic interpretations of the accents) which treat the accents as if they were **primarily syntactic**. If the accentuation really is primarily melodic, then there should be a one-to-one correspondence between the combination of graphic form and position and the melodic meaning (as there always is in any genuinely musical notation). Moreover, the very hierarchy of forms should make some kind of graphical sense – of the kind that one often finds in IQ tests (“which of the figures below should follow in the above sequence?”). This is not what we find in the Masoretic “tables of accents”. The same graphical form receives different meanings and even names according to its position in a verse, and different forms receive the same or similar meanings according to the same criteria. Besides, the order of forms in the tables, which rank the accents according to their alleged relative disjunctive and conjunctive values, makes no graphical sense at all – it is haphazard. **In these tables, the syntactic function literally dominates everything else (form, position, melody, rhythm), and some grammarians see the fact and insist on it just as SHV did.** (By contrast, the order of signs in SHV’s key, especially that given for the sublinear signs, makes eminent sense to a musician – above all to one who is familiar with ancient and modern gestural systems for conducting music – and even to one who appreciates graphical relationships for their own sake.)

George Orwell had a word for the basis of such a conundrum as this: “**doublethink**” – an attempt to believe two contradictory things at the same time, whether the person realizes he is doing so or not. In the case of the accentuation, it arose because the Masoretes and everyone else were trying to justify the rules governing the accents by the indications given in the Talmud about the “accents” as used in contemporary synagogue practice. In other words, the conundrum arose (as did so much else in Talmudic Judaism) because **people were trying to reconcile two contradictory traditions as if they had equal authority and meant the same thing.** These were the priestly tradition (in this case, the written accentuation we now use and the gestures that it transcribes) and the Pharisaic/Rabbinic tradition (in this case, the oral synagogue chants and the various local gestures that they transcribed or went along with them).

“Doublethink” (which is typically based on **argument from authority**) can keep people from seeing the truth about something for centuries, even millennia.¹¹ The only reason SHV avoided its effects is paradoxically because of her blissful ignorance (back in World War II, when she started her work) of the Masoretic paradigm and its “tables of accents”. Only when she started writing her French book (in the mid-1970’s) did she start coming to grips with the conundrum. She may not have done so in complete detail – but she did so in essence. A triumph of intuition and common sense, I would call it, were it not really a triumph of serendipity.

However, there are many problems that I have with her work. Although she exhibits some knowledge of Hebrew, her academic credentials and the way she discussed the language, when she did, do not reflect much depth.

Here are some of the problems I see with her work:

1. With respect to the traditional body of knowledge of accents, she demonstrated no awareness of the acknowledged authorities such as William Wickes, Aron Dotan, Israel Yeivin, etc. While she mentions G. E. Weil, who wrote in French, she references his work on the history of Hebrew grammarians, but not his extensive work on accents. Likewise, she shows no knowledge of the authorities in the field of cantillation. Yet she criticizes the authorities on accents as though they were entirely wrong. This makes me wary about the value of her work. It has the appearance of the work of a gifted but partially informed amateur.

*JDP's comments on SHV's partial knowledge, while **valid** as far as they go, are a two-edged sword. His comments here and in our correspondence show very little knowledge of music theory – even compared to JHW, an admitted self-taught amateur in such matters. They show practically none of music history, especially that of the ancient Middle East (the documentation of which poses real surprises to those who think it was all like the Arabo-Persian **maqamat** in use since Islamic times). They show little or no knowledge of musical composition, which was SHV's specialty. In fact, SHV seems to have learned more about Masoretic studies than JDP has learned so far about music. Here we simply have the pot calling the kettle black – and raising a red herring in the process. **The issue is whether the syntactic paradigm or the melodic paradigm explains the accentuation better - not whether either SHV or JDP knew the other's specialty perfectly.** However, SHV made a serious effort to understand the implications of the Masoretic paradigm on its own terms (and that while knowing virtually no English, let alone medieval or modern Hebrew). Has JDP made a like effort to understand ethnomusicology?*

*JDP seems unaware that GEW published the first volume of his extensive computerized analysis of the accents (again, in principle similar to JDP's) **after** SHV published the first edition of her French book.¹² An appendix in the second edition of SHV's French book (and its English translation) gives GEW's comments as a pre-publication reviewer. He remarked that SHV's work was "very dangerous" to his own – enough so that he took no little space to attempt to refute it in his own first volume. One of his fellows at CRNS, Hellenist and musicologist Denise Jourdan-Hemmerdinger (DJH), **reviewed** SHV's later work on the 150 Psalms and in passing refuted several of GEW's key claims.¹³*

*SHV also mentions Jules Derenbourg,¹⁴ a French grammarian who was all too familiar with his grammarian predecessors and how some were **misled** by the classification of the "disjunctive accents" (as defined, of course, by the Masoretic paradigm) into four grades.¹⁵ Derenbourg's comments were more than sufficient for SHV's purposes, as they summarized so much of the historical and technical quandary that the Masoretic paradigm poses. JDP (assuming he reads French) should become familiar with Derenbourg's comments, if he is not already! They dovetail with certain comments made by other grammarians whose works were written in or have been translated into English (but not French, so far as I know).¹⁶*

*It is inaccurate to say, as JDP does, that SHV criticized the Masoretes, grammarians and cantors as if they were **entirely** wrong. He should have read her English book more carefully. SHV criticized the "authorities" as if they were **fundamentally** wrong (which they **were and remain**) – and there is a considerable difference! For example, she acknowledged freely that the accents have "disjunctive" and "conjunctive" functions, but she pointed out – as her paradigm demonstrates – that in principle they are not **irreversibly** so.¹⁷ Moreover, while her critique of the traditional cantillation*

*styles has some flaws in detail (though not fatal ones), it describes the essential issues correctly in principle. **When one corrects the matters of detail, they make SHV's overall case stronger, not weaker.** JDP, being **so certain** of the authority of the Masoretic paradigm, has never addressed this last point.¹⁸*

*How did SHV know that the "authorities" are **fundamentally** wrong, once she started examining what they actually say? By the **stubborn fact** that JDP continually evades: **none** of these "authorities" can explain the reasons for **most** of the features of the notation or of its relationship to the verbal text! SHV managed to explain nearly all of them, and I can explain the rest by following her logic through consistently. (Textual criticism poses a qualification here, which I will address in due time.) JDP even gave me a needed clue to help me do so, in the case of one rare accent (**merkha kefulah**) – and that despite himself, when **appealing to technicalities** in an effort to refute SHV's overall case.*

2. The accents are a part of the oral tradition passed down from antiquity and recorded by the Masoretes by means of the signs they inserted into the Hebrew text. The oral pronunciation of the words was recorded by the vowel points, whereas the ancient cantillation was recorded by the accent marks. SHV seems to regard the vowel points as accurate and reliable, so that a current reading of the text is a valid rendering of the ancient oral tradition. In like fashion she regards the accent marks to be an essentially accurate preservation of the hand signs used by the ancient teachers of cantillation. However, she asserts that the musical values of the accent signs had been lost by the time the Masoretes recorded the signs, and that they must be rediscovered. This is a problem. How could the signs be preserved but the music they represent be lost? The music of cantillation is an integral ingredient for memorizing the oral pronunciation. How difficult it is to remember the words of a familiar hymn without the accompanying music! How difficult it would have been for the ancient cantor to recite the text without the associated music of cantillation! Music and recitation seem to be inseparable. If the oral tradition of pronunciation had been accurately preserved, then the cantillation surely must have been preserved as well. If the cantillation was lost, then that suggests that the pronunciation was lost also. That suggestion has serious ramifications.

*JDP makes several unjustified assumptions here. **First**, neither the accents nor the vowel-points that we presently use represent an **oral** tradition – not since the days of Ezra, when **both were set in writing** according to the Masoretic and other medieval sources (such as the Manuel du Lecteur, Moshe ben Asher's colophon to the Codex of the Prophets, and his Song of the Vine). "Logoprosodic analysis" being done by Dr. Duane Christensen of BIBAL.net, and "melothematic analysis" being done by myself in cooperation with him, both indicate that the accents were established in writing **not later** than the time of Ezra, when the Samaritan and the Hebrew texts went their separate ways and the latter was canonized.*

*Before Ezra's time, and still long after it, indeed the accents were **taught orally** – again, according to the same sources. But this is in keeping with the consistent ancient and medieval practice of keeping liturgical notations and even vowel-points **secret among the initiated** and teaching what they represented orally to others. (Such treatment of musical notation goes at least as far back as ancient Babylon, as ethnomusicologist Curt Sachs documented long ago.¹⁹) The meanings themselves were passed down among the initiated, father-to-son or teacher-to-pupil. There are cases where the notations were passed down but the meanings were not,*

because the master teachers were killed or died out before they passed on their knowledge. This happened with the [Armenian liturgical notation](#), which was only recently deciphered by a process very similar to what SHV used.

This evidently happened with the so-called “Tiberian” notation, because that notation (according to one of its chief transcribers) is (contrary to JDP’s **second** unjustified assumption) **not** a transcription of then-living synagogue chant or of then-current vowel pronunciation. It is a transcription of the “Second Temple edition” of accentuation and vowel pronunciation as preserved and taught by a specific and well-known family of priests. The fact that no complete explanation of the notation was passed down by that family (or anyone else) demonstrates that they did **not** pass on their knowledge to others (due to massacre, scattering, or simply a break in the secret transmission of knowledge).

Whereas to start from JDP’s **second** assumption leads to a conundrum (one which he continually evades). If the Tiberian accentuation in particular – and on some matters even the Tiberian vowel-points – transcribe then-current “oral tradition”, why then is there so little real correlation between that tradition and the notation? Why do the historical explanations of the accentuation especially start off as being so tentative, unsystematic and incomplete, according even to grammarians such as Israel Yeivin? **Who invents a notation and then leaves it to others to explain what lies behind it?** While it is true that musical notations often appear first and then the theoretical explanations of them, nowhere else does there seem to be so **vast** a difference between the actual norms of the notation and the theories proposed to explain them. And yet, those who should have known the most – the ben Ashers, Moshe (fl. 895) and his son Aharon (fl. 930) – are the least forthcoming about the actual meaning of the accentuation, despite the size of the latter’s treatise about it (the famous *Diqduqê ha-Te`amim*).

