

DANIEL BOYARIN

The Talmud –  
A Personal Take

Selected and edited by  
TAL HEVER-CHYBOWSKI

*Texts and Studies in  
Ancient Judaism  
170*

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**Mohr Siebeck**

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170





Daniel Boyarin

# The Talmud – A Personal Take

Selected Essays

edited by  
Tal Hever-Chybowski

Mohr Siebeck

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For Chava, who accompanied all of this.  
May she thrive.



## Table of Contents

|                            |    |
|----------------------------|----|
| Original Publication ..... | IX |
| Introduction .....         | 1  |

### *I. The Linguistic Gambit*

|  |    |
|--|----|
| The Loss of Final Consonants in Babylonian Jewish Aramaic (BJA) .....                                  | 13 |
| On the History of the Babylonian Jewish Aramaic Reading Traditions:<br>The Reflexes of *a and *ā ..... | 19 |

### *II. Pilpul*

|   |    |
|---|----|
| “Pilpul”: The Logic of Commentary ..... | 47 |
|---|----|

### *III. Gender and Sexuality*

|  |     |
|--|-----|
| Literary Fat Rabbis: On the Historical Origins of the Grotesque Body .....                   | 69  |
| Reading Androcentrism against the Grain: Women, Sex, and Torah-Study                         | 101 |
| Are There Any Jews in “The History of Sexuality”? .....                                      | 123 |
| Rabbinic Resistance to Male Domination: A Case Study<br>in Talmudic Cultural Poetics .....   | 145 |
| Homotopia: The <i>Feminized</i> Jewish Man and the Lives of Women<br>in Late Antiquity ..... | 165 |
| Torah Study and the Making of Jewish Gender .....  | 199 |

*IV. Judaism and Christianity*

|  |     |
|--|-----|
| Martyrdom and the Making of Christianity and Judaism .....               | 229 |
| A Tale of Two Synods: Nicaea, Yavneh, and Rabbinic Ecclesiology .....    | 273 |
| Two Powers in Heaven; or, the Making of a Heresy .....                   | 305 |
| Archives in the Fiction: Rabbinic Historiography and Church History .... | 339 |

*V. The Bavli in its Hellenistic World*

|   |     |
|---|-----|
| Why Is Rabbi Yohanan a Woman? or, A Queer Marriage Gone Bad:<br>“Platonic Love” in the Talmud ..... | 357 |
| Hellenism in Jewish Babylonia .....   | 379 |
| Patron Saint of the Incongruous: Rabbi Meir, the Talmud,<br>and Menippean Satire .....              | 405 |
| The Talmud as a Fat Rabbi: A Novel Approach .....   | 431 |
| <br>  |     |
| Bibliography .....  | 447 |
| Index of Primary Sources .....  | 479 |
| Index of Personal Names .....   | 488 |

## Original Publication

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Reading Androcentrism against the Grain: Women, Sex, and Torah-Study

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Are There Any Jews in “The History of Sexuality”?

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Homotopia: The Feminized Jewish Man and the Lives of Women  
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## A Tale of Two Synods: Nicaea, Yavneh, and Rabbinic Ecclesiology

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## Why Is Rabbi Yohanan a Woman? or, A Queer Marriage Gone Bad: “Platonic Love” in the Talmud

First published in *Authorizing Marriage?: Canon, Tradition, and Critique in the Blessing of Same-Sex Unions*, ed. Mark D. Jordan (Princeton, NJ: Princeton University Press, 2006), 52–67, 178–84.

## Hellenism in Jewish Babylonia

First published in *The Cambridge Companion to the Talmud and Rabbinic Literature*, ed. Charlotte E. Fonrobert and Martin S. Jaffee (Cambridge: Cambridge University Press, 2007), 336–63.

## Patron Saint of the Incongruous: Rabbi Meir, the Talmud, and Menippean Satire

First published in *Critical Inquiry* 35, no. 3 (2009): 523–51.

## The Talmud as a Fat Rabbi: A Novel Approach

First published in *Text & Talk* 28, no. 5 (2008): 603–19.

## Introduction

The title of this book involves, as usual with me, a pun. On the one hand, my work has always been Talmud-centered: the Talmud, which to my mind is the greatest cultural production of the People called Jewish, is never far from my mind. I cite it and allude to it in my daily speech regularly and sometimes seem (to myself at least) to have an appropriate quotation or story from the Talmud for any occasion. I love studying and teaching Talmud, so, of course, anything I write about this book is inflected deeply by my love (tough love, love that has gotten tougher over the years). It is a personal take. On the other hand, in a more jocular sense, this book is a personal take because it was, of course, impossible to republish all my uncollected papers; instead, we (I'll flesh out that pronoun immediately) have sought representative examples of the particular stages and phases of my work with Talmud, so this book is a personal take, as it were, on my work itself. The second person of that "we" is the absolutely indispensable Tal Hever-Chybowski, without whom this book would have remained an unfulfilled contract, perhaps forever. Tal was enormously helpful in every way possible, from picking out the papers to be included to fixing errors in them and producing the copy upon which the publisher could base this book.

I'll use this introduction to contextualize the modes of talmudic research that appear in this book. Contextualization is a critical term here. In a sense, or rather in several different senses at different times, I have always sought to open up the Talmud by allowing its intellectual (and other) problems to listen to and speak to the concerns of disciplines in the humanities. These different disciplinary openings have been the *Spielbein* of my work, while the talmudic (and associated) texts have been the *Standbein*. In ancient statuary, one leg, the *Standbein* (the standing leg), is always firmly on the ground, while the other leg is in motion: the *Spielbein* (the playing leg). One of my legs has always been firmly on the ground of the Talmud and the other always in motion. As I tell my students even now, learn one thing very very well, and then always work with other things, new things, things that you don't know very well. To use another ancient image, be a strange monster, a hybrid of a hedgehog and a fox.

The first game to catch my eye was historical linguistics. The earliest of the publications in this book appeared even before I had my PhD: "The Loss of Final

Consonants in Babylonian Jewish Aramaic (BJA).”<sup>1</sup> Since I had been studying some very venerable manuscripts of the Talmud plus one exceedingly important Gaonic work in Babylonian Aramaic, the famous *Halakot Pesuqot* (HP) manuscript, I’d noticed a phenomenon in the orthography of these manuscripts attesting to the more than sporadic (but less than regular) loss of certain root consonants at the end of verb forms. Using some concepts that I’d learnt by studying historical linguistics at Columbia while working on my MA thesis there, I concluded that these losses were not phonetically, but morphologically originated, occasioned by a misanalysis on the part of speakers of the language of certain verb forms. This was really my first exercise of the *Spielbein*, and I discovered that I rather liked that play.

We’ve included another, more complex and developed article from this phase and stage, one in which seemingly random spelling variation in the HP was shown to manifest statistically significant conditioning and thus to reveal important and interesting information about phonetic variation in the Aramaic of Jewish Babylonia: “On the History of the Babylonian Jewish Aramaic Reading Traditions: The Reflexes of \*a and \*ā.”<sup>2</sup> I am astonished now that I had the patience both to learn the details of linguistic theory involved in that research and to think through the extremely complex details required to discover the patterns behind the seemingly random variations. Looking from this distance, it is perhaps difficult to discern the significance of that work, although it seems to me that establishing the vocalization of the HP manuscript as the closest witness to a living tradition of Babylonian Jewish Aramaic speech is of potential importance in different ways.

The next stage of my work took a completely new turn. During the last years of my graduate study with Prof. Dimitrovsky, OBM, he had offered a year-long seminar on the history of talmudic interpretation. Although all of the seminar was riveting, the part that grabbed me the hardest intellectually was the introduction to the early modern methods of talmudic interpretation known collectively as *pilpul*, which I would gloss now, for reasons that should become apparent, as “close reading.” Prof. Dimitrovsky had done more than any other scholar to describe what was at stake in this mode of reading of the Talmud, in both its Sephardi and Ashkenazi versions, and I found the method of reading itself, for itself, riveting. A love for this material was formed during that semester, which bore fruit only upon completion of my dissertation. I produced, as my first book, a work in Hebrew called *Ha’Iyyun ha-Sefaradi (The Sephardic Speculation)* on how the methods of talmudic interpretation that developed in the late medieval and early modern periods under the rubric of *pilpul* consisted of an integration of

<sup>1</sup> Daniel Boyarin, “The Loss of Final Consonants in Babylonian Jewish Aramaic (BJA),” *Afroasiatic Linguistics* 3, no. 5 (1976): 19–23.

<sup>2</sup> Daniel Boyarin, “On the History of the Babylonian Jewish Aramaic Reading Traditions: The Reflexes of \*a and \*ā,” *Journal of Near Eastern Studies* 37, no. 2 (1978): 141–60.

modes of thought from philosophy, biblical interpretation and Talmud. As such, they represented yet another mode of cultural integration, namely the integration of talmudic interpretation, the most “Jewish” of intellectual enterprises, into the thought world of scholasticism, beyond the borders of Jewish culture per se. The paper included here in this collection, “*Pilpul*: The Logic of Commentary,”<sup>3</sup> was the only publication in English that I produced on that topic.

Interestingly enough (at least for me), it was the research on *pilpul* that led me in another new direction in my work, one that has proven most fruitful in many directions. This interest led me first to Yale University for a sabbatical in 1983–1984, the very epicenter then of the literary theory wave in the US, and then to the School of Criticism and Theory (AKA “theory camp”), which was held in those years at Dartmouth College (not at Cornell University): a six-week intensive summer program for advanced graduate students and younger scholars (I was younger then). Between both of these experiences, I was introduced to two of the most active of my later *Spielbeine*: gender studies, on the one hand, and the Russian theorist and critic, Mikhail Bakhtin, on the other. The combination of these two explains the presence of the next paper in this collection: “Literary Fat Rabbis: On the Historical Origins of the Grotesque Body.”<sup>4</sup> Many of the themes that would dog my work for the next several years were first adumbrated in this paper: the rabbinic male body as feminized (gender), the text of the Talmud as discourse (poetics of culture), the role of the grotesque in the Talmud (Bakhtin), and the problems of fecundity and reproduction. This article first articulates a theme that is still current in my scholarship decades later: the relation between Greco-Roman and Eastern cultural materials in the world of the Babylonian Talmud. Over the years I developed more complex models, but this article was a beginning. It’s worth mentioning too that several of the incidents first introduced in the texts translated and partly interpreted in this article were later returned to, some of them – Rabbi Yohanan and Resh Lakish and the story of the Fat Rabbis – more than once. Anyone who happens to read this essay and remembers or rereads the introduction to Bakhtin’s *Rabelais and His World* will see how completely I was (and still am – albeit critically) besotted with Bakhtin, as evidenced indeed by my 2009 book *Socrates and the Fat Rabbis*.

In any case, the next move within the collection found in the present book was to more directly feminist work: “Reading Androcentrism against the Grain: Women, Sex, and Torah-Study.”<sup>5</sup> Reading this essay on Beruriah again after a quarter of a century, I see how deeply it was influenced by structuralist theory (this is not meant as a repudiation).

<sup>3</sup> Daniel Boyarin, “*Pilpul*: The Logic of Commentary,” *Dor Ledor* 3 (1986): 82–106.

<sup>4</sup> Daniel Boyarin, “Literary Fat Rabbis: On the Historical Origins of the Grotesque Body,” *Journal of the History of Sexuality* 1, no. 4 (1991): 551–84.

<sup>5</sup> Daniel Boyarin, “Reading Androcentrism against the Grain: Women, Sex, and Torah-Study,” *Poetics Today* 12, no. 1 (1991): 29–53.

In “Are There Any Jews in ‘The History of Sexuality?’,”<sup>6</sup> I moved from gender studies per se to the consideration of the “history of sexuality,” as argued for by Foucault in his famous work. As is surely well known by now, Foucault claimed that the concept of “sexuality,” that is, the division of people into homo- and heterosexuals was a (relatively) modern invention. I raised the question of whether ancient Hebrew/Judaic culture wasn’t a counterexample, since, as is often claimed, homosexuality is roundly condemned within that culture. In the article, using multiple examples from Bible and Talmud and different modes of analysis, I showed that not only is Foucault not refuted by the biblical and talmudic culture, but that his argument is strongly borne out. Both biblical and talmudic texts confirm rather than refute Foucault’s general hypothesis of the “history of sexuality.” Neither of them excludes sexual practices from the general categories of forbidden and permitted.

My article “Rabbinic Resistance to Male Domination: A Case Study in Talmudic Cultural Poetics”<sup>7</sup> continues the themes of the Beruriah article discussed just above. In this paper, I laid out a certain political program for the work itself. I quote it at some length as it clearly describes the way I was thinking and working throughout the 1990’s:

I want to begin, however, by stating something of my personal/political commitments in this discourse, at least to the extent that I am conscious of them. I desire to empower a change of gender relations within the communities of Jews who are dedicated to maintaining a powerful connection with the Talmudic tradition. This statement, unpacked, demonstrates two motivations: a progressive feminist motivation and a conservative religious and cultural one. I wish to change the practice of Judaism out of a moral, political commitment, but I wish to change Judaism because of another urgency: the need to have a Judaism to hold on to and pass on. Jews (or others) who simply find the memory of Talmudic Judaism irrelevant will not respond to the political force of this inquiry except perhaps vicariously.

My assumption is that we cannot change the actual past. We can only change the present and the future; yet this involves changing our understanding of the past. Unless the past is experienced merely as a burden to be thrown off (which indeed it might be by many), then constructing a monolithically negative perception of the past and cultivating anger at it seem to be counterproductive and disempowering for change. Finding only misogyny in the past reproduces misogyny; finding only a lack of female power, autonomy, and creativity reifies female passivity and victimhood. In contrast to this, recovery of those forces in the past that opposed the dominant androcentrism can help put us on a trajectory of empowerment for transformation. Jana Sawicki has made a similar point in a different context, arguing that some feminist scholars portray the power of reproductive technologies over women’s bodies as such that “our only options

<sup>6</sup> Daniel Boyarin, “Are There Any Jews in ‘The History of Sexuality?’,” *Journal of the History of Sexuality* 5, no. 3 (1995): 333–55.

<sup>7</sup> Daniel Boyarin, “Rabbinic Resistance to Male Domination: A Case Study in Talmudic Cultural Poetics,” *Interpreting Judaism in a Postmodern Age*, ed. Steven Kepnes (New York & London: New York University Press, 1995), 118–41.

appear to be either total rejection of them or collaboration in our own domination.”<sup>8</sup> Instead of this, Sawicki suggests a strategy of paying “constant attention to the ruptures, discontinuities and cracks in the systems of power,” such that “multiple strategies for resisting their dangerous implications” can be developed without either collaborating in domination or total rejection. Since I do not wish to collaborate in domination and certainly not to reject Judaism, the latter type of research can be a powerfully redemptive tool. Precisely and paradoxically, where the culture did not work then, that is where we can make it work for us now.

In the next paper in this series of investigations, entitled “Homotopia: The Feminized Jewish Man and the Lives of Women in Late Antiquity,”<sup>9</sup> I most clearly set out my thesis that Jewish men were read as “feminized” within the dominant European culture because in the traditional Jewish culture of Ashkenaz, the ideal male did not perform gender as the ideal male of the general culture. Instead of the martial ideal of the knight in shining armor, the pale, scholarly student of Talmud was the ideal male, which read as feminine within the gender semiotics of the culture at large. After close analysis of two talmudic stories that portray such feminized positioning for rabbinic men from within the texts of rabbinic culture itself, notably the Talmud, the article is at pains to make clear that such feminized males are not necessarily good for women in the society, not at all:

The politics of this project have, therefore, two faces. The traditional valorization of femininity for Jewish men hardly constituted good news for Jewish women. There is no question that women were disenfranchised in many ways in traditional Jewish culture, and the culture authorized, even if it did not mandate, efflorescences of misogyny as well. If the ideal Jewish *feminized* male has some critical force vis-à-vis general European models of manliness, at the same time a critique must be mounted against “him” for his oppression of Jewish women – and indeed, frequently enough, for his class-based oppression of other Jewish men as well, namely the ignorant who were sometimes characterized as being “like women.”

In the next paper in this collection, “Torah Study and the Making of Jewish Gender,” first published in *A Feminist Companion to Reading the Bible: Approaches, Methods and Strategies*, ed. Athalya Brenner and Carole Fontaine (Sheffield: Sheffield Academic Press, 1997), 515–546, I move in the same general territory, but focus on modernity and female resistance. Rather than emphasize the ways that Jewish ideal gendering through study opposed general European gender standards, I demonstrate precisely how the figure of the feminized scholar constituted a dominating performance of maleness within Jewish culture in Ashkenaz and how some Jewish women in the early twentieth century defied that ideal.

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<sup>8</sup> Jana Sawicki, *Disciplining Foucault: Feminism, Power, and the Body*, Thinking Gender (New York: Routledge, 1991), 14.

<sup>9</sup> Daniel Boyarin, “Homotopia: The Feminized Jewish Man and the Lives of Women in Late Antiquity,” *differences: A Journal of Feminist Cultural Studies* 7, no. 2 (1995): 41–81.

The next section of the book turns in quite a different direction to an area of research with which I am still engaged: the complex histories of what we call “Judaism” and what we call “Christianity” in late antiquity. The papers presented here will index a certain evolution of thinking on my part about these questions over the last twenty years. A good introduction to the whole project can be found in the opening of the first piece in this section, “Martyrdom and the Making of Christianity and Judaism:”<sup>10</sup>

Current historical positions on the origins and history of Christian martyrology generally take one of two positions. W. H. C. Frend, in his classic *Martyrdom and Persecution in the Early Church: A Study of a Conflict from the Maccabees to Donatus*, argues essentially that Christian martyrology is a “prolongation and supersession” of Jewish martyrology. In diametrical opposition, G. Bowersock, in his recent *Martyrdom and Rome*, argues that Christian martyrdom has nothing to do with Judaism or with the Palestinian cultural context of early Christianity, but is entirely a Roman cultural product, adapted for Christianity, and later borrowed from Christians by Jews. Both are dependent on the assumption of a clear and virtually absolute separate identity for the two religions in Late Antiquity. In the current essay, I shall try to show that we need to think of much more complex ways that Christianity and Judaism interacted during the crucial second, third, and fourth centuries, as well as of a much more nuanced understanding of the nature of martyrology itself. Martyrology is an overdetermined, multisourced discourse that undergoes significant development within late antique Judaism and Christianity. Many of the new elements can be shown to be shared by both religious groups, and the direction of “influence” is not only one-way. A model of close contact and dialogue between the two emerging “religions” seems to explain best the historical developments. The present essay is one part of the first of a series of planned monographs on such contact and dialogue with respect to the religious creativity of Judaism and Christianity in Late Antiquity.

Much of the work I’ve done in the last nearly two decades has grown out of that initial statement, while my thinking on these subjects has grown more and more nuanced during that period as well.

The next paper in the selection here is entitled, “A Tale of Two Synods: Nicaea, Yavneh, and Rabbinic Ecclesiology.”<sup>11</sup> In this paper, following cutting edge work on the construction of the Council of Nicaea by scholars such as Virginia Burrus, I have produced a similar account of the so-called Council of Yavneh, at which rabbinic Judaism was allegedly founded, arguing that contradictions in the representation of this “event” within rabbinic literature can be explained by the different ideas and interests of the various groups engaged in producing these representations:

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<sup>10</sup> Daniel Boyarin, “Martyrdom and the Making of Christianity and Judaism,” *Journal of Early Christian Studies* 6, no. 4 (1998): 577–627.

<sup>11</sup> Daniel Boyarin, “A Tale of Two Synods: Nicaea, Yavneh, and Rabbinic Ecclesiology,” *Exemplaria* 12, no. 1 (2000): 21–62.

If Nicaea was a belated Athanasian invention that helped produce a Christianity ‘in which dissent and debate were literally swept aside,’<sup>12</sup> the pluralistic, elastic interpretation of Yavneh was an equally belated talmudic invention. In this “grand coalition,” everybody in Jewish antiquity who wasn’t an outright “heretic” was a Rabbi, and all opinions were equally “Torah.” This late moment of literary crystallization was the juncture at which the “agreement to disagree” was raised to a theological and hermeneutical principle of the highest order, indeed to a divine institution. Just as the story of Nicaea ‘gives rise to the 318 conciliar ‘fathers,’ and also to their only begotten credal Word,’<sup>13</sup> the story of Yavneh gives rise to the father Rabbis and their only begotten Oral Torah.