Next, there is sufficient disagreement and discussion among the medieval sources (written in different locations concerning different traditional pronunciations) to show that the authoritative meanings of the vowel-points and even of some **consonants** were likewise in dispute. Certainly, there was no one monolithic “oral tradition” of pronunciation in the ninth and tenth centuries, no more than there was until the 20th century and the founding of the State of Israel. The pronunciation used by the scribes of Tiberias for certain consonants and vowel-points was not that used by the Karaites of Egypt, nor was it that used by the Babylonian Rabbinic community – just to cite three examples documented in medieval writings. For that matter, the Tiberian accents and vowel-points (which, according to Moshe ben Asher, ultimately came from the Second Temple priests via the Karaites) were **much different** from their analogues in Palestinian and Babylonian synagogues.²⁰

It is interesting that JDP focuses in passing – as do so many – on the cantillation as a form of “memory aid”. The “primitive” synagogue chant was no better than that, but the Temple chant (being the work of inspired professionals) was of another order. The very Hebrew word for the accents – **te`amim** – point to their real purpose, that of **exegesis**. (Various medieval commentaries confirm this function, beginning with the almost rhapsodic descriptions by Moshe ben Asher of the implications of the word **te`amim** itself.) The accents enable the reader and the listener to **discern and appreciate** (literally, to “**taste**”) the intellectual, emotional and spiritual **sense** of the words, which in normal speech was indicated by the vocal

*inflection. What good would it do to **remember** the words if one did not **understand** them first? This last is not something even our modern four-part hymn form is very good at; at least one study has shown that our hymn form actually makes it harder, not easier, to focus on the meaning of the words.²¹ The same often applies to synagogue chant, which typically **ornament** rather than **interpret** the words. Not so with the melodies derived from SHV's key!*

*In sum, JDP's objection only holds water if one assumes with him that the "Tiberian" notation transcribes the living synagogue tradition rather than the lost Temple tradition – which two traditions (incidentally) the Babylonian Talmud effectively equated. **That equation above all has kept nearly everyone who deals with this subject confused for most of the last two millennia.** Some modern scholars (and traditionalists) still insist on this equation, even though it has been refuted long ago.*

*Yes, there are serious ramifications involved here, but this should not be disturbing to those who value the truth above argument from authority. From a theologian's point of view, nothing says that God cannot preserve His Word (which includes, in the case of Hebrew Scripture, its original cantillation: Psalms 119:54, etc.) **despite** those who preserve it, if necessary (Romans 3:1-4).*

It is true that the pronunciation and cantillation have drifted into different traditions in the past thousand years, but that does not prove that the ancient tradition was lost by the time the Masoretes provided the vowel points and accent marks. On the contrary, the Masoretes provided the marks to preserve the tradition that had been passed down to them in order to prevent such an eventual drift. She herself acknowledged that the Masoretes regarded the tradition to go back to the Second Temple. The fact that such a drift occurred subsequently, does not prove that it had already occurred previously. The way to recover the ancient tradition is to compare the current divergent traditions and discover their common ancestral roots. This certainly would be possible for the pronunciation of the consonants and vowels, since the various traditions are not that different; so it is reasonable to assume that for the music as well.

*It is the source materials of the Masoretes' own time and the few centuries immediately following, **not** the lingual and musical shifts that have occurred since, that show how very much was **not** understood about either the pronunciation and (most especially) the cantillation as the Tiberian notation preserve them.²²*

*Let us apply some **genuine** "common sense" here. If the Masoretes wanted to fix a living oral tradition, then why does their attempt to fix a major part of it – the cantillation – have so little in common with either their own explanations of how the accents should be sung or with any documented form of synagogue chant? Here is where the shoe pinches, **hard**. Neither the Masoretic syntactic paradigm, nor the Masoretic musical explanation that accompanies it in the early sources, nor any documented form of synagogue chant, nor any computer analysis based on the Masoretic paradigm (including GEW's and JDP's), explains the following features of the accentuation:*

- 1. The reason for the existence of sublinear and superlinear accents;*
- 2. The musically based predominance of the sublinear accents (in that entire melodic-verbal verses and clauses may be marked by sublinear*

accents alone, but only clauses may be marked by superlinear accents alone);

3. *The various forms of the accents;*
4. *The various positions of certain accents relative to a syllable, or even to an entire word;*
5. *The reasons for the various orders of the “conjunctive” signs in between the “disjunctive” signs (something that even GEW acknowledged was mysterious);*
6. *The haphazard assignment of sublinear and superlinear signs to “disjunctive” and “conjunctive” roles;*
7. *The assignment (within the Masoretic paradigm) of different signs to the same syntactic function, or similar signs to different syntactic functions, in a way and to a degree that cannot be explained away as “conservation of symbols”;*
8. *The fact that the names (many of them now disregarded) that are given to the various signs in the early sources “had neither fixity nor unity” and (in some cases) evidently arose out of a desire to assign special domains to signs which originally meant one and the same thing (Derenbourg);*
9. *The arbitrary application of the accentual hierarchy to the verbal text by the Masoretic paradigm (e.g., similar verbal texts in which one would expect exactly the same sequence of accents may have considerably different sequences of accents, and – as Israel Yeivin effectively acknowledges – we do not know within the framework of the Masoretic paradigm why this is so).*

*JDP did and still does his best to evade these points, but these are the very points that demonstrate the invalidity of the Masoretic paradigm and the validity of SHV's. It is worth repeating, and then verifying by examining SHV's work: hers is the simplest and only adequate explanation, past or present, of all these features **plus** the disjunctive and conjunctive functions that preoccupy the Masoretic paradigm.*

*I believe that JDP rather underestimates the differences between Jewish dialects. Certainly, I believe that he **vastly** underestimates the differences between different cantillation traditions. In particular, he misunderstands why their similarities and differences exist. All these chant styles were influenced by, or even were derived from, the music of the communities' Gentile neighbors. Comparative studies have shown that the “tropes” in particular were derived from the local varieties of the Arabo-Persian **maqamat**, which comprise the Islamic “Great Tradition”. Of these, only the Ashkenazic Torah chant (which afterward was influenced by European tonality) gives an ornamental “trope” to each accent. Other traditions give “tropes” to only the disjunctive accents, or even to the major disjunctive accents only. Then there is the much older, and much more primitive, variety of chant which the **Masorah**, the early treatises and the transcriptions into Christian neumes by Obadiah the Norman document. This kind of chant consists of a simple rising and falling melodic curve which is stretched or contracted to fit the verses, clauses and phrases.*

*It is utopian to expect that a common source for all of these **primitive and folk tunes** may be inferred, other than in the general Oriental tendency to “melodize” texts according to various kinds of patterns – and musicologists in general realize this now. In particular, musicologist Israel Adler (no fan of SHV's work, believing as he does that the “Tiberian” notation had a medieval origin) points out that the structural differences between the Temple liturgy and the “primitive” synagogue liturgies make **unreasonable a direct***

connection between the two.²³ Primitive song and art song have fundamentally different structures, no matter where or when they exist. Whatever else might be said about SHV's restitutions, they fall into the structural category of art song (however simple) and not of primitive song.

Again, JDP fails to "think outside the box". He repeats SHV's claim that the accentuation goes back to the Second Temple, yet he cannot or will not address the real implications of that claim. SHV (on the basis both of her own key and of Masoretic testimony) claimed direct written transmission, **not** direct or indirect oral transmission, of the accentuation to the Masoretes – and that it contains Temple chant, not synagogue chant. That such written transmission also involved the loss of the original meaning is very reasonable; such a circumstance is far from limited to the Masoretic and Armenian liturgical notations.²⁴

How then was the "Tiberian" notation passed on intact while its meaning was not? One among several possible lines of transmission (all of which may have co-existed) is that the "Tiberian" notation was transmitted via "Dead Sea Scrolls" of the Bible and sectarian manuscripts (apparently including at least one extant in our own Dead Sea Scrolls, the Damascus Document) found in a cave "near Jericho" in 800 AD and brought to the Karaites in Jerusalem. The Karaites would have scrutinized such scrolls intensely, and what they found apparently gave great impetus to the Karaite movement. Within perhaps as little as 20-50 years (depending on who is doing the dating), "fully pointed" manuscripts start appearing with the "Tiberian" notation.

It takes no great imagination to postulate that while the original scrolls of the Bible were hidden, those who understood their meaning had died out. Enough knowledge had been preserved in **corrupted** oral form and **corrupt** local written notations so that the accents and vowel-points in these old scrolls could be recognized for what they were. But while the **significance** of these markings was understood, their actual **meaning** (especially that of the accents) was not – which accounts for why the accents and vowel-points alike were analyzed, tentatively at first, then more systematically, in the early treatises (beginning with Aharon ben Asher's), and then yet more systematically by stages from the Renaissance to the present.

3. SHV assumed that all signs of a given shape have the same musical value, without regard to their position in a word. This is in serious disharmony with the traditional view of the accents and with respect to the different names given to the signs in different positions.

*This is no argument against the validity of SHV's paradigm! The traditional view of the accents is **fatally flawed**, for the reasons I have just stated (if not others as well). But JDP in his turn is in disharmony with what SHV actually said. She took into account not only the form of a given sign, but its position relative to a word or syllable and its sublinear or superlinear position as well. Otherwise, what the Masoretic paradigm calls in prose texts **pashta**, **azla**, and **tifha** would all have exactly the same meaning in SHV's paradigm – and they do not.*

*Derenbourg's comment about different names being given to what was originally the same sign with the same meaning applied specifically to the secondary names given to **munah**, a common sublinear sign found in many different syntactic contexts. But if such secondary names (now rejected by Hebraists) could have been given to **munah**, why then could not some of*

the names that modern “tables of accents” still retain also have been secondary names originally? Since the “tables” explain so very little about the accents, why not examine the notation (as SHV did) via a paradigm created vis-à-vis the verbal syntax but apart from the traditional names and then cross-check the results against the names? What better way could there be to discover which names were truly ancestral and which were added later?

So for example:

(a) Pashta, a postpositive accent, is a disjunctive accent; whereas Azla, the same sign, occurs only on the stressed syllable of a word, and is a conjunctive accent. These signs never occur in the same context, that is, they have different laws of sequence. Their different names, different context, and different function, imply that they are different; and traditional cantillation treats them differently.

*This statement and the following ones simply argue from the Masoretic paradigm’s traditional authority – thus committing a logical fallacy and missing the whole point of why SHV’s paradigm **differs** and why its results are **superior**.*

*In SHV’s deciphering key, **pashta** includes what the Masoretic paradigm calls “**azla**”. What apparently escapes JDP’s notice is that **the functional disjunctive-conjunctive differentiation between pashta and “azla” is not lost thereby**. Indeed, nowhere does SHV claim that it is. It is just that this differentiation is **implicit** in the tonal hierarchy that she inferred – and is immediately sensible to a musician, even if he is not trained specifically to express it in the terminology of Masoretic studies.*

*What makes the syntactic distinction possible in SHV’s “deciphering key”? What she called **pashta** therein is a **melisma** – an ornament (in this case, of just one note) on one syllable. As with the melismas of other sets of “neumes”, its position relative to a word or syllable determines its “point of entry” on that word or syllable. Thus, when it is found at the end of a syllable (i.e., when it is “postpositive”), its value “enters” the melodic line at the end of the syllable (see the countless examples in SHV’s scores and hear those on her recordings). This **nuance** of the melodic line dictates a syntactic **disjunction**. When it is found in the midst of a syllable – that is, according to the “scribal habit” preserved in the Letteris and Ginsburg Editions (in which case SHV’s paradigm gives it the same name but the Masoretic paradigm adds a new name) – it “enters” at that point (in effect, the whole syllable is sung with that melisma). This **nuance** dictates a syntactic **conjunction**.*

*SHV was justified in assigning the same name to the same graphic sign in these different contexts because **pashta** refers to the **melodic** effect, which is always the same. It is always an **extension**, tonally speaking – and that fact was discovered long before SHV addressed the meaning of the various names. That same tonal effect nevertheless may either divide or connect words, and even mark the primary and secondary verbal stresses as well on those words that have **pashta** doubled. (JDP does not mention this special case, though he can by no means ignore it in his own work; it is extremely common.) Whereas **azla**²⁵ may be accounted for as an additional name intended to give a special domain to **pashta** in one context which it does not possess in another – all on the premise that the accentuation is primarily syntactic rather than primarily melodic. Thus, SHV’s explanation is at once*

simpler and more complete than the Masoretic one. We should accept it (here and everywhere else) for that reason alone.

*How the synagogue chants interpret the accents, in this case and the others that follow, is **irrelevant**, because the chants are **unrelated** to the “Tiberian” accents. Either they appeared before the notation did (and such chants do not attempt to “interpret” the accents in any systematic or complete way), or else they were (partially and arbitrarily) adapted to the notation after its appearance. In either case, they simply follow the lead of the Masoretic paradigm and in no way confirm its validity.*

*It is likewise irrelevant to point out that a given graphic form may appear in different sequences of forms and thereby may have different syntactic functions. **Of course it may! That is characteristic of any systematic musical notation whatever!** The same degree of the scale, or the same melisma of just one note, represented by just one sign, may be used in different ways, may follow different laws of sequence and position and thereby have different effects in different contexts. This does not mean that the same sign has different fundamental meanings in different syntactic contexts, and thus should receive different names in those contexts. If it does, then it is no longer a primarily melodic sign, but a primarily syntactic one – and this leads to an insoluble conundrum.*

(b) Yethib, a prepositive accent, is a disjunctive, a musical substitute for Pashta under certain conditions; whereas Mahpak, the same sign, occurs only on the stressed syllable of a word, and is a conjunctive. These signs never occur in the same context. Their different names, different context, and different function, imply that they are different; and traditional cantillation treats them differently.

Similar considerations apply here. These comments have merit only if one argues from the authority of the Masoretic paradigm – which explains practically nothing about the accentuation other than its disjunctive and conjunctive hierarchy (and that not perfectly). Again, what the synagogue chants do in following the Masoretic paradigm’s lead is irrelevant.

*Let us conduct an Einstein-style “thought experiment” for a moment. Suppose we encountered the Masoretic accentuation for the first time – let us say, as it stands in the Letteris Edition (which none other than GEW recommended that SHV use for her work). Suppose we knew that the accentuation is melodic, rhythmic and syntactic (that is, “musical”) in its function and exegetical in its purpose. Now – **suppose we knew nothing else about the notation** (no names, no cantorial interpretations). How would we decipher it? If one is clever and careful, one may use the Hebrew verbal syntax as a **virtual bilingual** (a sort of “Rosetta Stone”). This requires that one decide what the primary function of the accentuation is. It also requires the creation of exhaustive statistical tables of the accents themselves (their orders, associations, frequencies, etc.), in order that one may infer rules as to their use vis-à-vis the verbal syntax and each other.*

*What few indeed seem to grasp is that that Masoretic paradigm, and indeed everything that the **Masorah** and the early treatises say about the “grammar of the accents”, **may be explained completely** on this basis. So may the many differences of opinion (in both manuscripts and printed editions) as to what accents should be used in what contexts (especially in Psalms, but also in numerous prose passages) and how certain accents should be placed relative to a word or syllable. All this information is best*

explained not as the result of a careful transcription of an ancient oral tradition, but of a medieval research program that created a deceptively complex “deciphering key” to the accents based on an incorrect premise. Those who conducted the research program then text-criticized the accentuation and gave rise to various “scribal habits” and opinions as to its correct use, and finally argued from authority so that the accentuation overall might be accepted by “all Israel”.

Thus, starting from the premise that the accents are primarily syntactic, the Masoretes and grammarians concluded of necessity that mehuppakh and “yetiv” are two different “accents” – and that the latter is in some way a “variant” of pashta. (Allegedly, it is a musical variant.) JDP cannot and does not explain why not only the form, but the position of pashta is so changed – and in a way that can get the resulting “yetiv” confused with mehuppakh (including eleven times in a particularly telling circumstance, as pointed out by the Masorah). This is a sure sign that the foundational premise of the paradigm is a faulty one.

Again, in this case SHV’s paradigm passes the closer shave with Occam’s Razor. In fact, it explains something that SHV herself did not address, if followed through consistently. Mehuppakh and “yetiv” are indeed the same sign musically speaking (the 6th degree of the scale, originally called just mehuppakh thanks to its tonal function); but their melodic-verbal contexts give them different syntactic functions. Given the accents that surround it, mehuppakh in the guise of “yetiv” happens to fulfill a syntactic function similar to that of pashta. “Yetiv” nevertheless may be explained as a name that was added in order to give mehuppakh a special domain in a particular position, as based on the Masoretic paradigm. The same tonal function applies whether mehuppakh is syntactically disjunctive or conjunctive; the name “yetiv” is redundant.

The reason that “yetiv” is prepositive (i.e., before the vowel, at the beginning of the syllable), and why certain other prose accents are likewise prepositive, has to do with rhythm. Just as pashta (at the end of a syllable) or telisha qetannah (ditto) extend the syllabic time forward, so “yetiv” (along with certain other prepositive superlinear melismas) extends the syllabic time backward. Esther Lamandier (who has recorded many of SHV’s scores) accounts for this intuitively in her singing, even though SHV’s scores do not make this distinction of position clear. When I account for the prepositive position of “yetiv” in this way as I read straight from the Hebrew text, the cantillation always benefits (as does the way it clarifies the meaning of the words).

(c) Silluq, the strongest disjunctive accent, occurs only on the stressed syllable of the last word of a verse, causing its pronunciation to be modified; whereas Metheg, the same sign, but not part of the traditional system of cantillation, occurs only on syllables with secondary stress in long words. Metheg may occur anywhere in a verse where a word is long enough to require it. There is no rule of accentuation that governs its occurrence with respect to the regular accents. Its use is governed strictly by linguistic grammar.

SHV’s paradigm shows that JDP notwithstanding, silluq and the so-called “meteg” (Israeli Hebrew spelling) or “ga`ya” are governed by consistent laws with respect to the rest of the accents. But they are tonal laws, not merely disjunctive-conjunctive laws (which are of necessity subject to the tonal laws and not vice versa, else the format and layout of the notation would be much different).

The reason “**ga`ya**” (the term I prefer, as it is used by Israel Yeivin in his very helpful book) is not “part of the traditional system of cantillation” is that it is neither “disjunctive” nor “conjunctive” in its function. Nevertheless, it fulfills no less than **four** important functions even within the Masoretic paradigm (one of which is explicitly, if indefinitely, called “**musical**” by Yeivin), and **all** of them involve the ability of “**ga`ya**” to mark a **pause**. JDP failed to grasp this point in our correspondence, thinking that I misquoted a scholar cited by Yeivin on this matter. I did not.²⁶ That scholar was not speaking of “**ga`ya**” as having a **disjunctive** function; he obviously knew better. Rather, he was speaking of it having a **pausal** function – and again, there is a considerable difference! This **pausal** function is what “**ga`ya**” shares in common with **silluq**. SHV’s paradigm shows why this is so. The tonic degree (which tonally is **inherently pausal**) may end every verse, and may be found two or three times on the same word on its primary and secondary stressed syllables (marking what musicians call a **repeated final cadence**). Yet it may also have common associations with every other musical sign and in the process mark secondary stresses on the words within a verse or on the last word of a verse. In other words, the **pausal** force in the tonal sense can dictate the strongest possible **disjunctive** force at the end of a verse and several **other** kinds of force in other places (the very kinds that the so-called “**ga`ya**” imposes on the Hebrew verbal text). This is **normal** for tonal vocal music – a fact that JDP needs to reckon with!

The Masoretic scholar Aron Dotan has noted that in many cases, the early scribes did not understand why an important type of “**ga`ya**” was present in the reading tradition, and therefore simply left it out. In JDP’s base text (BHS), this is a very frequent occurrence (even on many “long words”), as is the shift of “**ga`ya**” to other syllables and (under special circumstances involving the hyphen or **maqaf**) to other words relative to SHV’s base text (Letteris). These variants apparently are part of an overall attempt by the pointer of the manuscript behind BHS (Leningrad B-19) to make the accentuation as “self-consistent” as possible as a syntactic notation. Yet even therein, “**ga`ya**” is not limited to long words – unless JDP considers words of two or three syllables as being “long”. SHV’s base text (Letteris) is much more consistent in marking “**ga`ya**” overall, and yet in a way that almost always leads to superior results under her paradigm. What is often melodically and rhythmically anomalous or even **awkward** in a reading from BHS “flows” with great “lyricism” in a reading from Letteris. Rarely does BHS preserve a reading that is actually melodically and exegetically superior to the grammatically more “conservative” reading found in Letteris.²⁷

Since SHV’s tonal system is so simple and yet so rigid, and since BHS and Letteris **mostly** agree in detail with regard to the accentuation, the above difference in the musical results could hardly be an artifact either of her paradigm or of its application to the Letteris Edition. It has to represent something fundamentally different in the quality of the two base texts. While Letteris is not without flaws (even in the liturgical layout of its consonantal text), in the light of SHV’s paradigm it is the best edition that I have seen with regard to the accentuation – better than BHS, BHL, Snaith, Qoren or even Ginsburg.²⁸

Here we touch upon a matter of textual criticism: should we base our printed editions on one or a few of the **oldest** extant manuscripts, simply because they are the oldest extant, or because of argument from authority? Or should we base our editions on the **best** manuscripts irrespective of age and source? SHV’s paradigm implies the latter; but it also implies that we

can analyze and criticize, to a depth we could not before, the state of the accentuation in manuscripts and printed editions, and create a truly "correct" edition accordingly.

(d) She was correct in not including Paseq in her musical system, but she overlooked the fact that Paseq occurs with certain of the accents to produce Legarmeh, a minor disjunctive accent, one that is omitted in her system.

*SHV made no distinction between **munah** and "legarmeh" (sometimes called **munah legarmeh**) for a very good reason. According to her paradigm, **munah** (which represents the 5th degree of the scale) marks either a **suspensive cadence** (by itself or – much more frequently – in combination with the superlinear melisma **zaqef qatan**) or a continuation of the melodic line on a **suspended degree**. In other words, the same tonal value (the 5th degree) **inherently** may play either a disjunctive or a conjunctive role (and much more at the same time besides), depending on how it is used. Again, the use of the 5th degree in this multifaceted way is **normal** for tonal music, and not just in the modern West.*

*Originally, then, there would have been one sign with one name (**munah**), one tonal meaning (the 5th degree), and one tonal function (as signified by the name itself). "Legarmeh" (which name has a strictly **syntactic** significance) would have been a name added to give **munah** a special domain in a particular place. The fact that this sign in such a place is sometimes called **munah legarmeh** in early and modern treatises supports this conclusion. There were other names added to **munah** in the early treatises in the same way for the same reason, as Derenbourg pointed out. The syntactic distinctions attempted by those other names are obsolete for JDP and his grammarian peers, and rightly so (even if the Ashkenazic Torah chant, developed long after Masoretic times, makes those distinctions **melodically**). But since one simple tonal value fulfills all the observed syntactic functions, and since the meaning of the name **munah** and the form of the sign itself point to that very tonal value and its tonal function, there is no need to propose or retain other names – including "legarmeh". Again, this is a simpler and more complete explanation than that offered by the Masoretic paradigm.*

*Yeivin comments that the vertical line **paseq** probably was added after the accent system was divulged, as it completes the system. If so, this would explain its lack of systematic use²⁹ – presumably meaning in the early manuscripts, as printed editions deemed suitable for liturgical use mark it consistently. Sometimes **paseq** occurs with sublinear and superlinear accents other than **munah**. My comment: while **paseq** is a useful guide to the placement of the minor cadences in the melodic line, the accents alone are sufficient to mark those cadences by their rediscovered **melodic** values. Thus, **paseq** shows itself to be redundant, and is indeed a later addition under the influence of the Masoretic paradigm.*

(e) Similar problems exist in the way she treats the accent system of the Poetical Books.