In the next paper, “Two Powers in Heaven; or, The Making of a Heresy,”<sup>14</sup> I make an intervention that I had not thought to make – or at any rate not thought to make clearly – before, namely to argue that “heresy” in rabbinic Judaism, and particularly the heresy of “Two Powers in Heaven,” is not a dereliction from an original pristine orthodoxy, but rather a means by which that very orthodoxy is constructed. This paper implicitly continues, then, the line of thinking opened up in the one just before it in this selection. It was also the first development of an idea and the interpretation of a set of texts with which I am still busy now (I hope to make my own final statement on these issues in a short monograph within the next year or so, *insha'allah*). For the nonce, for Hebrew readers, a more developed analysis of the talmudic and midrashic texts discussed in this article can be found in my “Once Again: ‘Two Dominions in Heaven’ in the Mekilta.”<sup>15</sup>

The next paper, “Archives in the Fiction: Rabbinic Historiography and Church History,”<sup>16</sup> still within this rubric, moves in a somewhat different direction. This is, or so it seems to me, the most extended programmatic, theoretical and methodological essay I have ever produced, laying out an entire program for a historiography based on rabbinic texts.

The next section is entitled “The Bavli in its Hellenistic World.” The first article that appears under this rubric, paying homage to a celebrated article by David Halperin, is “Why Is Rabbi Yohanan a Woman?, or A Queer Marriage Gone Bad: ‘Platonic Love’ in the Talmud.”<sup>17</sup> In this article, I draw upon Eve Kosofsky Sedg-

<sup>12</sup> Richard Lim, *Public Disputation, Power, and Social Order in Late Antiquity*, The Transformation of the Classical Heritage 23 (Berkeley: University of California Press, 1995), 227.

<sup>13</sup> Virginia Burrus, “Fathering the Word: Athanasius of Alexandria,” in *Begotten, Not Made: Conceiving Manhood in Late Antiquity*, *Figurae* (Stanford: Stanford University Press, 2000), 60.

<sup>14</sup> Daniel Boyarin, “Two Powers in Heaven; or, The Making of a Heresy,” in *The Idea of Biblical Interpretation: Essays in Honor of James L. Kugel*, ed. Hindy Najman and Judith H. Newman (Leiden & Boston: Brill, 2004), 331–70.

<sup>15</sup> Daniel Boyarin, “Once Again: ‘Two Dominions in Heaven’ in the Mekilta” [Hebrew], *Tarbits* 81, Festschrift for J. Sussman (2013), 87–101.

<sup>16</sup> Daniel Boyarin, “Archives in the Fiction: Rabbinic Historiography and Church History,” in *The Cultural Turn in Late Ancient Studies: Gender, Asceticism, and Historiography*, ed. Dale B. Martin and Patricia Cox Miller (Durham, NC: Duke University Press, 2005), 175–92.

<sup>17</sup> Daniel Boyarin, “Why Is Rabbi Yohanan a Woman? or, A Queer Marriage Gone Bad: ‘Platonic Love,’ in the Talmud,” in *Authorizing Marriage?: Canon, Tradition, and Critique in the*

wick's notion that women mediate male homosocial desires, reading the Torah as the mediating feminine in the context of the *havruta* study-pair:

Sedgwick has elucidated how in a "heterosexual" economy of desire, shared female objects (principally women) provide the medium of erotic exchange between men. She refers to this desire as homosocial. The Torah is the authorized female object of erotic, straight desire between Rabbis, in their own powerfully eroticized relationships with one another, particularly through the medium of the study-pair, the *havruta*.

To explore this suggestion, I will be taking a new look at another talmudic text that I have looked at several times in the past but always under the sign of a gender studies, rather than through the lens of sexuality or queer theory. Then, I was primarily interested in the construction of masculinity involved in this text. Now, it is the issue of desire and its fulfillments that concerns me, as well as the ideology of marriage. This new reading, moreover, suggests quite a different way of relating late ancient Jewish sexual culture to that of antiquity.

This shift in focus to universal structures of desire and to the ideology of marriage allowed me to situate the talmudic worldview within a broader, heterogenous framework of the ancient Mediterranean.

Instead of reading the talmudic narrative as oppositional to a posited "Greek" way of thinking, we see it as deeply embedded in the Hellenistic culture of the East and West Mediterranean. The next paper, "Hellenism in Jewish Babylonia,"<sup>18</sup> represents an attempt to incorporate the Babylonian Talmud into a general Mediterranean thought world. While some of the argumentation seems to me today somewhat overdrawn, at least some of the examples remain compelling and the overall case seems valid.

The next paper in this selection, "Patron Saint of the Incongruous: Rabbi Meir, the Talmud, and Menippean Satire,"<sup>19</sup> also situates the Talmud within the wider cultural, discursive, and literary terrain of the ancient world. It places the modern pietistic image of Rabbi Meir, a second-century Palestinian Rabbi, within the hybrid genre of Menippean Satire, characterized by its combination of bawdy, carnivalesque hijinx, and religious and mystical elements. Viewed through what Bakhtin has referred to as the "slum naturalism" of this heterogenous literature, Rabbi Meir emerges as both a man of flesh and saintly miracle worker, in line with the contradictory view of human knowledge suggested by the Talmudic *episteme*:

The modern Rabbi Meir somehow seems – not so surprisingly perhaps – closer in his profile, not only to a St. Jude, but to a nineteenth-century Hassidic *Wunderrabbiner* or a

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*Blessing of Same-Sex Unions*, ed. Mark D. Jordan (Princeton, NJ: Princeton University Press, 2006), 52–67, 178–84.

<sup>18</sup> Daniel Boyarin, "Hellenism in Jewish Babylonia," in *The Cambridge Companion to the Talmud and Rabbinic Literature*, ed. Charlotte E. Fonrobert and Martin S. Jaffee (Cambridge: Cambridge University Press, 2007), 336–63.

<sup>19</sup> Daniel Boyarin, "Patron Saint of the Incongruous: Rabbi Meir, the Talmud, and Menippean Satire," *Critical Inquiry* 35, no. 3 (2009): 523–51.

twentieth-century North African miracle-working saint than to any figure that we could locate in late antiquity. Going back, however, to the time and text in which his “biography” came into being, the Babylonian Talmud, we will find – again not surprisingly – a somewhat different profile: earthier, wilder, indeed an altogether rather indecorous affair. In this paper, I want to interrogate this indecorum itself, making a rather simple attempt to historicize sanctity, by which I mean hagiography (allowing that term its broadest possible scope), arguing that the Talmudic accounts of Rabbi Meir’s life are as characteristic of the culture of the time of their production as the modern accounts are of theirs. The feature that I will focus on most directly in this text is a certain heterogeneity or hybridity in the presentation of the Rabbi in the Talmud, arguing that this very hybridity in the presentation of the holy man’s life is one key to the import of the Talmud itself as a religious document and text.

What renders, however, this discussion an instance of the alleged Hellenism of the Bavli is the close connection between the hybridity of the narratives of R. Meir’s life and the genre known as Menippean satire that was current in the Mediterranean world at about this time, including especially the type of literature known as *spoudogeloion* (“serious laughing”; the seriocomic). One point of comparison is to Petronius’s famous *Satyrice*. I try to show, as well, how the conflicting image of Rabbi Meir points, on the level of forms of knowledge, to the weakening of trust in reason characteristic of the discourse about sophists in this world, with their commitment to a dialogical critique of the institution of the *episteme*.

The final article collected here, “The Talmud as a Fat Rabbi: A Novel Approach,”<sup>20</sup> continues exactly the theme of the previous one, situating the Talmud within a broader literary context by relating it to the dialogical nature of the menippea:

Menippean satire (broadly understood in Bakhtinian terms) provides a most, if not the most, significant formal and perhaps historical context for reading the Babylonian Talmud (ca. sixth century after Christ). Some theoretical considerations on the role of agency in dialogism (intertextuality) are lifted up as well.

The interaction of the halakic *sugya* with the fabulae of aggadic legend exhibits the contact and respective undercutting of dissonant literary elements characteristic of Bakhtin’s system:

If as Bakhtin has put it, “the novelistic hybrid is *an artificially organized system for bringing different languages in contact with one another*, a system having as its goal the illumination of one language by means of another, the carving out of a living image of another’s language,”<sup>21</sup> and if the Talmud itself is, as I have proposed, such a system as well, then it would be an error to reduce that system of languages in contact to a single

<sup>20</sup> Daniel Boyarin, “The Talmud as a Fat Rabbi: A Novel Approach,” *Text & Talk* 28, no. 5 (2008): 603–19.

<sup>21</sup> Mikhail M. Bakhtin, *The Dialogic Imagination: Four Essays*, ed. Michael Holquist, trans. Michael Holquist and Caryl Emerson, University of Texas Press Slavic Series 1 (Austin: University of Texas Press, 1981), 361.

monologic discourse. It is not, accordingly, the polyphony potential in narrative that produces dialogism in the Talmud but the yoking of the narrative with the halakic dialectic, like an ox with a donkey, in the talmudic field that renders the Talmud a dialogical text. I am suggesting that the languages of the halakic *sugya* and of the “wild” biographical *aggada* are deeply antithetical and thus in dialogue with each other. Bakhtin’s claim is that the *menippea* is characterized by “a striking combination of what would seem to be absolutely heterogeneous and incompatible elements: philosophical dialogue [the *sugya*], adventure and fantasticality, slum naturalism, utopia, and so forth.”<sup>22</sup>

The positing of such a genre of literature – whether in the end we call it “novel” or not – in which one could, and I would, read the Talmud seems powerfully compelling.

The End So Far.

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<sup>22</sup> Mikhail M. Bakhtin, *Problems of Dostoevsky’s Poetics*, ed. and trans. Caryl Emerson, Theory and History of Literature Series 8 (Minneapolis: University of Minnesota Press, 1984), 134.

## I. The Linguistic Gambit



## The Loss of Final Consonants in Babylonian Jewish Aramaic (BJA)\*

### *Abstract*

The purpose of this paper is to explain the sporadic loss of final continuants in certain BJA forms as owing to metanalysis of forms in which the final consonant was assimilated to the enclitic preposition [l-]. For example, \*/'marleh/ ("he said to him") developed to /'malleh/, thence to /'maleh/, thence metanalyzed as /'ma/ + /leh/, yielding the new verb form /'ma/ ("he said"). One necessary step in the above development is neutralization of consonant length, implying that such neutralization had already taken place in this dialect. The case for this explanation is strengthened by comparative and intra-dialectal evidence.