4. The joining of Silluq and Metheg as the same tone seems like a fundamental flaw in her whole system. First of all, the Metheg is not part of the traditional system of accentuation; it is not assigned a musical value in that system. Yet SHV has combined the statistics of the Silluq and Metheg to provide the basis for selecting them as the "tonic" note for the whole system--because they have the same shape! But Metheg occurs far more frequently than Silluq. Without the statistics of Metheg,

Silluq would fail to be the "tonic" note because its frequency count is significantly less than some of the other signs, such as Zaqeph, Tiphcha, Munach, and Merecha. This appears to be a gross disregard for the cantillation tradition that the Masoretes recorded when they invented the accent signs. I know she discussed this on page 445, but the discussion seems like an afterthought to cover her tracks, a dogmatic assertion without dealing with the issue. How could Metheg, the fundamental tone of cantillation (according to SHV), be lost and completely fall out of the tradition as a non-musical sign?

*The answer to the last question is the same answer I gave to an earlier question: how could the notation be preserved but the original meaning be lost? **Easily!** All it would take is 1) the death of the ancient initiates who preserved and taught the accents (when the Second Temple was destroyed and in the period following); and 2) the later reinterpretation, in partial ignorance and starting from a faulty premise, of the accents. This is the **only** adequate explanation of what the Masoretic paradigm, as given in the actual source materials and as analyzed down to the present, accounts for and does **not** account for. JDP never addresses this reality.*

*Actually, SHV **could** have inferred that **silluq** was the "tonic" without taking "ga`ya" into account. **Its consistent placement at the end of the verse was far more than enough to facilitate this.** There are many precedents for such a thing in other forms of ancient and early music, including ancient Greek music, synagogue chant and Christian plainsong. (In addition, the simple presence of such a "final" all but proves a **diatonic or diatonic-chromatic modality** for the music that contains it. No other kind of modality has a "center of attraction" of anywhere near that strength, as indicated by its placement and frequency at the end of every verse.)*

*In antiquity a single note, or a very simple melodic formula of two or three notes, acted to end a verse repeatedly. **Normally** a single note fulfilled this function – which is why (as Sachs pointed out³⁰) the ancient name for the tonic note, in more than one language, was "end" or "final" (essentially what **silluq** means). With that note at the end of a verse, the tonal and the syntactic functions coincide – and become one. This, **not** the relative frequency of the tonic degree to other degrees or melismas, is the **primary** factor to consider with regard to the tonic in early music – and it is the **first** factor that SHV looked at. The "final" as such may indeed be considerably less frequent in early music (and indeed in much modern Western music) than certain other degrees and melismas, yet without losing its innate power as a "center of attraction".*

*Since SHV was after all a conservatory student when she began her work, it would be no great shock if she looked at the problem of the tonic from the viewpoint of **Western** music theory. Her book seems to indicate that. In Western music, **after** the placement of the tonic at the end of a verse and in the midst of a verse, the actual frequencies of occurrence of the tonic and its association with other degrees become important. These are what SHV looked at next (after her identification of the other universal "pillar degrees", the 4th and the 5th³¹). In traditional and modern Western music, those frequencies can become quite high. But so can they in other kinds of tonal music, including ancient Greek and traditional Hindu music!*

*What JDP overlooks – again, because he is not a music theorist or composer – is that were **silluq** and "ga`ya" actually two very similar signs with completely different meanings, **that fact would have become evident immediately during SHV's analysis.** First, there is such a thing as using*

the tonic **too often** – even **far too often** – in the construction of a melodic or melodic-verbal line. Such **redundancy**, in combination with other degrees and melismas, **weighs the melodic line down**, to the detriment of its own expression and of its interpretation of the words (not to overlook the physical ability of the singer to handle the line). This reality has to do with **psychoacoustics** – with the way the human brain processes musical and linguistic sounds.

Interestingly, a comparison of Letteris with other editions in (for example) Psalm 150, under SHV's paradigm, will illustrate the point. The other editions typically use "**ga`ya**" much more frequently there; and because they do, the melodic line **all but collapses under its own weight** (and **draws attention to itself rather than to the words** in the process, something most uncharacteristic of SHV's renditions in general). Now, there is nothing "unlawful" about the use of "**ga`ya**" in these editions according to the Masoretic paradigm; indeed, the usage would be what a grammarian normally would expect. The accentuation in Letteris is unusually sparse by comparison. Yet the melodic line in Letteris, though relatively more repetitive from verse to verse, is much lighter and more expressive – as if encouraging the instruments mentioned in the Psalm to have their place in its performance. It also expresses the sense of the words better of itself. This result makes eminent musical sense.

We must not overlook the other side of the coin: while **silluq** and "**ga`ya**", taken together, are the most common of the sublinear graphic forms, they are far outnumbered by all the other graphic forms (or even just the sublinear ones) taken together. This is no surprise either.

There is something else that JDP overlooks (which is understandable, since SHV did not dwell on it). **Any** arbitrary placement of the tonic relative to the other degrees and melismas – which SHV would have imposed were "**ga`ya**" not in fact a tonic degree– would be evident to her trained musician's ear **regardless of whether any other accents had been deciphered yet or not**. All she needed to do (and apparently what she **did** do, if only "in her head") was read a given text through in a monotone (as it were, on a tonic pitch), emphasizing all the accented syllables (including those syllables marked by **silluq** and "**ga`ya**") with a slight stress. **Any** arbitrary placement of the tonic, either on **silluq** or "**ga`ya**", would have shown up **immediately** – as would the actual identity of one or both signs as the tonic, were that the case.

As a test, I started reading through Genesis 1 in this way. I only got as far as verse 3 before the equivalence of these two supposedly different signs became **glaringly obvious**. It became more and more so as I proceeded. Without any special emphasis within the monotone recital, the meaning of each word, each syllable marked by **silluq** and "**ga`ya**" **"leaps out"** at the reader. At those places in the Letteris Edition (and those places **only**), the tonal value and the semantic value in a monotone reading **coincide**, whether the syntactic or the grammatical value also coincides with the tonal value or not. In all places these supposedly different signs represent a **pause** of the same kind, yet having different functions according to the context. The only way this would be possible is if both **silluq** and "**ga`ya**" were in fact the tonic degree. No ornament of as small as two or three notes can have this consistent coincidence even at the end of the verses, let alone within the verses as well. Similar considerations apply to SHV's identification of the other degrees of the scale, especially (right after **silluq**) **atnah** as the 4th degree and **munah** as the 5th degree.

How do the traditional grammarians explain the use of one sign with two different meanings and functions? Conservation of symbols. Two symbols are not needed if the given phenomena never occur in the same context. Thus one sign plus its context is sufficient for several independent phenomena. That principle explains why one word in a language has several different meanings, depending on context. Without the principle of conservation of symbol, language would be burdened with an unmanageable number of words. The principle provides the system of cantillation with a manageable number of signs. The same conditions exist within the system of vowel points. There are two shewas, but one sign: the vocal shewa and the silent shewa. They never occur in the same context. Grammarians know the difference and never confuse them. There are two Qamets, but one sign: the Qamets Gadol, and the Qamets Qatan; one a lengthened "a" class vowel, and the other a short "u" class vowel. They never occur in the same context. Grammarians know the difference and never confuse them. There are two dageshes, but one sign: dagesh forte and dagesh lene. They never occur in the same context. Grammarians know the difference and never confuse them.

The same is true about the accent signs. There are two different accents with different cantillation values, but the same sign. They never occur in the same context. Cantors know the difference and never confuse them. It is evident that SHV does not know these facts. At least she has not addressed them and appears to have ignored them.

*SHV's critique of the traditional cantillation styles did not go this far. It did not need to. The same problems inherent in the Masoretic paradigm (which she **did** critique) are inherent in the cantillation styles that are attached to it. To simply go over the same ground from a different angle would have been redundant, however much it would have underlined the point. Evidently some people need the underlining, because they cannot figure the point out for themselves. Fair enough.*

*But let us look at JDP's point regarding conservations of symbols for a moment. On the surface, it seems to be a valid critique. How is it then that SHV's paradigm manages to "conserve the symbols" **far more** than does the Masoretic paradigm (19-20 accents versus up to nearly three dozen accents, depending on who is doing the counting in the latter)? Conservation of symbols leads to **one-to-one correspondence** between form, position and meaning – **not** (as in the Masoretic paradigm, as understood either by grammarians or by cantors) to the same form having different meanings in different contexts **and** different forms having the same meaning in similar contexts. This last state (which is similar to what occurs in English pronunciation and spelling of consonants and vowels, **not** overall to what occurs in Hebrew pronunciation and spelling regarding these same values) is a sign of loss of knowledge, corruption, and confusion – **not** of "conservation of symbols". Again, this is something JDP cannot or will not address here, no more than he did in our correspondence. How can he? To do so honestly would be to deny the **fundamental** validity of the Masoretic paradigm, and therefore of his entire body of work!³²*

*Indeed, two symbols are not needed when one will do. That is **exactly** why the pairs **silluq** and "ga`ya", **munah** and "legarmeh", **mehuppakh** and "yetiv", **pashta** and "azla", etc., should **not** be distinguished as they are in the Masoretic paradigm. Within each pair, **one value** suffices to explain the form and positions of each graphic form. It is also why (in the psalmodic system of accents under SHV's paradigm) one superlinear sign – **illuy** – uniquely may represent a rise of a 5th or a descent of a 4th on a syllable, in*

order to keep the melodic line from surpassing its normal ambitus of eleven degrees. It may do so because what musicians call the **dominant** interval (the ascending 5th) and the **subdominant** interval (the descending 4th) have the **same tonal effect** (through **inversion**). If in performance a distinction needed to be made between the two, a simple rise or fall of the hand-gesture which **illuy** represents would have sufficed.

Likewise **pashta** and “**yetiv**” on the one hand and **segolta** and **shalsholet** on the other should not be considered “musical variants” of the same “syntactic accent”. They are different musical accents that happen to have similar syntactic functions within a melodic-verbal verse. They have different melodic meanings because syntax is only one semantic factor that they indicate. Moreover (as noted) “**yetiv**” should not be considered a separate “accent”. It is simply **mehuppakh** placed at the very front of the word, in order to allow its tonal meaning to fulfill a syntactic function like that of **pashta**. A comparison of various verses (especially those which contain both **mehuppakh** and “**yetiv**”) under SHV’s paradigm will illustrate the point. Again, all this is the simplest and yet the most complete explanation of the facts.

JDP’s comparison of the accents to the vowel-points is specious. First, the only reason grammarians and cantors never confuse the various “Masoretic accents” is because they have been **taught** not to confuse them – just as we English speakers have been **taught** not to confuse the various uses of the letter **a** in different contexts and having different pronunciations (for example). It is not because there is anything **inherently** clear or simple about the Masoretic paradigm’s treatment of certain accents. When similar forms have different values in different places and different forms have similar values in similar places, this **invites** confusion! Moreover, nothing in the accents themselves – not their forms, not their positions relative to a word or a syllable (or each other), not their syntactic contexts – **demands** that some of them (**pashta** and “**azla**”, etc.) should be treated as they are. That treatment simply is **imposed** on the notation, on the assumption that the notation is primarily syntactic – and grammarians and cantors then are trained to make distinctions accordingly. They do not see what they are really doing, because they are taught to believe in “tables of accents” that are primarily syntactic while yet claiming that they are primarily melodic.

Second, one should expect Hebrew vowel-points having the same form to have the same meaning regardless of position, barring proof to the contrary. Most of the time, they **do**. “Proof to the contrary” involves taking the positions of certain vowels relative to and association with certain consonants and accents into account. This is particularly true of the vowel-points associated with (or substituting for) the letter **waw** acting as a **matre lectionis** (“mother of reading”). Yet the vowel system is not as simple in traditional usage as its forms suggest it should be. **Qamets gadol** and **qamets qatan** on the one hand and the two kinds of **shewa** on the other should mean the same thing in traditional usage – and they do not. The fact that they are in different contexts is **insufficient** to account for the different interpretations, **because those differences depend on how the accents are interpreted**. Since the grammarians and cantors have misinterpreted the accents (seeing that they cannot explain **most** of the accents’ features), it would be no surprise if they have misinterpreted certain vowel-points associated with certain accents as well. In both cases, it would not be a matter of **conservation of symbols**, but of **multiplication of meanings** – just as in the English language, if for different reasons!

SHV's paradigm, if nothing else does, implies that **shewa** always represents a neutral "e" – it is never truly silent. Its function is always to make the pronunciation of the letter it marks clear. Thus, the conservation of symbols is **genuinely** preserved. As grammarians know, sometimes it takes on the value of a full syllable and sometimes it does not, but how it does this should be re-examined in the light of SHV's paradigm. For example, a simple verb like the imperative **shim-` ah** (Psalms 17:1) allegedly should really be pronounced **shim-(e)` ah**, with the **shewa** joined with the second syllable (as a half-syllable) rather than ending the first syllable as in Israeli Hebrew practice.³³ Yet the accentuation under SHV's paradigm indicates that Israeli Hebrew practice is essentially correct. The accent is placed on -` **ah**; and the change in melody begins where the accent is placed (**not** at the beginning of the word as the Masoretic paradigm, again making the accentuation primarily syntactic, would have us believe), nor on the alleged half-syllable. Thus, the melody effectively associates **(e)** with **shim-** no matter how hard one tries to make it go elsewhere; and this perforce makes the pronunciation of the letter **m** clear. It is **voiced**, yet it falls within the regular "syllabic time" of the first syllable (**shim(e)-` ah**).

Likewise, I submit that there is no good reason why **qamets** should ever have more than one meaning, let alone why it should also have the same meaning in some cases as **patah** (as it does in Israeli Hebrew). Viewing the accentuation from SHV's perspective changes one's perspective on these vowels as well. There is a very good reason (for example) why the word **kol** has **qamets** ("ah") when it is unaccented and why it has **holem** ("o") when it is accented (and even, in some very rare cases, when it is not). This has to do with **euphony** – how the sounds of consonants, accents and vowels blend in order to bring out meaning (and this is something to which ancient man often paid considerable attention). What examples such as these tell is worth further detailed study.

JDP's argument, again, ultimately rests on argument from authority and not on the facts. His objections are valid only if one accepts the validity of the Masoretic paradigm first.

5. She says that the accent signs below the line have a fixed musical meaning, but that the signs above the line are subordinate to those below. This is inaccurate. The laws of accentuation regard the signs above and below the line to be integral parts of the same structured system, some of the signs below the line being hierarchically subordinate to those above. This has been verified by my computer analysis of the whole Old Testament.

*JDP's computer analysis, based as it is on the Masoretic paradigm, **starts by ignoring the sublinear and superlinear positions of the accents as irrelevant.** Were those positions taken into account, **the Masoretic paradigm would die in the cradle – and so would JDP's analysis!** These positions above and below the verbal line constitute by far the most obvious feature of the accentuation, yet it is the feature most studiously avoided by the Masoretic paradigm and its adherents (including JDP in our correspondence). A return to our "thought experiment" will show why.*

If one takes the accentuation as a notation of known significance (it is melodic, syntactic, and rhythmic) but of unknown meaning (which function predominates?), then the place to start one's analysis is where SHV did: with the most obvious feature of the layout, the superlinear and sublinear positions of the signs. Many whole verses, as well as many more whole clauses, are marked with sublinear signs only. Only some clauses are

marked with superlinear signs only. Within other clauses, and within many verses, the two sets are interspersed – so they are indeed interdependent. Yet because an entire melodic-verbal verse may be marked by sublinear signs only and never by superlinear signs only, this presages the **musical predominance** of the sublinear signs. SHV knew all of this. What she did not know, **then**, is **how** the sublinear signs are predominant. That came later, when the sublinear signs were verified one by one as representing degrees of a scale. (Besides the exhaustive statistical tables she created to verify this, there was also the singing of a melodic line with the words. Even in the absence of associated superlinear melismas, the melodic character of each sublinear sign became evident through its uses in countless contexts vis-à-vis the Hebrew verbal text.)

What would the Masoretes have done, and how would the early grammarians and cantors have followed suit, when they looked at the same phenomena? SHV, remember, looked at the notation as primarily **melodic**. But if one assumes that it is primarily **syntactic**, immediately one runs into a conundrum. **There is no strict correlation between the position of a sign above or below the line and its disjunctive or conjunctive function. What correlation exists is partial and haphazard. One now must choose between abandoning the hypothesis and “saving” it by adding an unverifiable assumption: the positions of the accents above and below the line are irrelevant to the accents’ primary function.**

In the end, all JDP’s analysis verifies is that the accents (regardless of sublinear or superlinear position) work together in a hierarchy of disjunctive and conjunctive values, with one value that is neither interspersed among them. But this result does not **disprove** SHV’s paradigm, but rather **confirms** it in one of the strongest ways possible. This result is precisely what one would expect of a class of degrees having fixed values (one of them being a **tonic degree**, capable of serving in many functions) and a class of melismas having relative values working together to produce a melodic line. **This sort of hierarchical integration is what makes tonal music work – and this fact is something that JDP, by his own admission, is not equipped to understand!**

Some of the sublinear accents are indeed (**syntactically**) subordinated to **some** of the superlinear signs. It takes no computer analysis to verify this – anyone who has read the Hebrew Bible even part way through may understand this readily enough. But what JDP cannot or will not see is that **all** of the superlinear accents are (**tonally**) subordinated to **all** of the sublinear accents. This too has been verified rigorously throughout the Hebrew Bible. It is not something SHV made up; it is inherent in the notation. We are not faced with a paradox here – again, this is consistent with how tonal music works. Regardless of the accents’ actual melodic meaning, their positions above and below the words are not related systematically to either syntax or rhythm. Those positions therefore must concern melody. But we have already seen that melodically, the sublinear signs are predominant in some way, for they alone can form an entire melodic-verbal unit (a verse). Such a unit never employs all of the sublinear signs at the same time; indeed, such a unit never or almost never employs certain sublinear signs at all (because these are so frequently associated with superlinear signs). But this is irrelevant, for it is unreasonable **a priori** that some sublinear signs would be of one musical class and other sublinear signs would be of another. Such a distinction is what one would expect the inherent differentiation between sublinear and superlinear signs to make. (But if for some really strange reason such a mixture of classes occurred,

SHV's paradigm would have revealed it – just as the Masoretic paradigm reveals that both the disjunctive accents and the conjunctive accents contain a mixture of sublinear and superlinear accents.)

*In some cases a melisma will take syntactic precedence over a fixed degree, not only because that melisma has a particular disjunctive effect, but also because it has other effects not at all related to syntax. It still is subordinated tonally to the fixed degrees by definition, because it is a melisma. **The tonal hierarchy and the syntactic hierarchy of the accents are not identical** – they are **interwoven** with each other and with the parallel verbal text. Thus SHV ultimately concluded that the melodies preserved in the Masoretic Text are essentially those of the biblical authors. Such "irreducible complexity" had to exist from the beginning of the biblical Hebrew text (at least as we have it canonized now) or it could not have existed at all.*

*Finally, JDP sets up a straw man here. SHV does not say that the sublinear accents and the superlinear accents do not form an integral system – she says precisely the opposite. What she **does** say is that that sublinear signs are **predominant**. Once more, this is not something she made up – the fact is inherent in the notation itself. It simply is not obvious when one assumes (even if unknowingly) that the notation is primarily syntactic (and especially when one claims nevertheless that it is primarily melodic).*

6. My knowledge of structured systems, such as the structured system of rules governing Hebrew accents, and my limited knowledge of music, makes me very suspicious that any thoughtful mapping of musical features with the elements of any structured system will produce "music" which may indeed sound pleasing. Such music could be thought to be like the "primitive" music of antiquity, when it is merely the product of human ingenuity. This would be particularly true if the structured system were a semi-musical system of rule-governed signs for cantillation. I strongly suspect that is what has happened with Suzanne Haik-Vantoura. My suspicion is that her music of the Psalms is boringly redundant, because many of the lines of the Psalms are short and have essentially the same accents from line to line.

*Working backwards from JDP's last suspicion: why should he think the restituted music of the Psalms "boringly redundant"? Has he ever listened to any of the forty-three Psalms that SHV and Esther Lamandier have recorded to date?³⁴ Or worked through SHV's melody-only score *Les 150 Psaumes dans leurs mélodies antiques*? Far from being redundant, the Psalms display astonishing variety given the rigidity and simplicity of their melodic dress.³⁵ There is nothing "primitive" about that dress – which is why even very small changes in the accentuation from line to line make a considerable difference in how the melody interprets the words. This is all the more evident when one compares variant readings, most of which exist due to one or another scribe's preconceived idea about what the accentuation "should" be according to the Masoretic paradigm. It takes **so little** to disturb the delicate balance between melody and words, above all in the Psalms!*

It has been claimed that in general, the further one goes back in music history, the more predominant vocal music is over instrumental music and the more subordinated to the words the music becomes. It is true that vocal music in which the melodic line is largely independent of and even predominant over the words is a relatively recent phenomenon in Western music. But in world music, the predominance of the words over the music or vice versa no doubt has gone in cycles, due to the competing tendencies

toward what Sachs called **logogenic** (word-born, word-dominated) and **pathogenic** (passion-born, ornament-dominated) extremes. It is these extremes, in fact, which are characteristic of truly primitive music. Even synagogue chant (before the rise of professional cantorial schools in Europe in the 16th century) oscillated between these two extremes without becoming either one. But neither did it strike the happy medium which is characteristic of high culture: what Sachs called **melogenic** music. This music is characterized by the fusion of melodic and verbal syntaxes, a fusion that the Greeks called **melos** (from which we get our word “melody”).³⁶ According to Sachs, the link between the two syntaxes, in ancient Greek music, was rhythm. But in fact, **melos** as a concept was taken further: “The **lexis** (words, speech) and the melody and the rhythm must all be perfect so that the **melos** may be perfect,” said Aristides.³⁷ Add to that the need to have precise instrumental accompaniment, solo and choral arrangements and in some cultures even particular circumstances and **melos** or its cultural equivalent could get demanding indeed.