One of the striking features of Babylonian Jewish Aramaic (BJA) is that the final consonant of certain verbal roots is often lost. This phenomenon is most common in the late or Gaonic dialect.<sup>1</sup> It is more restricted but still well attested in the normal talmudic dialect, and considerably less prevalent in the special (archaic?) dialect of certain tractates.

All of the consonants so affected are voiced continuants. They are /l/, /m/, /r/, /b/, and /d/.<sup>2</sup> Examples have been gathered by Margolis and Epstein.<sup>3</sup> Among them are

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\* I wish to thank Professors H. L. Ginsberg, Stephen J. Lieberman, and Joseph Malone for their aid in formulating this paper. Also grateful acknowledgement is due the Maxwell Abbell Foundation for Grants-in-Aid enabling me to pursue my studies. I have also discussed some points with my friends, Prof. Richard Steiner and Prof. Yona Sabar. Of course, responsibility for all opinions expressed is my own.

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<sup>1</sup> For a general introduction to BJA and catalogue of the sub-dialects see Jakob Nahum Epstein, *A Grammar of Babylonian Aramaic* [in Hebrew], ed. Ezra Zion Melamed (Jerusalem: Magnes Press, 1960), 13 ff.

<sup>2</sup> In Hebrew and Aramaic /b/ and /d/ (as well as /t/, /k/, and /g/) were realized as continuants post-vocally. In BJA the continuant allophone of /b/ was almost definitely a frictionless, voiced bilabial continuant [w]. Cf. Joseph L. Malone, "Observations on Linguistic Similarity between the Babylonian Aramaic of *Halakot Pesuqot* and Mandaic" [in Hebrew], *Leshonenu* 37 (1973): 161.

<sup>3</sup> Max L. Margolis, *A Manual of the Aramaic Language of the Babylonian Talmud: Grammar, Chrestomathy and Glossaries* (München: C. H. Beck, 1910), 10; Epstein, *Grammar of Babylonian Aramaic*, 57 ff. /n/ and /t/ may be added if we admit the examples in n. 24. In this paper italics will be used for graphemic representation, / / for phonemic, and brackets for occasional phonetic. I am assuming only one front vowel phoneme for BJA, an assumption which, while perhaps questionable, does not affect the argument of this paper.

ʔzl (“he went”) ~ ʔzʔ /ʔza/.

ʔzyl (“going”) ~ ʔzy /ʔāzi/.

šql (“he took”) ~ šqʔ /šqa/.

šqyl (“taking”) ~ šqy /šāqi/.

ʔyqbyl (“I will receive”) ~ ʔyqby /ʔiqabi/.

tyqwm (“it will stand”) ~ tyqw /tiqū/.

qʔym (“standing”) ~ qʔy /qāj/.

qym (“established”) ~ qy /qi/.

ʔymr (“I will say”) ~ ʔymʔ /ʔima/.

ʔmr (“he said”) ~ ʔmʔ /ʔma/.

nšb (“it blew”) ~ nšʔ /nša/.

nyʔbyd (“he will do”) ~ nyby /nibi/.

Many more examples are found in the cited secondary literature, but these will suffice to illustrate the process. To my knowledge, Epstein makes no attempt to account for this phenomenon.<sup>4</sup>

In brief, my explanation is that in certain morpho-syntactic conditions these consonants were assimilated to the initial liquids of common affixes, and later, when long or geminate consonants were not phonemically distinguished from short /nongeminate, the resulting form was reanalyzed and, by analogy, the previous bound form substituted for the previous free form.<sup>5</sup>

Obvious examples of such assimilation are such forms as ʔzynʔ /ʔzina/ (“I am going”) or šqyn /šāqina/ (“I am taking”) from /ʔāzilna/ and /šāqilna/ respectively.<sup>6</sup> It is easy to see why speakers of the dialect would analyze these forms as /ʔāzi/ and /šāqi/ respectively plus /na/, just as /hāwena/ is /hāwe/ plus /na/.

However, the primary operative morpho-syntactic conditioning for the loss of final root consonants was apparently the preposition [l-] used enclitically. Evidence for the enclisis of the prepositions [l-] and [b-] is found quite clearly in the excellent Hamburg MS of *b. Neziqin*.<sup>7</sup> There we find *hyybbh*, written clearly as one word (3x!), *ptrbh*,<sup>8</sup> *ʔybʔyʔln*.<sup>9</sup> In the Columbia University MS of *Pesaḥim*,

<sup>4</sup> Nor does anyone else of whom I am aware. But see Theodor Nöldeke, *Mandäische Grammatik* (Halle, 1875), 50 and cf. n. 21 below.

<sup>5</sup> Determination of sufficient conditions for this development awaits a full-scale phonological treatment of the language including a distinctive feature analysis. The author is presently engaged in the early stages of such a study.

<sup>6</sup> James Alan Montgomery, *Aramaic Incantation Texts from Nippur* (Philadelphia: Philadelphia University Museum, 1913), 143.

<sup>7</sup> *The Babylonian Talmud, Seder Neziqin, Codex Hamburg 165*, facs. (Jerusalem: Maqor, 1969). For an excellent linguistic discussion of this manuscript see Edward Yechezkel Kutscher, “Research on Babylonian Aramaic Grammar” [in Hebrew], *Leshonenu* 26 (1961): 174–77.

<sup>8</sup> Both examples are on *b. B. Qam. 13b*. For the orthography cf. Mandaic *šʔkybbh* (“he sleeps in it”) with *b* written twice. Rudolf Macúch, *Handbook of Classical and Modern Mandaic* (Berlin: de Gruyter, 1965), 12.

<sup>9</sup> *B. B. Qam. 20a*.

there is *maqšwlk* (“they object to you”).<sup>10</sup> Furthermore, Epstein has recorded a form *pršnlhw* (“we have explained them”).<sup>11</sup> Also in the important Spanish MS of ‘*Abodah Zarah*’ published in facsimile by Shraga Abramson<sup>12</sup> I have found *hwtyly* and *tymrwlyh* both clearly written.<sup>13</sup> The existence of such enclitic forms ought not surprise us as such pronunciation is clearly indicated in Biblical Aramaic by the Massoretes, through their use of the *maqḳef*, /wye’mar-leh/.<sup>14</sup> An orthography similar to the ones attested above for BJA is already found in the *Genesis Apocryphon* 2:9 *dkrlk*.<sup>15</sup> In Mandaic, enclitic use of these prepositions is the rule, and therefore it is with some degree of certainty that we may say it was in Babylonian Jewish Aramaic as well.

I wish to propose that the final consonant of the verb was assimilated to the /l/ of the enclitic preposition, and that by metanalysis the resulting bi-radical root morpheme came to be used in positions even where free of the enclitic. I have found intra-dialectal evidence for the assimilation hypothesized, in the forms /qillah/<sup>16</sup> and /mašgillih/.<sup>17</sup> It should be pointed out that all verbs would function with the enclitic /l/ in BJA because it marks direct object, and indirect object, as well as being used ingressively in such phrases as /ʔzal lih/ (“he went”).

Therefore the proposed development of these forms would be, for instance (1) /ʔimar/ (“I will say”), > (2) /ʔʿimarlik/ (“I will say to you”), > (3) /ʔimalik/<sup>18</sup> > (4) /ʔima/ (“I will say”), the most common of these forms. All stages of the process are attested in the above-mentioned form from *Hal. Pes.* where we have also attested *qy ly*.<sup>19</sup> Therefore /qim li/ > /ʔqimli/ > /qil li/, and finally with sim-

<sup>10</sup> *B. Pesah.* 140b. This excellent manuscript has never been published. See Kutscher, “Aramaic Grammar,” 177.

<sup>11</sup> Jakob Nahum Epstein, *Der Gaonische Kommentar zur Ordnung Tohorot* (Berlin: Mayer und Müller, 1915), 150.

<sup>12</sup> Shraga Abramson, ed., *Tractate Abodah Zarah of the Babylonian Talmud* (New York: The Jewish Theological Seminary of America, 1957).

<sup>13</sup> *Ibid.*, 96.

<sup>14</sup> Dan 4:32. See Franz Rosenthal, *A Grammar of Biblical Aramaic* (Wiesbaden: Harrassowitz, 1968), 18 for other examples.

<sup>15</sup> Nahman Avigad and Yigael Yadin, *A Genesis Apocryphon: A Scroll from the Wilderness of Judaea* (Jerusalem: Magnes Press, 1956), photograph n.p.

<sup>16</sup> For /qim lah/. /kiwan dʿtta la qil lah bšum la samka daʿtah/ (“Since a woman is not expert in evaluation she does not depend on it.”) Solomon Sassoon, ed., *Sefer Halakot Pesuqot* [in Hebrew], Reprinted with facsimile, Jerusalem: Maqor, 1971 (Jerusalem: Hevrat Meqitse Nirdamim, 1950), 106. See Kutscher, “Aramaic Grammar,” 173–74. The form *qym l-* in this meaning is of course well attested in talmudic literature.

<sup>17</sup> From *mšqyr lh* (“he sends to him”). Louis Ginzberg, *Geonica*, vol. 2, Reprinted 1968 (New York: Hermon Press, 1909), 104.

<sup>18</sup> Well attested, e.g. Benjamin Menashe Lewin, *Otsar Haggeʿonim, Berakot* [in Hebrew], vol. 1, part 2 (Haifa, 1928), 61.

<sup>19</sup> Sassoon, *Halakot Pesuqot*, 96. /hani mili hika dla qi li bgawih mar qi li bgawih/ (“That applies when he is not well known to me. Master is well known to me.”)

plication of the doubling and metanalysis > /qi li/. Again the transfer of /qi/ and related forms to other environments is easy to imagine and indeed occurs.<sup>20</sup>

It must be admitted that it is not completely impossible to imagine a purely phonological solution to this development, i. e. a vocalic realization of these consonants in final position or some other type of simple apocope.<sup>21</sup> The following considerations, however, render such an explanation considerably less likely: (1) The doubled /l/ in /qillah/ and /mašgillih/ militates against such an assumption; (2) The somewhat skewed distribution of affected phonemes supports the assimilation explanation. While all of the affected consonants could easily assimilate to /l/ (the liquids for obvious reasons, [t] and [d] because of their similar point of articulation to /l/ and /b/ because of its phonetic realization as [w]),<sup>22</sup> it is hard to imagine why general apocope of voiced continuants would not affect all equally. But see n. 5; (3) There are partially related phenomena in closely related dialects which cannot, by any means, be interpreted in such manner:

In Modern Mandaic the final r of AMR ‘to say’ regularly assimilates to the encl. (l). Such assimilated forms are then often written phonetically ‘mala (Morg. 273) emalla ‘he said to her’<sup>23</sup>

Mandaic is a dialect intimately related with Babylonian Jewish Aramaic and therefore it seems to me that *’malla* in Mandaic cannot be separated from *’malih* (“he said to him”) etc. in BJA. Since in Mandaic there is no secondary root AMA, we must interpret this form as assimilation, as of course, the pronounced doubling indicates as well. It follows that the BJA forms are assimilated as well (i. e. and not to be explained as mere scribal survivals of /’ma/ plus enclitic /lih/, preserved orthographically because of the unusual shortness of the verb form). Mandaic, then, represents an earlier stage of the process than does BJA.<sup>24</sup>

In Mandaic, apparently, there was never any back-formation from these assimilated forms, perhaps because of the retention of gemination.<sup>25</sup> In fact, the

<sup>20</sup> Although, ironically, not attested with this particular verb.