Here, yet once more, JDP’s self-admitted limited knowledge of music betrays him. It is indeed very easy to impose a self-contained musical system on a pre-existing hierarchical system that has nothing to do with music (or that actually has a different musical value). This has been done many, many times, not least by those who have imposed their own ideas³⁸ of what ancient music “should have been” on the Masoretic accents!³⁹ But JDP misses several points, among which are these:

1. In SHV’s restitutions, Psalms, Proverbs and the body of Job alone have four basic diatonic-chromatic modes or scale types; the prose texts have about twice that number. The modality in any given text is inferred in the same way it is inferred in other musical notations of the same genre. In addition, both systems of biblical accents utilize “variable degrees” similar in principle to those employed by the **musica ficta** of early Western music. Yet as in the earliest Western musical notations (and other kinds of musical notation or its equivalent besides, including the **chironomy**⁴⁰ of Egypt from the Middle Kingdom forward), neither of these factors was spelled out by the biblical accentuation or by the system of gestures it transcribes. Rather, they had to be inferred from the relationship of the melody to the words. Simply looking at the signs used from verse to verse (especially on the tacit, if unconscious, assumption that they are primarily syntactic in function) gives one no indication of these things. One has to understand their basic musical meaning first.⁴¹
2. Many people who impose an arbitrary musical system on a non-musical hierarchy (or a hierarchy that actually has a different musical basis) do so using the **pentatonic** scale. The reason is simple: any sequence of notes in a pentatonic scale is “pleasing” due to the harmonic purity of the intervals within the scale. That is why pentatonic wind chimes are so popular. But it is decreasingly easy to impose the diatonic, the diatonic-chromatic, the chromatic and the enharmonic genres of scale on a rigid, pre-established hierarchy. The hierarchy on which the scale is imposed, and/or the scale itself, and/or the melodic line based on the scale, has to be manipulated arbitrarily more and more in order to get a “best fit”. Synagogue chant presents us with many, many examples of this very thing being done vis-à-vis the Masoretic accentuation. Indeed, such manipulation is at the very heart of any synagogue chant whatever!⁴² Nothing of the kind happens in SHV’s paradigm. As she has written, the absolute guarantee of her deciphering key is its applicability to the entire

Hebrew Bible. Not a note was added, and none was taken away, from the melodic line that her paradigm demanded (even to the point of SHV's respecting obvious typographical errors in the Letteris Edition that should have been disregarded).⁴³ Nothing in her inferred changes of mode and variable degrees was arbitrary; on the contrary, most well-defined passages⁴⁴ use the same basic mode throughout, and what changes there are fall into patterns that are consistent with changes in verbal content and sometimes even with traditional Masoretic paragraphing.⁴⁵

3. *Those who make such arbitrary impositions, with rare exceptions, are **not** trained musicians and composers! Such specialists if anyone **know** when a musical imposition is an arbitrary one. To them, it typically sounds arbitrary at once – because it has either an arbitrary tonal syntax or none at all. But the untutored cannot tell the difference readily (as their own compositions typically illustrate). As a corollary, it is not at all easy to impose a genuinely musical syntax arbitrarily on an unrelated system. Musical beauty (in **melogenic** music), even if **subjectively** perceived and appreciated (or not), has **objective** bases in musical syntax – in other words, **in tonal laws founded in harmonic relationships**. This thing called “beauty” – as founded in inherent mathematical elegance, not in subjective pleasure among all hearers necessarily⁴⁶ – is precisely the quality that artificial impositions lack.*
4. *SHV knew when she was about when she wrote that the “primitive” synagogue chant was not “music worthy of the name”. In most cases, such chant has no inherent syntax – it simply oscillates within a **tetrachord** (at most, within a **hexachord** with few departures) between obvious verbal cadences. Not all musicians (I have found) appreciate the fact when it is presented to them, but probably this is because they have not been trained (as SHV was) to discern the difference between a melody that **expounds** the verbal syntax and a melody that merely **follows** it. This is one of the key differences between the classical and folk music of high cultures and the music of primitive cultures.⁴⁷ If there is one thing that everyone with adequate training acknowledges (one way or another), it is that SHV's restitutions have a definite and even deep musical syntax, are **melogenic**, and therefore expound the verbal syntax in the best possible way. Most (even among her critics) add that the restitutions are exceptionally “beautiful”. This combination of objective and subjective reality is not something that can be imposed arbitrarily. It must have a basis in the notation itself. But if it does, then the Masoretic paradigm and all hierarchical analyses and musical renditions that take its authority as an **article of faith** should be thrown in the dust bin of history where they belong, right along with other partial, systematic but misleading explanations of the world.*

I regret to report these things. I think SHV is a sincere, lovely lady who loves the Bible and music. Her motives and intentions are honorable. But I think she is mistaken to think she has discovered the lost music of the Bible. Having mapped the features of music to the structured system of rules governing the accents, it is begging the question to state that “the syntactical complexities are intuitively obvious via the original music!” They are only obvious from the music because they have been built into the music. That does not make the music original, but merely a musical image of the built in laws of cantillation.

....

Sincerely,

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 Temple Baptist Seminary

*I regret in my turn to say that JDP has the issues **exactly backwards**, and I have done my best to demonstrate where and why. But let us put the subject in its proper perspective. If an **archaeological** notation of any kind whatever was given the same sort of detailed, systematic, yet astonishingly incomplete and empirical interpretation that the Masoretes and their grammatical and cantorial heirs (**including JDP**) have given the biblical accents, that interpretation would immediately and rightly be thrown out by an impartial court. Only argument from traditional and scientific authority has kept the Masoretic paradigm alive for over a millennium. It would not be the first time (far from it) that argument from authority has blinded people to the obvious while keeping them unable or unwilling to consider new ideas.*

*SHV does far more than argue about how the syntactical complexities of the accentuation are **intuitively** obvious. She demonstrates – in terms a **trained musician** can understand with ease⁴⁸ – the **objective basis** of those complexities. She also gives an objective tonal explanation for features of the accentuation which the Masoretic paradigm does not even address (save at times to explain them away). The question really being begged is why JDP sees neither the objective realities nor the subjective ones regarding SHV's work. The answer is plain, and truly regrettable:*

1. *It is specious for JDP to speak of SHV's deciphering key as being "merely a musical image of the built-in laws of cantillation." First, **JDP knows nothing of any such "laws of cantillation" – because his own analysis does not deal with such, but only with the "disjunctive and conjunctive functions" of the "Masoretic accents". In theory and in practice alike, one may attach a virtually endless number of melodic meanings to those functions. The question is whether the attached melodic meanings explain all the features of the accentuation and its relationship to the words. No traditional melodic interpretation of the accents does so! Second, JDP's analysis (yet once more!) fails to explain **most** of the features of the notation and its relationship to the words. SHV's analysis, on the other hand, takes into account **all** these features, as **most** of her reviewers (**all** of whom are qualified musicians and/or composers, **many** of whom are **intimately** familiar with synagogue chant) have noted.***
2. *JDP does not have anywhere near the training in music theory, composition and history to even **understand** SHV's work, let alone to **critique** it. If he did, then he could **figure out for himself** how the syntactic relationships he analyses are implicit in her deciphering key, and how all the other relationships inherent in the notation are as well. He would not need JHW(happy as he would be to try) or anyone else to explain how this is so.*
3. *JDP's unflinching commitment to the Masoretic paradigm is that of one who takes the paradigm as an **article of faith**. As a "true believer" in that paradigm, he cannot or will not admit an explanation of the facts contrary to it, not even when the explanation accounts for everything the Masoretic paradigm addresses **and** for everything that the Masoretic paradigm fails to address. His only logical recourse is to take up SHV's gauntlet and show that hers is not the only explanation that is consistent with every feature of the notation –*

and that is the one thing that neither JDP nor anyone else has ever done. Certainly, no one has (or can) do this starting from the Masoretic paradigm. Yet apparently, JDP hopes against hope that, somehow, someone will succeed (as is evidenced by his thinking regarding the sources of synagogue chant).

- 4. JDP, like countless others before him, has been deceived into accepting the traditional “tables of accents” as containing the original and authoritative “key” to the accents’ meaning – sometimes contrary to what others among his fellow grammarians have realized from examining the Masoretic and post-Masoretic source materials.*
- 5. In addition, JDP has been deceived into believing two contradictory things at once: the accentuation is primarily melodic, and the accentuation is primarily syntactic. As noted above, the reason is that the “tables of accents” he accepts actually treat the accents as primarily syntactic, even as those who originally promoted those tables insisted that the accents are primarily melodic. But if the accents were primarily syntactic, then the formal categorization of accents into disjunctives and conjunctives and their application as such would be much more self-consistent vis-à-vis the verbal text than it is. Only the premise that the accents are primarily melodic can account for their features and layout – **including**, but not **limited to**, their disjunctive and conjunctive functions. But if that is so, then the “tables of accents” JDP accepts are neither original nor authoritative. Rather, they were post-imposed by well-meaning but mistaken medieval scholars on a notation that (by the testimony of at least one of their own) was far older than their time.*
- 6. Accordingly, JDP cannot or will not see that his analysis, whatever its value concerning **one** of the functions of the accents, is but a partial one, because it does not and cannot explain **all three** of the accents’ functions coherently and in the right order.*

*The plain truth is that it is the Masoretic paradigm, **not** SHV’s, that “maps” an arbitrary and incomplete interpretation onto the accentuation. **JDP’s fellow Hebraist and grammarian, GEW, who was in a position to know, confessed as much** (if I understand SHV’s citation of him correctly).⁴⁹ Yet even GEW accepted the Masoretic paradigm as the basis of his analysis, all the while rejecting the explicit testimonies of the **Masorah** and the early treatises regarding the **melodic** value of the accents. No wonder SHV’s thesis was so “dangerous” to his own, by his own admission!*

Argument from authority, in matters large and small, is a dreadful thing – all the more so when people fail to realize how it influences them. I accept SHV’s thesis not because of argument from authority, but because it passes a very close shave with Occam’s Razor. It leads to the simplest and yet the most complete explanation of the available facts – and it even allows me to ask questions of those facts that SHV herself could not address and that no one else has answered heretofore. Only a simpler, yet more complete explanation of the facts would compel me to modify or abandon her paradigm. If such an alternative arose, then I would accept that alternative without hesitation. But then, I have nothing to lose by following the evidence wherever it leads, and at whatever cost to myself. I have done so all my adult life on every issue I have encountered, large or small. I challenge JDP to do the same on this issue.