<sup>21</sup> Nöldeke, *Mandäische Grammatik*, 50 does refer to “Abfall” of /m/, and does adduce some seemingly clear cases of it. However, this does not damage our argument appreciably.

<sup>22</sup> Cf. n. 2. Also cf. Neo-Syriac /hal/ “give!” < /haw/ < /hab+l/. Theodor Nöldeke, *Grammatik der neusyrischen Sprache am Urmia-See und in Kurdistan* (Leipzig, 1868), 256.

<sup>23</sup> Macúch, *Handbook*, 52.

<sup>24</sup> It does not seem impossible to me that we may parallel this relationship with another. In Mandaic the /t/ of perfect 1<sup>st</sup> and 3<sup>rd</sup> feminine is preserved except before the enclitics Nöldeke, *Mandäische Grammatik*, 225. Joseph Malone, in a review of Macúch’s book has already demonstrated that *both* forms are to be explained by assimilation (Joseph L. Malone, “Review of Handbook of Classical and Modern Mandaic by Rudolf Macúch,” *Language* 45 [1969]: 197–98). Therefore, it seems likely that the Babylonian Jewish forms, generally without *t*, should be explained as analogical extension of the forms with enclitics. The same may be true for forms like /amrittu/ (“you say”), which again in Mandaic only exist before enclitics, otherwise /amrittun/ is found Nöldeke, *Mandäische Grammatik*, 233. Phrases like /’atun ’amritu/ in BJA point in this direction.

<sup>25</sup> For rare exceptions see Macúch p. 37 (cf. Malone, “Handbook (Review),” 200).

only exact Aramaic parallel I know of to the process hypothesized here is in neo-Aramaic, where in Garbell's formulation, "stem final in q-j/O-m is replaced by zero, [...] further, the objective suffixes of the 2<sup>nd</sup> p. sg. m. and f. are optionally (but more frequently, except after the negative particle) affixed to the stem: *qu* (~ *qúlox* m., *qúlox* f.), pl. *qúmun* 'rise' [...] the stem of O-z-l is suppleted by *si* and takes the suffixes described for q-j/O-m; *si* (~ *sílox* m., *sílox* f.), pl. *símun* 'go.'<sup>26</sup> It seems most likely that the suppletion in precisely these two roots and no others is a result of metanalysis of the enclitic forms.<sup>27</sup>

Finally, it should be noted that there are other assimilation-elision phenomena in Aramaic, such as /nezzun/ < /nezlun/ in Syriac. Such phenomena occur in Babylonian Jewish Aramaic as well in nouns, such as /qola/ < /qolra/, /zuta/ < /zutra/. These are always post-consonantal, pre-vocalic, and are therefore irrelevant here as can be shown from their general occurrence in Aramaic,<sup>28</sup> in contrast, of course, to the phenomenon discussed here.

In brief conclusion, the loss of certain root final consonants is to be understood as the result of a syntactically conditioned phonological change, extended beyond its original environments by analogy.

The implications of this analysis, if correct, are two-fold: philological and linguistic. As to the first, Babylonian Jewish Aramaic cannot be used as support for apocope or vocal realization of /r/ in final position. Therefore, for example, it cannot support the interpretation of ʔlʕz ("the name" /ʔelʕazar/), in the recently published Givat Hamivtar inscription as /ʔelʕaza/.<sup>29</sup>

As to the second, we would have to conclude that gemination must already have been non-phonemic in this dialect in order to allow such metanalysis.<sup>30</sup>

<sup>26</sup> Irene Garbell, *The Jewish Neo-Aramaic Dialect of Persian Azerbaijan: Linguistic Analysis and Folkloristic Texts* (The Hague: Mouton, 1965), 71. The /m/ in /símun/ is apparently the result of another analogy viz. /qúmun/ analyzed by speakers as /qu/ and /mun/ in spite of the fact that the /m/ is original in the root (Prof. Yona Sabar, oral communication, 1974).

<sup>27</sup> The phenomenon would perhaps be more widespread in the dialect were it not for the fact that the /rl/ combination became /rr/ > /r/ in this dialect (Garbell, *Jewish Neo-Aramaic Dialect*, 70), therefore /mirox/ ("you said"), not /\*milox/. The metanalysis was therefore prevented.

<sup>28</sup> Carl Brockelmann, *Grundriss der vergleichenden Grammatik der semitischen Sprachen*, vol. 1 (Berlin: Reuther & Reichard, 1908), 159.

<sup>29</sup> Eliezer Shimshon Rosenthal, "The Inscription from Givat Hamivtar" [in Hebrew], *Peraqim* 2 (1974): 343n31. I do not wish, of course, to deny the correctness of the interpretation, only to question the support from BJA.

<sup>30</sup> Cf. Syriac /ntl/, certainly from *ntn+l* > *ntl+l*, where when false division takes place the /l/ goes with the root because of the gemination.



## On the History of the Babylonian Jewish Aramaic Reading Traditions: The Reflexes of \*a and \*ā\*

*To the master, Professor H. L. Ginsberg, on his seventy-fifth birthday*

The vowel system of Babylonian Jewish Aramaic (BJA) is only imperfectly known. There are, however, several “reading traditions,”<sup>1</sup> preserved orally or in writing, with varying claims to authenticity. Among these are the Babylonian vocalization of *Targums Onqelos* and *Jonathan* (TOJ),<sup>2</sup> the vocalization of the codex unicus of the Gaonic work *Halakot Pesuqot* (HP),<sup>3</sup> and the oral tradition for reading the Talmud preserved by the Yemenite Jews (Y). Of lesser importance are the

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While this article was in press, I was able to show it to Professor Morag, who has established secure foundations for the reconstruction of Babylonian Jewish Aramaic vocalization through his recognition of the importance of both HP and the Yemenite traditions. He made many valuable suggestions for which I am grateful. I have been able to make some changes in the proofs in response to his suggestions, and there are others I would have made were it possible. Instead, I have inserted some new notes and clarifications, and remarks inspired by his suggestions have been added to my original notes.

First published in *Journal of Near Eastern Studies* 37 (1978): 141–60.

<sup>1</sup> The term is Shelomo Morag’s (“Oral Tradition as a Source of Linguistic Information,” in *Substance and Structure of Language*, ed. Jaan Puhvel [Berkeley: University of California Press, 1969] and “Oral Traditions and Dialects,” in *Proceedings of the International Conference on Semitic Studies, Held in Jerusalem, 19–23 July 1965* [Jerusalem, 1969]). However, I am using it specifically to refer to traditions of the West Semitic type where much of the phonology and morphology is normally unexpressed in writing and must be realized in one of two ways, either by rote memorization of the pronunciation of specific form-tokens and / or types, or by the application of rules. A written vocalization-pointing can be seen then as the expressions in writing of “reading tradition.”

<sup>2</sup> See the introduction to my edition of Daniel Boyarin, ed., *Targum Onkelos to the Pentateuch: A Collection of Fragments in the Library of the Jewish Theological Seminary of America, New York* [in Hebrew], 4 vols. (Jerusalem: Maqor, 1976) and works cited there.

<sup>3</sup> Solomon Sassoon, ed., *Sefer Halakot Pesuqot* [in Hebrew], Reprinted with facsimile, Jerusalem: Maqor, 1971 (Jerusalem: Hevrat Meqitse Nirddamim, 1950).

vocalization of Codex Paris 1402 of *Halakot Gedolot* (HG)<sup>4</sup> and that of various Genizah fragments of Talmud (F).<sup>5</sup> These sources converge with dramatic frequency in the picture they provide of BJA phonology and morphology. However, they also diverge quite significantly in many major details. It follows, therefore, that in order to make use of these sources in reconstructing the phonology and morphology of BJA, some theory of their historical relationships must be assumed. One such theory has already been offered by Shelomo Morag in a series of publications. Perhaps the crucial issue in question is the status and development of pre-BJA \*a and \*ā implied by the sources.<sup>6</sup> This paper will examine afresh the data and analyses presented by Morag and offer an alternative theory.

### 1. Morag's View of the Vocalization Tradition of BJA

1.1 The Yemenite oral tradition serves as the cornerstone of Morag's theory. His view is summed up by the following statement:

Whereas the authenticity of the Yemenite oral tradition of post-Biblical Hebrew may be tested by comparison with vocalized Mss., the same is not true for the Yemenite tradition of the Aramaic of the Babylonian Talmud. Our opinion of the value of the latter must be based on internal evidence only in the absence of vocalized Mss. with which it could have been collated. Having examined the nature of this tradition as it stands, we may say that in many of its features it would appear to constitute a genuine reflection of a Babylonian Aramaic dialect, as learned by Yemenite scholars, assiduous readers of the Talmud, who went to Babylonia for the purpose.<sup>7</sup>

Presumably by "the absence of vocalized Mss.," Morag meant talmudic manuscripts per se, for it was only shortly later that he proposed a comparison between Y and HP and used it to validate the antiquity and stability of the former.<sup>8</sup> However, here a paradox results, for vis-à-vis the \*a-\*ā question, Morag did not abandon the primacy of the Yemenite tradition. Indeed on this matter, the opinion that Y preserves a more ancient tradition than HP emerges from his discussion.

<sup>4</sup> *Sefer Halakot Gedolot* [in Hebrew] (facs., Jerusalem: Maqor, 1971).

<sup>5</sup> Morag has gathered and discussed many of these fragments (see his "On the Vocalization of the Babylonian Talmud in the Gaonic Period" [in Hebrew], in *Proceedings of the Fourth World Congress of Jewish Studies*, vol. 2 (Jerusalem, 1968), 89–94).

<sup>6</sup> In the present paper the following notation will be adopted: \* will be used for proto- and pre-phonemes and forms. Otherwise, italicized letters will be used, unless specific reference is being made to the phonological status of a given phone, in which case / / will be used for phonemic status and [ ] for allophonic. Phones are cited for each tradition in accord with that tradition's phonological structure. Thus for TOJ and Y, we have /â/ and /ä/, while for HP we have /a/, which equals [a]–[â].

<sup>7</sup> Shelomo Morag, "Notes on the Vowel System of Babylonian Aramaic as Preserved in the Yemenite Tradition," *Phonetica* 7, no. 4 (1962): 217–39.

<sup>8</sup> Shelomo Morag, "Towards the Phonology of Babylonian Aramaic" [in Hebrew], *Leshonenu* 32 (1967): 76–77, 83, 86–87.

Such a view is, of course, not excluded but a priori does seem rather less plausible than the position defended below, namely that HP is more faithful to the structure of a living dialect of Gaonic Aramaic, while Y is an archaizing tradition.

1.2 According to Morag, Y preserves a tradition of a dialect in which very limited merger (Morag's "neutralization") of the \*a-\*ā opposition took place. The environments of merger are: (a) penultimate open syllables preceding open syllables (/ \_CV##); and (b) some open syllables preceding *shwa*, with concomitant syncope of *shwa* (\*āCəCV > äCCV).

1.2.1 Morag tenders the following explanation of the mergers:

(a) he assumes a length and quality contrast between /ā/ and /a/. On the basis of comparative evidence, stress is posited for penultimate open syllables, where the final syllable is also open. Because of the stress on these syllables, the feature of length is neutralized, "/a/ also being realized as a long vowel in this position. The /ā/-/a/ contrast has thus turned out to be based upon one phonemic factor only, the qualitative. It seems, however, that this feature alone was not sufficient to retain the contrast in question, all the more so since both phonemes were realized as low vowels of approximately the same highness";<sup>9</sup>

(b) the shift of āCəCV > äCCV is explained by syncope of the *shwa* followed by vowel shortening in the now closed syllable.<sup>10</sup> This shift is only sporadic in Y.

1.2.2 Morag employs analogy to account for other instances in which Y ä replaces historical \*ā. Thus in the sound participle, the shift of \*qātel > qätel, where the conditions of merger are not met, is explained as owing to the analogy of the III Y verb where they are, viz. \*qāte > qäte.<sup>11</sup> In sum, then, in Y, according to Morag we are dealing with a very limited sound shift partially morphologized.

1.3 In HP the environments in which [a] replaces \*ā are wider than in Y. In the environment \*āCəCY, the shift occurs virtually without exception, and Morag accounts for the rest by positing further analogical processes. For example, the feminine plural suffix, \*ān, is *an*. Morag somewhat tentatively explains this again as resulting from analogy to the III Y participle in two stages. First, the masculine plural *qatan* influenced the feminine \*qatyān > *qatyan*. Then the feminine plural suffix in all other forms followed suit.<sup>12</sup>

Morag claims that all other incidences of the two phones in both traditions appear practically identical. The opposition remained phonemic in both tradi-

<sup>9</sup> Morag, "Vowel System of Babylonian Aramaic," 228.

<sup>10</sup> *Ibid.*, 229.

<sup>11</sup> *Ibid.*, 223–24.

<sup>12</sup> Morag, "Phonology of Babylonian Aramaic," 74–75.

tions.<sup>13</sup> Since in Morag's view the main environment in which Y has *â* versus *a* in HP is the feminine plural suffix, it follows that HP represents a dialect which is a later stage of development than Y, precisely by virtue of having carried the \*âCəCV shift to completion and having carried analogy further as well. The Yemenite realization of *qamets* as *â* in Bible and Targum would then be simply a survival. This is the same phon(eme) which existed in the dialect of BJA and reading of Bible and Targum, which they received from the Gaonim.<sup>14</sup> This account has obviously great implications for the description of BJA, as it would tend to establish Y as the frozen record of a living dialect more ancient in visage than HP and therefore presumably closer to the language of the Talmud itself. However, as I shall try to show, Morag's theory is not the only way to account for these data.

## 2. The Rationale for an Alternate View

### 2.1 The Conditioning of the Neutralization

Morag's explanation for the shift of *â* > *a* in penultimate open syllables is not satisfying for the following reasons:

(a) it assumes a double contrast between /*â*/ and /*a*/; both length and quality are distinctive.<sup>15</sup> This is itself uneconomical, unless supported by the system as a whole, while in BJA there are no other length contrasts.<sup>16</sup> However, here, the sole purpose of this assumption is to motivate the shift, and there is no other evidence for length in this vowel at this time;<sup>17</sup>

(b) it involves the assumption of a stress placement rule, again solely to motivate the hypothesized development;<sup>18</sup>

<sup>13</sup> Morag, "Vowel System of Babylonian Aramaic," 221.

<sup>14</sup> See Shelomo Morag, *The Hebrew Language Tradition of the Yemenite Jews* [in Hebrew] (Jerusalem: The Academy of the Hebrew Language, 1963), 100–106; and cf. Shelomo Morag, "Babylonian Aramaic in the Tradition of Yemen: The Sound Verb" [in Hebrew], in *Sefer Hanokh Yalon*, ed. Saul Lieberman et al. (Jerusalem: Qiryat Sefer, 1963), 182–221, esp. 185n75.

<sup>15</sup> Morag, "Vowel System of Babylonian Aramaic," 228.

<sup>16</sup> To be sure, such a system is often assumed for Eastern Syriac as well. Birkeland himself characterizes it as "very curious" (see Harris Birkeland, "The Syriac Phonematic Vowel Systems," in *Festschrift til professor Olaf Broch på hans 80-årsdag*, ed. Christian Schweigaard Stang, Erik Krag, and Arne Gallis [Oslo: Dybwad, 1947], 13–39, esp. 14). It is not at all clear from his discussion what necessity there is for assuming a length contrast between his /*a*/ and /*â*/, since he avers that "no doubt *a:â* and *e:ë* express *qualitative* differences as well." Surely the fact that the Syrians call the supposed *short* member of the second pair *long* and the *long* member *short* hardly supports a length contrast theory. Confusions and variations between the members of the pairs also does not prove that the opposition was quantitative (see further sec. 3.3.2 below).

<sup>17</sup> Morag, "Vowel System of Babylonian Aramaic," 237n59.

<sup>18</sup> With his usual scrupulousness, Morag (*ibid.*, 237n57) duly notes that there is no support from Y for his posited stress rule. He does try to find evidence for it in Western Syriac. Whether

(c) it posits that given stress lengthening of a short vowel in a certain position and its consequent merger with a long vowel, the product of the merger was paradoxically interpreted as belonging to the short vowel phoneme.<sup>19</sup>

Moreover, if the durational increment which triggered the vowel shift were caused by stress, one would expect that other stressed syllables would likewise undergo the shift. Since there is no evidence for a shift of other stressed *ā*'s, nor for a distinction between the stress-type evident here and the stress-type evident for nonshifting *ā*'s, we will have to look elsewhere for the motivating factor or factors.

## 2.2 The Use of Analogy

As seen above (secs. 1.2.2 and 1.3), by positing a very limited phonetic shift, Morag is forced to assume rather complicated analogical processes to explain other *\*ā > a* transfers. These analogies do not appear to have a clear rationale, such as regularizing paradigms or the like, and therefore an explanation which obviates the need for them would seem a priori preferable.<sup>20</sup>

## 2.3 Unexplained Forms

In the following forms in HP the shift *\*ā* to *a* has to be explained on an ad hoc basis if Morag's reconstruction be accepted:

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or not this stress rule can be posited for proto-Aramaic is questionable (but cf. Birkeland, "Syriac Phonematic Vowel Systems," 17), and diffusion seems out of the question because of the lack of geographical continuity between the areas of BJA and Western Syriac. Incidentally, proto-Eastern Aramaic will not do, since as I have tried to show in a forthcoming paper ("Is Syriac Eastern Aramaic," delivered at the A. O. S., March 1976, Philadelphia), there simply was no such entity.

<sup>19</sup> Morag seems to anticipate this difficulty by his complex account of "neutralization" of length rather than lengthening, which seems to point to a resulting phonological entity belonging neither to *a* nor *ā* but to both, an archiphoneme, *A* (see Nikolai Sergejewitsch Trubetzkoy, *Grundzüge der Phonologie*, 4th ed. [Göttingen: Vandenhoeck & Ruprecht, 1967], 71–75). This, at least, could possibly explain the choice of the sign for the less marked vowel to indicate the suspended opposition (see *ibid.*, 73, par. b). Since, however, many of these *A*'s alternate morphophonemically with *ā* and not *a*, I would expect the *ā* graph to have been chosen (cf., for example, German *Land, Bund*, not *Lant, Bunt*, of course). In short, I believe even this interpretation of Morag's theory raises as many problems as it solves.

<sup>20</sup> See, e. g., Philip Baldi, "The Latin Imperfect in *\*bā-*," *Language* 52 (1976): 839–51, esp. 845: "It seems to me methodologically reasonable that we should invoke analogy only when all structural possibilities have been exhausted. [...] But it must be conceded that sound change, while less than perfectly regular, at least displays consistent tendencies; and we are always on firmer ground with phonetic explanations than with analogical ones." See also John J. Ohala, "Phonetic Explanation in Phonology," in *Papers from the Parasession on Natural Phonology, April 18, 1974*, ed. Anthony Bruck, Robert Allen Fox, and Michael W. La Galy (Chicago: Chicago Linguistic Society, 1974), 251–75, esp. 268.

- (a) *kəwəθeh*, “according to him” < \**kəwāθeh* (pp. 5, 231)  
 (b) *həwa* = *leh*, “he became” < \**həwā leh* (pp. 23, 206)<sup>21</sup>  
 (c) *ləβaθar*, “after” < \**ləβāθar* (p. 40)<sup>22</sup>  
 (d) *nəšarif*, “he will join” < \**nəšārif* (p. 209)<sup>23</sup>  
 (e) *bənaḥ*, “your sons” < \**bənāḥ* (p. 219)<sup>24</sup>  
 (f) *ləmiqam*, “to stand” < \**ləmiqām* (p. 214)<sup>25</sup>

For each of these forms an individual explanation can be found to solve the problem (see notes). These individual explanations represent at least a complication of the theory. Taken together with the two preceding arguments, especially the first, they lead us to search for a different explanation.