I do have deeper reasons, however, for accepting SHV’s paradigm, and they are the same reasons that lie behind my confidence that in principle it will not be refuted:

1. Every poet-composer has personal idiosyncrasies and contextual influences that form his or her musical “fingerprint”.
2. The restituted melodies consistently and plainly preserve evidences of both **as consistent with the traditional authorship of the biblical books** (nowhere is this more striking than in the Psalms of David).
3. The restituted melodies not only clarify long-debated matters of exegesis, but they also reveal the attitudes and emotions of the biblical authors in a truly profound way (once again, this latter revelation may be experienced subjectively, but it has a completely objective basis).
4. If there is one thing I have learned and believe, it is that God has preserved His “oracles” in Hebrew through the Jews – often despite themselves (Romans 3: 1-4). If the original cantillation of the Hebrew Bible were part of the “oracles of God”, then it would make sense that it would form an irreducible whole with the words it supports – otherwise, the cantillation could and would not have been preserved. In fact that sort of interweaving is what SHV discovered and (for many texts) has documented in detail in her scores. (Much more could be illustrated by means of cadential and melothematic studies.)

*I submit that the discovery published by SHV (in various forms) as The Music of the Bible Revealed should be revered, and **used**, as the sacred treasure that it is. How much more could a Hebrew student appreciate biblical studies if he knew that he were studying not only the **words**, but the very **thoughts, emotions and attitudes** of the biblical authors and those they discuss? How much debate regarding the meaning of a passage, or sometimes of a whole **book** like the Song of Songs, could be resolved by knowing these things?*

*Some on BaptistBoard no doubt would claim that this subject is “not essential to salvation”. On the contrary, the “word of God” includes **both reason and speech** (the reason being conveyed by the vocal inflection as preserved by the Hebrew and, in the New Testament, by the Greek accents). Every bit of Scripture – including “**how** it says **what** it says” through its accentuation⁵⁰ – is **inspired** and thus profitable for equipping one for his or her Christian service (2 Timothy 3: 16-17). If people can succeed in “thinking outside the box” long enough, they will be astonished at what they can learn through this discovery, even with regard to what most would consider “saving knowledge”. I was – and remain so.*

Sincerely,

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¹ Prof. Zakai worked with SHV in recording the Alienor CD *Visages antiques d’Israël* (see my [Recordings](#) page).

² Even SHV’s style of musical composition was widely acknowledged as idiosyncratic. This may have given her an unexpected advantage in grappling with the biblical cantillation and the idiosyncrasies of its original authors.

³ Paul Kahle, *The Cairo Geniza* (Oxford University Press, 1959), p. 82-84. Cited by SHV, *The Music of the Bible Revealed*, trans. Dennis Weber, ed. John Wheeler (BIBAL Press/King David's Harp, Inc., 1991), pp. 499-502, 519; cf. pp 510-511, 521-522.

⁴ SHV, *op. cit.*, pp. 501-502, 519.

⁵ SHV, *op. cit.*, p. 447.

⁶ All editions published before the 20th century, and some during it, have had such errors.

⁷ The accentuation of the Letteris Psalms has been my special study for the past few years. During that study (during which Dr. Duane Christensen, doing a parallel and more detailed study of the accentual-verbal syntax which he calls "logoprosodic analysis", asked me many times for clarifications), I discovered that the Masoretic paradigm does not completely explain even how the accentuation divides the verses into phrases and clauses – let alone any other important feature of the notation or of its layout vis-à-vis the words.

⁸ This comment is found in a paper presented (in French) by SHV at the Conference of the Society of Jewish Studies at Paris, May 25, 1981. It reflects her understanding at that point of the historical and technical background behind the twofold "traditional" (i.e., syntactic and associated quasi-melodic) interpretation of the accents as put forward by the Masoretes, the early grammarians and cantors. It also contains her responses to claims put forward by GEW in his first published concordance of the accents. Dennis Weber translated the paper into English as *The Real Meaning of the Musical Signs of the Bible*; it has been edited and [republished](#) by JHW.

⁹ Jules Derenbourg, "Manuel du Lecteur", in *Journal Asiatique*, Oct.-Nov.-Dec. 1870, p. 527.

¹⁰ SHV documented these factors in her last published score, *Message biblique intégral* (Paris: Foundation Roi David, 1992). (This score is available from JHW on CD-ROM, as are SHV's other melody-only scores, in PDF format.) She summarizes the factors (on p. T-40) as 1) the **polarity** (relative pitch) of the melodic line; 2) the **axiality** of the pitches relative to the tonic or 1st degree; 3) the **melodic accentuation** given by the melismas; 4) the **modality** used in the cantillation. Another factor, which she treats (on p. T-34) as a subset of 2), may be treated separately as 5) the **texture** of the melody as defined by the succession of sublinear degrees. One could go further, and show how each of these **musical** factors corresponds to a **semantic** factor in the verbal text, and how all these musical and verbal factors are **interwoven** to form the **melos** that is biblical chant.

¹¹ I only wish that the meaning of the Hebrew accentuation were the only subject that this phenomenon has affected!

¹² The first edition of SHV's French book was published in 1976. The second edition was published in 1978. GEW published his **Concordance de la Cantillation du Penteteuque et des cinq Megillot** in 1978 – at least two years **after** SHV asked him to review her work, if his review appeared in her first edition.

¹³ Unfortunately, or not depending on one's point of view, GEW did not live long enough to see this done.

¹⁴ Derenbourg, *op. cit.*

¹⁵ "They [the Masoretes] spied out the slightest nuance, noted not only the separations but the connections...they built up a veritable hierarchy, a feudal system of accents, all rather burlesque, which misled several great scholars of the 15th, 16th and 17th centuries" (Derenbourg, *op. cit.*, p. 528).

¹⁶ One grammarian cited by JDP (and who is familiar with Derenbourg's work) has this to say: "Christian scholars of the Renaissance classified the accents according to their pausal value into four grades, to which they gave names such as 'emperors, kings, dukes, counts'. Some Jewish writers, such as Zalman Hanau and ben Ze'ev, also used this type of classification. Wickes opposed classification in this way (as did others before him), and indeed it does give a false impression of the

accent system. One cannot argue that the pause after one accent must be longer than the pause after another. The value of the accents is relative." Israel Yeivin, *Introduction to the Tiberian Masorah* (Scholars Press, 1980), pp. 168-169.

¹⁷ This relates to how SHV's definition of an "accent" differs from that of the Masoretic paradigm in several significant cases. JDP addresses several of these cases, as do SHV in her book and paper and (here in response) JHW. For SHV's analysis of the problem, see her book, *op. cit.*, pp. 166-178. For example: "Each sign fits very well with the verbal syntax, but none of them has an **irreversible** divisive function. Hence, no sign is strictly disjunctive or conjunctive" (p. 168). But this only becomes evident when one knows the original musical meaning of the signs (*ibid.*) – that is, when one has the one and only explanation that fits all the facts, as SHV did. Whereas starting from the premise that each "accent" **is** strictly disjunctive or conjunctive (as SHV notes) paralyzed all attempts to analyze the **signs** as such from the very beginning (cf. p. 169 and her paper)!

¹⁸ And may JDP live long enough to see JHW try!

¹⁹ Curt Sachs, *The Rise of Music in the Ancient World: East and West*, 1st Ed. (New York: W.W. Norton & Co., Inc., 1943), pp. 85-86.

²⁰ Despite this, the Jews at the time assumed that **all three kinds** of accentuation represented the oral tradition preserved in the Rabbinic synagogues, which itself was confounded with the tradition of the Temple – cf. SHV's citation of the **Mahzor Vitry**, ca. 1100 AD, on this point (pp. 65-66).

²¹ James Rawlings Sydnor, "The Hymn Society's New Hymnal," in *The Hymn* (Volume 44, No. 3, July 1993), pp. 7-8.

²² With regard to both, a most useful source is *Karaite Bible Manuscripts from the Cairo Geniza* (Cambridge University Press). Another is *Introduction to the Tiberian Masorah* by Israel Yeivin (*op. cit.*).

²³ Cited by SHV, *op. cit.*, pp. 133, 136.

²⁴ We face a similar situation with many systems of early Christian neumes, if not for exactly the same reasons. These notations – generally both much more complicated and much less efficient than the biblical Hebrew accentuation – were "memory aids" for melodies already known by those who used them. Generally, the master musicians did not leave explanations of what the notations signified, and the character of the notations makes them difficult if not impossible to decipher vis-à-vis the parallel verbal syntax. Only thanks to later notations that give more information do we know about the melodies that the earlier notations inefficiently transcribed.

²⁵ An additional complication exists for the name **azla**: grammarians attach it to a superlinear accent that looks like **pashta**, while cantors attach it to a superlinear accent that looks like **geresh** (yet once more, as the Masoretic paradigm would define these accents).

²⁶ "As is said in Nutt, 1870, text p. 129 (ascribed to Ḥayyuj): 'Ga`ya is the opposite of **maqfep** [the hyphen], because **maqfep** joins words while **ga`ya** separates them.'" Yeivin, *op. cit.*, p. 264.

²⁷ On the other hand, the Ginsburg Edition actually **adds ga`ya** in places where it should not be used according to SHV's paradigm, even if such usage is within the bounds of the Masoretic paradigm. Ginsburg's "scribal habit" (or "editorial policy"), compared with those of many other editions and manuscripts (as well as those inherent in the "ben Asher/ben Naphtali" lists of variant readings), shows how many contradictory readings one may derive when one attempts to decipher a primarily melodic notation as if it were primarily syntactic, and then make the notation more "syntactically self-consistent" according to one or another criterion.

²⁸ Does it also surpass the Jerusalem Crown Edition, which is based mostly on the Aleppo Codex of Aharon ben Asher himself? That would be a most interesting result! When my copy arrives, I will find out.

²⁹ Yeivin, *op. cit.*, p. 216. However, the rules for its use are outlined comprehensively in the *Diqduqê ha-Te`amim* (*ibid.*). Ironically, then, the rules

given in this treatise for **paseq** are (according to Yeivin, in several other references) considerably more comprehensive than the rules inferred therein for the disjunctives, conjunctives and **ga`ya** themselves!

³⁰ Sachs, **op. cit.**, pp. 35, 65. It is important to read Sachs' comments about the "final" in their contexts. His comments first of all point out "the discrimination between...two tones: the full-cadential note, being the goal of the melodic trend, took the ascendancy over the half-cadential note, and the later conception of a *final* (to avoid the misleading word tonic) was prepared" (p. 35). This distinction (which is the consequence of "uniting two phrases with cadential distinction to form what musical theory calls a *period*" in "the *lied* form") is the musical equivalent of the distinction between the two most important "disjunctive" functions in the Masoretic paradigm! (How then can JDP assert, as he does later, that one begs a question when one points out that these and other "disjunctive" functions are **intuitively obvious** in the tonal relationships within SHV's restitutions? Literally **everyone**, from "primitive" musicians on up, **perceives** such distinctions through tonal relationships, as Sachs implies! This is a matter of empirical fact as well as of theory.) Page 65 deals with how the *finalis* or tonic is used in medieval Western music theory, in comparison to ancient and world music theory. Clearly, different peoples in different times and places have used the fundamental concept of the *final* in different ways, some more hierarchical in nature than others, some using different kinds of hierarchy than others, yet all grounding their theories and practices in acoustic and psychoacoustic laws.