### 3. The Present Hypothesis

3.1 The present theory posits three stages in the development of Babylonian Jewish Aramaic vocalism: an archaic one, more or less represented by the Babylonian pointing of the Targums; a later stage of the spoken language, represented by the pointing of HP; and an archaizing tradition, represented by other Gaonic fragments, HG, and perhaps best by Y, arising after the death of the spoken language in Babylonia and in Yemen. This tradition is characterized by systematic (non-random) interference between the TOJ system and the HP system.<sup>25a</sup>

<sup>21</sup> For (a) and (b) one could claim that the /h/ had been lost at the end rendering the ultimate syllable open, thus meeting the conditions of the shift. However, HP seems to retain final /h/, as attested by such forms as *tānah* “he teaches it,” not *tānā*. Furthermore, one would have to maintain that both the stress rule and vowel shift rule were synchronically active after the loss of /h/ for this explanation to work. This solution is, of course, not impossible, but certainly uneconomical.

<sup>22</sup> Forms with short *a* in *baθar* are attested as Sura’*e* variantes in the Masorah to TO (see Samuel Landauer, *Die Masorah zum Onkelos auf Grund neuer Quellen lexikalisch geordnet & kritisch beleuchtet* [Amsterdam, 1896], 19). As far as I can tell, however, these are only forms with suffixes in which the shift is predicted by Morag, e. g., \**bāθərohi* > *baθrohi*. Perhaps Morag would explain *baθar* as back formation from such suffixed forms.

<sup>23</sup> For (d) one could easily assume that the vowel was never long and that /r/ was doubled in the language. This solution, however, creates problems for another part of Morag’s hypothesis, since he claims phonemic status for the /ā/-/ā/ opposition in Y on the basis of a contract between “*pāreš*, ‘is separated’ [...] (act. part. of the simple stem [...]) – *pāreš*, ‘expounds’ [...] (act. part. of the *qattel* stem [...])” (Morag, “Vowel System of Babylonian Aramaic,” 221). If we assume the latter was \**parreš* historically, then in any case *pāreš* is a secondary development influenced by Targumic and/or Biblical Aramaic, and we are back where we started from.

<sup>24</sup> Resulting from contraction of \**bənaix*, the vowel is \**ā* in both the *Qre* of Biblical Aramaic and in Targum. One could claim, however, that in some dialects an exceptional short vowel resulted (cf. Syriac *ʿax* from \**ʿaix*, “how”).

<sup>25</sup> Perhaps to be explained as owing to analogy of the Med. Gem. verbs. Syncretism of these two classes was a prevalent phenomenon in the language.

<sup>25a</sup> I should like to emphasize that the difference between my view and that of Prof. Morag is one of detail. In the main, my paper is based on and accepts his description and evaluation.

3.2 On the basis of this hypothesis, a model of the history of \*a and \*ā may be constructed. At the earliest stage, when the vocalization signs of TOJ were invented, \*a and \*ā were phonemically distinct, with the opposition transmuted from a quantitative to a qualitative one. At the stage of HP, these two phonemes had unconditionally merged, but this new phoneme /a/ had an allophone [ɔ] (signified by *miqpatš puma*, the Babylonian *qamets*, in HP). The reading of Bible and Targum, however, maintained a phonemic opposition between these phones, at least among the learned elite. In the final stage (Y, HG, F), the HP tradition had been partially reformed after TOJ according to a coherent set of principles, and the opposition between /a/ and /ā/ or /ɔ/ was rephonologized. What evidence is there for such a model?

3.2.1 Two types of evidence may be offered in support of the claim that TOJ represents an archaic form of Babylonian Aramaic vocalization. These are: (1) isoglosses which may be established between details of TOJ vocalization and known Babylonian features, and (2) the *patakh-segol* merger in the Babylonian vocalization system, which almost surely must be explained by the interference of the then current structure of Babylonian Aramaic vocalism.

3.2.2 The Babylonian provenience of the TOJ vocalization is by now accepted in all quarters, although many think the consonantal text to be Palestinian.<sup>25b</sup> It is therefore perhaps somewhat surprising that virtually no attempt has been made to identify various features of that vocalization with known features of Babylonian Aramaic.

In two recent papers, I have pointed out some connecting links between TOJ and BJA.<sup>26</sup> These include:

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However, he believes the HP and Y are parallel sub-dialects. Although he has never expressed or implied a judgement that Y is more ancient in origin than HP, this view does seem to follow from his explanations. If certain aspects of the linguistic patterning are explained as resulting from further development of diachronic processes occurring in Y, Y is in these respects at least an earlier *état de langue*. I do not wish to claim that Y is substantially different from HP and therefore unreliable. Quite the contrary, as Prof. Morag has shown, Y is nearly identical with HP. I believe, however, that they are not sub-dialects but rather the same dialect. I have tried to explain the differences between the two as owing to systematic interference between the Yemenite reading traditions of Aramaic (or their Babylonian ancestors). This interference was originally pointed out by Prof. Morag, and I have really only extended its scope. In my view, it does not nearly approximate the extent of such interference in HG and F, but it is present and significant (see below sec. 3.5). Y is invaluable for reconstructing the grammar of BJA and indispensable in those matters for which HP provides no attestation. My feeling is that in areas where they diverge, HP should be preferred over Y as representing something closer to living speech.

<sup>25b</sup> See M. Goshen-Gottstein, "The Language of Targum Onqelos and the Model of Literary Diglossia in Aramaic," *Journal of Near Eastern Studies* 37, no. 2, The Language of Targum Onqelos (1978): 169–79.

<sup>26</sup> See the introduction to my edition of *Targum Onkelos*, 3–4 and my article "Studies in Babylonian Aramaic" [in Hebrew], *Leshonenu* 40 (1976): 172–77, esp. 175–76. Lest there be

- (a) the forms *qatalaθ-qataliθ* for the third person feminine singular and first person preterite of the verb, vs. *qa|itlaθ-qa|itliθ* in all other dialects;
- (b) the form /-āx/ for the second person suffix on plural nouns, otherwise only attested in the *Qre* of Tiberian Biblical Aramaic;<sup>27</sup>
- (c) the form /qātan/ for the masculine plural participle of III Y verbs;
- (d) the vocalization of the conjunction /w-/ with /i/ before consonants followed by *shwa*, e.g., /wiβnān/, “and daughters”;
- (e) epenthesis of /i/ after /ʿ/, e.g., /yiʿirūq/, “he will run away”;<sup>28</sup>
- (f) frequent replacement of CVCCəCV by CVCiCCV, as in e.g., /maḏinḥaʿe/, “Easterners” < /maḏnəḥaʿe/. This epenthesis is well attested in HP by both the writing of *y* and the vocalization; cf. e.g., *ləmiqiryeh*, “to read it” (HP, p. 184);
- (g) avoidance of reduced vowels after laryngeals and pharyngeals, e.g., /ʾināš/, “man” not \*ʾənāš/ or \*ʾənāš/.

It must be admitted that none of the above prove Babylonian origin for the vocalization tradition, as they could all be secondary impositions of Babylonian forms on a basically Palestinian text, but such a wealth of isoglosses shows, at the very least, a very heavy influence of Babylonian speech on the oldest attested TOJ vocalization.

3.2.3 It is well known that the Babylonian sign *miftah puma* covers the territory of Tiberian *patakh* and at least some of Tiberian *segol*.<sup>29</sup> This by itself does not prove that the Babylonian sign indicates merger of two vowels since in many lexical items Tiberian *segol* is derived from a historical short *a* through various phonetic processes. However, there is at least one class of forms in which Babylonian *miftah puma* is equivalent to a Tiberian *segol*, not from an earlier short \**a*, namely verbs and nouns from III Y roots which earlier had a diphthong /-ay/. An example of this would be ירעה in Tiberian vocalization < \*ʾyirʿay/, with ירעה in Babylonian.<sup>30</sup> There is, therefore, little doubt that Babylonian Hebrew represents in this regard a later stage than Tiberian and that there was in Babylonian Hebrew

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any misunderstanding, I wish to make it clear that I have never claimed that TOJ may be used as direct evidence in the sense that Y and HP are. However, Prof. Morag suggests that the possibility of a Palestinian substratum, as it were, has not been taken seriously enough by me. The point is well taken, and I would say now that TOJ should only be used to corroborate other sources of evidence.

<sup>27</sup> The *Qre* of TBA shows other Eastern features as well; note for instance the shift *āye* > *āʿe* in the gentile. Of course, I am not claiming to have discovered all of these correspondences, merely their systematic importance. See cited papers for reference to previous work.

<sup>28</sup> Since (d) and (e) occur in Babylonian Hebrew also, one could see that tradition as their source in TOJ and BJA as well. This has been pointed out to me by Professor Sokoloff. It seems much more likely, however, that these were features of native Babylonian Aramaic speech which affected the Hebrew vocalization tradition rather than the converse.

<sup>29</sup> Gotthelf Bergstrasser, *Hebräische Grammatik* [in Hebrew], trans. Mordechai Ben-Asher (Jerusalem: Magnes Press, 1972), 88.

<sup>30</sup> Israel Yeivin, “The Babylonian Vocalization and the Linguistic Tradition It Reflects” [in Hebrew] (PhD diss., The Hebrew University, 1968), 128.

a merger of /ε/ and /a/. The most probable explanation for the loss of a phoneme of Hebrew in Babylonia is the lack of that phoneme in Babylonian Aramaic. It does not seem at all likely that the originators and transmitters of the Babylonian Hebrew tradition would have maintained one Palestinian phonemic distinction *not current in their speech* while losing another. It follows, therefore, that the system of vocalization signs – invented for Hebrew and adopted for Targumic Aramaic – most probably represents a Hebrew whose vocalic system had been adapted to the vocalic system of Babylonian Aramaic at the time of its invention. We may conclude with some degree of confidence, therefore, that the *miqpatš puma* (= Tiberian *qamets-â*) and *miftah puma* (= Tiberian *patakh-ä*) represented distinct phonemes of Babylonian Aramaic at the time of their invention. Since in Targumic Aramaic the two signs are placed quite where we should expect them on historical grounds, we assume no significant conditioned mergers either. The opposition was no longer quantitative but qualitative as will be shown now.