³¹ Sachs makes some valuable comments about the basic nature of **melogenic** music (such as one finds in SHV's restitutions). "Melogenic music represents the wide middle area between the extremes of logogenic and pathogenic music....On this melogenic level, both the logogenic and the pathogenic styles are submitted to *structural intervals* as a second principle of organization....Most melodies exceeding the range of a third...tend to crystallize in certain intervals; that is, spaces determined by simple proportions of vibration numbers: the ratio 2:1, which we call the octave; 3:2, the fifth, 4:3, the fourth. The strongest magnetic power emanates from the fourth – for physiological reasons it is here best to accept without attempting discretionary explanations. Such magnetic attraction appears in two forms [in effect, to the intervals of the *tetrachord* and the *pentachord* within the octave on the one hand and to either boundary note of the octave on the other, the second of which] leads as a natural consequence to the organization of melody in main and accessory notes. And here the way opens into the complex structures of more highly civilized peoples" (**op. cit.**, pp. 42-43). Here are the *reasons why* the octave, fifth and fourth intervals and the 1st, 4th and 5th degrees are so *prominent* and so *universal* in world music! Here too are the reasons why the attraction to the 1st and the 4th degrees (but also to the 5th degree) is so strong in SHV's restitutions (especially those of psalmody).

³² Again, this is not to say the Masoretic paradigm (or JDP's analysis based on it) is **completely** wrong. It is simply **incomplete**, because **it takes the functions of the accentuation in reverse order** (thus leaving **two** of those functions with little or no coherent explanation).

³³ Dr. Randall Buth's course, *Living Biblical Hebrew for Everyone* (Level II), gives some examples of such readings in its grammatical notes.

³⁴ Just those found on the original recording would shoot down JDP's suspicion in flames. The addition of those on Volumes 2 and 3 would **annihilate** it. Lamandier's solo performances on *Psaumes de David*, by the very fact that they are solo performances by a soprano, suppress some aspects of the original psalmody performe. Nevertheless, their expressiveness and variety should seal the tombstone over JDP's suspicion.

³⁵ The "didactic" Psalms do have rather repetitive melodies, and for the same reason that the Books of Proverbs and Job do: they are composed of short verses in which the melodic lines are more for intellectual guidance than for emotional

expression. (Consider then the triumph of expressive musical composition that Psalms 119 represents; in the Letteris Edition, no two verses in a row are exactly alike in their melodic lines!) It is a truism in musicology that the more linguistic content a sung text contains, the less musical content it will contain and vice versa. But also, the longer a verse of biblical psalmody (and biblical prose for that matter) is, the more opportunities it offers for melodic expression. Aharon ben Asher noted this last point himself in his famous treatise. All the same, there is nothing “primitive” about even the most repetitive melodies of SHV’s restituted biblical chant, as “primitive” chant lacks the detailed hierarchical organization that everywhere characterizes biblical chant (and the accentuation system itself, of course).

³⁶ Cf. Sachs, *op. cit.*, pp. 41-43. **Logogenic** music and **pathogenic** music are the opposite extremes of the song of “primitive” peoples. **Logogenic** music is “word-born” music, in which the words all but completely dominate a very simple, repetitive melody. **Pathogenic** music is “passion-born” music, which is “due to an irresistible stimulus that releases the singer’s utmost possibilities” – so much so that the primitive singer may lend “all his force and passion to the beginning of the song and [let] the melody drop as his vocal cords slacken, often passing to a scarcely audible *pianissimo*” (p. 41). **Melogenic** music – the wide, yet fundamentally different, middle area that the “primitive” musician is not trained to grasp – interweaves melody and words in a structurally organized way. It is (as SHV might have put it) “**melos-born**” (**melos** being the word used by the Greek music theorist Aristides for precisely this sort of interweaving – see main text). It is the music of more “advanced” peoples, yet it may go to nearly logogenic and pathogenic extremes (those extremes, in our modern popular culture, being perhaps best exemplified by and best known in the music of African-Americans).

³⁷ Cited by SHV in *Les 150 Psaumes dans leur mélodies antiques*, 1st bilingual edition (Paris: Fondation Roi David, 1985), p. T-24.

³⁸ Usually, and from the Masorettes themselves forward, the conclusions of these would-be decipherers have been much like the musical norms of their own times! A few have accused SHV of having the same fault. SHV has convincingly shown otherwise; but at least as devastating to that accusation is the testimony of Denise Jourdan-Hemmerdinger (DJH), musicologist and Hellenist at CRNS in France, who shows in detail how SHV’s results coincide with the evidence we have concerning music theory in ancient Greece and Mesopotamia. That evidence (DJH clarifies) is something that SHV **could not have known about in advance**. Coupled with what SHV showed she **did** know in advance, it is unreasonable to argue any longer that hers was an arbitrary imposition of modern Western music theory on the biblical notation. Her deciphering key fits the norms of ancient classical music, not of any form of medieval or modern Western music. Inevitably, some similarities with medieval and modern music may be traced; but this is partly because biblical chant draws from a well close to the source of world music (Mesopotamia), and partly because psychoacoustics and acoustical physics guarantees that people will discover certain universal musical principles independently at different times and places.

³⁹ Ironically, one recent (and post-SHV) effort to “decipher” the accents does exactly this. R. Daniel Meil Weil’s (DMW’s) book, *The Masoretic Chant of the Bible* (Jerusalem: Rubin Mass Ltd., 1995) assumes the basic accuracy of the earliest testimonies concerning synagogue chant **and** a direct link between the synagogue chant and the Temple chant. It also assumes the truth of the Masoretic paradigm, while ignoring most of the features of the accentuation itself and its layout relative to the words. Weil’s “analysis” thus contains all the faults of the Masoretic paradigm and adds some new ones. Seldom has anyone expended more effort to obtain, and then to defend, results that are so paltry. DMW’s book is a mountain laboring to bring forth a stone.

⁴⁰ **Chironomy** is the use of gestures of the fingers, hand and in some cases the arm to represent musical values such as notes and groups of notes. Hans Hickmann deciphered that documented by the pharaonic **mastabas**; he also showed that this chironomy allowed for, but did not directly indicate, the use of different modes and variable degrees within an eleven-degree pentatonic scale.

⁴¹ Psalms 33 has good illustrations of this point, especially in the modal transitions from verse 5 to verse 6, verse 11 to verse 12, verse 15 to verse 16, and verse 17 to verse 18. Two different modes, with the occasional use of a variable 3rd degree, are used in this Psalm under SHV's paradigm. The alternation in pitch of just **one degree** of the scale – the 3rd degree – plus the alternation between choirs implicit in the melodic-verbal thematic structure, not only avoids musical monotony, but brings out the verbal meaning in a simple, yet very effective way!

⁴² This is one reason why different chant styles even within the same rite, and improvisation within the limits of any one chant style, are allowed and even encouraged by the synagogues. Such improvisation (which is characteristic of **folk** music, both primitive and sophisticated) completely disregards most of the features of the notation; and it is impossible within SHV's paradigm. In the latter, every nuance of the notation and its relationship to the words must be respected, otherwise the original chant cannot be reconstructed reliably. It treats the original biblical chant as **classical** music. In fact SHV (and her volunteer assistants) had to learn by experience when all the factors were being taken into account; and sometimes SHV had to change her judgment accordingly. This is why her score of the Book of Ruth (in *Quatre Meghilot*) does not match in all places the melodic and modal nuances of the same book as performed by Mira Zakai (on *Visages Antiques d'Israel*). But this kind of challenge is no different in principle from the challenges faced by the Benedictine monks when trying to decipher the earliest manuscripts of Gregorian chant.

⁴³ Not surprisingly, JDP seized upon such examples in our correspondence as evidence against SHV's thesis. This kind of argument from technicalities is a logical fallacy – and (along with much else) proved to me JDP's commitment to the Masoretic paradigm at all costs. As noted, the correction of such typos makes SHV's case stronger, not weaker. Now a coherent musical system can withstand a certain amount of anomaly or distortion in transcription without being shattered; otherwise, certain hymnbooks (including one long maintained in successive editions by my own church) would never pass muster among the congregants, let alone among the worship leaders! Yet when one corrects those anomalies, no trained musician in his right mind would fail to acknowledge the corrections and give thanks for them. I therefore owe JDP a considerable debt; he came to certain very right and helpful conclusions. Unfortunately, he did so for the wrong reasons. The errors in Letteris that he pointed out are not erroneous because they defy the Masoretic paradigm, which explains practically nothing outside of disjunction and conjunction. They are erroneous because they defy the melothematic (melodic-verbal) laws that actually govern the accents (because these laws account for **all** of the accents' features and not just **one category** of those features).

⁴⁴ This may consist of whole strophes within a chapter, whole chapters, whole songs such as many Psalms, or even whole books such as Ecclesiastes.

⁴⁵ As Aristotle pointed out about ancient music, when the words changed, the music changed as well.

⁴⁶ Not everyone, after all, has had the experience in world music or the knowledge of Hebrew to appreciate SHV's restitutions immediately. (There is also such a thing as "new wine being put in old wineskins".) For my part, I have said to cantors and others that listening to SHV's renditions "ruins my ear" for the Ashkenazic "tropes" and vice versa. These two chant styles approach the Hebrew verbal text in completely different ways.

⁴⁷ This is not to say that the "primitive" chant is devoid of subjective passion and beauty. On the contrary, being the creation of a people that has suffered, endured

and hoped for so much, it is often heavily freighted with both. But the passion and the beauty alike are by turns rustic and otherworldly, because the foundation of the chant in tonal syntax is very thin. It is no surprise that a primitive liturgy (as opposed to a classical one, such as that of the Temple) should be this way. Some chants probably qualify as being of a more developed kind – “folk” chants of relatively advanced local cultures – and SHV probably did not make sufficient distinction of these. The point remains, **none** of these chants (least of all in Psalms) explain **most** of the features of the accentuation that allegedly transcribes them or their parent stock!

⁴⁸ It must be said that SHV found it all but impossible to explain her deciphering process to non-musicians – as Luanna Parker reported when she interviewed SHV for National Public Radio in 1986.

⁴⁹ Cf. Weil, **op. cit.**, p. X. Cited by SHV in her paper, **op. cit.**, p. 6.

⁵⁰ Here we deal with the issue of the *nature* of inspiration. As with the words, so with their accompanying systems of accentuation, one may say that the Hebrew and Greek Scriptures had “verbal and plenary inspiration in their original manuscripts.” Considering the level of detail involved, the possibility of scribal and typographical error, and how much textual criticism has been applied by the scribes, grammarians and editors of printed editions for so many centuries, the fact that the Letteris Edition (and secondarily the Ginsburg Edition) is as good as it is with regard to the accentuation is nothing short of miraculous.