### 3.3 The Phonetic Value of *miqpatš puma*

3.3.1 The question at hand in this part of our discussion is one that has been treated frequently in the literature. Some scholars have maintained that the opposition of *miqpatš puma* and *miftah puma* was essentially quantitative.<sup>31</sup> Others have maintained that the opposition was indeed qualitative, but both were front vowels of different height, *miqpatš puma*, a low vowel and *miftah puma*, slightly higher, perhaps /a/ and /æ/.<sup>32</sup> Morag has maintained, following Klar, that in fact the *miqpatš puma* was a back vowel and the *miftah puma* a front vowel, /â/ and /ä/. In my opinion, all the evidence points to the last view. Accordingly, I shall review here the evidence which has been offered so far and present some more supporting evidence.

3.3.2 Our first task is to examine the evidence for the contrary hypothesis, that *miqpatš puma* was a front vowel, long or length-indifferent. Kahle presented the following arguments in favor of this view:<sup>33</sup>

- (a) the pronunciation of the Spanish Jews and others Jews who read *qamets* as /a/ must have been derived from Babylonian models;
- (b) the Babylonian *naqdanim* (vocalizers) did not use the *miqpatš puma* to represent short /u/ as did the Tiberians; and
- (c) the Eastern Syrians read the sign paralleling *qamets* and *miqpatš puma* as a front vowel.

<sup>31</sup> Paul Kahle, *The Cairo Geniza*, 2nd ed. (Oxford: Blackwell, 1959), 71–72.

<sup>32</sup> Hanokh Yalon, “The *Qamets* in Babylonian Vocalization and in Yemen” [in Hebrew], in *Pirqê Lashon* (Jerusalem: Mosad Bialik, 1971), 262–80, esp. 262–68.

<sup>33</sup> Kahle, *Cairo Geniza*, 71–72.

Yalon already discredited the first argument very simply.<sup>34</sup> In the first place, it is by no means a necessary assumption that the “Spanish” pronunciation has Babylonian origins. We possess a Palestinian vocalization tradition which merges the reflexes of \*a and \*ā. Moreover, the structure of the Palestinian vocalization, with its merger of *segol* /ε/ and *šere* /e/, is much closer to the “Spanish” pronunciation than is the Babylonian. As a matter of fact, the “Spanish” reading cannot possibly be derived from the Babylonian since in the latter *patakh* and *segol* are merged and not in the former. We may sum up these relationships by the following statement:

The Tiberian seven vowel system (fig. 1) becomes five vowels in the Palestinian system by merging /a/ and /ɔ/ on the one hand and /e/ and /ε/ on the other (fig. 2). This, of course, fits the Spanish tradition exactly. However, in Babylonia, /a/ and /ε/ are merged, yielding a six vowel system quite different from the Spanish pronunciation (fig. 3).<sup>35</sup>

*The Tiberian Vowel System*

|       |       |       |
|-------|-------|-------|
| i (1) |       | u (7) |
| e (2) |       | o (6) |
| ε (3) | ɔ (5) |       |
| a (4) |       |       |

FIG. 1

*The Palestinian and Spanish Hebrew Vowel System*

|          |       |       |
|----------|-------|-------|
| i (1)    |       | u (7) |
| e (2, 3) | o (6) |       |
| a (4, 5) |       |       |

FIG. 2

*The Babylonian Hebrew Vowel System*

|          |       |       |
|----------|-------|-------|
| i (1)    |       | u (7) |
| e (2)    |       | o (6) |
| ä (3, 4) | å (5) |       |

FIG. 3

Morag has refuted Kahle’s second argument.<sup>36</sup> Whether the *miqpatš puma* was a back or front vowel, the Babylonians did not use it for a lowered short /u/ because

<sup>34</sup> Hanokh Yalon, “Review of *Masoreten des Westens*” [in Hebrew], in *Pirke Lashon* (Jerusalem: Mosad Bialik, 1971), 384–86.

<sup>35</sup> Cf. Morag, *Hebrew Language Tradition*, 103.

<sup>36</sup> *Ibid.*, 102.

short /u/ was not lowered in their pronunciation. There is, therefore no evidence whatsoever from this nonuse of the sign.<sup>37</sup>

As for the third argument that in Mesopotamia, in general, historical long \*ā was a front vowel, there is evidence that at an earlier period in East Syriac as well \*ā had become â. Thus before /w/, the East Syrians use the sign corresponding to long \*ā in morphemes where etymologically and in West Syriac short \*a obtains.<sup>38</sup> This phenomenon is almost certainly to be explained as assimilatory labialization,<sup>39</sup> and the resulting vowel must have been a back round vowel at the time, proving that the sign represented such a sound then. Furthermore, Greek words with *o* and *w* are written with this sign as well.<sup>40</sup> All this suggests very strongly that at an earlier period this phoneme was a back round vowel â which later merged with *a*, precisely the development we are suggesting for Babylonian Aramaic.<sup>41</sup> After all, it seems much more likely that the pronunciation of Θεόδωρος as *teodarus* is an inner Syriac development than that they ignored their /o/ vowel and chose a front /a/ to transcribe Greek *w*. In fine, Kahle's arguments may be said to contribute nothing to the view that Babylonian *miqats puma* was a front vowel or central vowel and may even favor the opposite view.

Yalon also claims to have proven "that the pronunciation of the Babylonian *qamets* equals Tiberian *patakh*."<sup>42</sup> His major proof seems to be the use of *alef* as a vowel letter for *qamets* and a sign derived from *alef* as its diacritic vowel sign.<sup>43</sup> Let us examine this argument. *Alef* has a history as a vowel letter for \*ā in Aramaic,<sup>44</sup> and a mere phonetic shift in the pronunciation of this vowel would have affected only the phonetic value of the graph, much as the English vowel signs changed their phonetic values with the great English vowel shift. With some redundancy, Yalon also argues that the same *alef* spelling proves that in

<sup>37</sup> Ibid., 103.

<sup>38</sup> Theodor Nöldeke, *Compendious Syriac Grammar*, trans. James A. Crichton, Reprinted edition, Jerusalem: 1970 (London: Williams & Norgate, 1904), 35.

<sup>39</sup> Cf. Morag, "Vocalization of the Babylonian Talmud," 85n75.

<sup>40</sup> Nöldeke, *Syriac Grammar*, 34.

<sup>41</sup> This view seems to be consistent with that of Nöldeke who states, "No one of these systems (i. e., Nestorian or Jacobite) carries out a distinction between long and short vowels [...]; in neither case is the quantity of the vowel considered, but merely the quality" (ibid., 9). Even Birkeland, who argues for a length distinction on the grounds that the Nestorians use terms for "short" and "long" (see Birkeland, "Syriac Phonematic Vowel Systems," 18–19), reversing, however, their historically correct application, which to me, as it did to Nöldeke (*Syriac Grammar*, 9) suggests strongly the opposite (i. e., that length was totally irrelevant) concludes, "no doubt a:ā and e:ē express *qualitative* differences too. And as this kind of difference is the only one that exists between the rest of the vowels, it must also be the one that is of main relevance as regards *a* and *e*" (Birkeland, "Syriac Phonematic Vowel Systems," 14; see also Morag, *Hebrew Language Tradition*, 105n2).

<sup>42</sup> Yalon, "Qamets," 278.

<sup>43</sup> Ibid., 265–66.

<sup>44</sup> Frank Moore Cross Jr. and David Noel Freedman, *Early Hebrew Orthography: A Study of the Epigraphic Evidence*, vol. 36 (New Haven, CT: American Oriental Society, 1952), 33–34.

Babylonian Aramaic the reflex of \*ā was a low front vowel, and since Qirqisani said that the Babylonians' reading of Hebrew was similar or close to Babylonian Aramaic, it follows that a similar pronunciation obtained in the former. This argument is weak for two reasons. First, the pronunciation of Babylonian Aramaic is itself the issue, and *alef* spellings prove nothing. Secondly, when Qirqisani says "similar" and "close," it does not by any means imply identical (see sec. 3.3.3 below). Finally, Yalon argues from the interchanges between [ä] and [â] in the Yemenite reading of BJA, an argument already rejected by Morag<sup>45</sup> and further refuted below.

A stronger argument is that of Kutscher,<sup>46</sup> who contends that in Yemen one outlying district, Ḥabbān, can be shown to have maintained a purer Babylonian reading of Hebrew. Since that district reads *qamets* as *a* as against all other Yemenites who have *â*, Kutscher argues that the *â* must be the innovation under Tiberian influence. While the argument seems sound on dialect geography grounds, it is not by any means watertight and is far outweighed by evidence in the other direction.

To sum up the results of this section, it seems that it may be said that only one significant piece of evidence has been offered to support the claim of a low front realization of *miqpatš puma*.<sup>46a</sup>

3.3.3 In this section I shall endeavor to show that there is strong evidence for a back round realization of *miqpatš puma*. The evidence falls into three categories: (1) the testimony of Qirqisani, (2) naive spellings of *w* (*vav*) for \*ā and *qamets* for *o*, and (3) use of the *miqpatš puma* sign to indicate labialization of *a*.

(1) Benjamin Klar seems to have been the first to recognize the significance of the passage from Qirqisani in this regard, but his interpretation requires modification in certain details.<sup>47</sup> In order to justify my interpretation, fairly extensive quotation is required:

What may be said in this matter (viz. the change in Babylonia from Tiberian pronunciation) is this: that the people, when they had long sojourned in Iraq, their reading became Nabateanized,<sup>48</sup> since our eyes see that the reading of the Babylonians is close

<sup>45</sup> Morag, *Hebrew Language Tradition*, 103–4.

<sup>46</sup> Edward Yechezkel Kutscher, "Yemenite Hebrew and Ancient Pronunciation," *Journal of Semitic Studies* 11, no. 2 (1966): 217–25.

<sup>46a</sup> Prof. Morag informs me that in fact in the Ḥabbāni pronunciation *qamets* was a low back vowel (differing from the other Yemenite traditions only in that it was not rounded) and always distinct from *patakh*. It does not therefore truly constitute counter-evidence to Morag's (and my) view that Babylonian Aramaic had originally a low back vowel for *qamets*, i. e., as the reflex of \*ā. As Prof. Morag points out, the unrounding of this vowel would have been a likely step in the direction of the merger for which I have argued in any case.

<sup>47</sup> Benjamin Klar, "Matters of Massorah and Pronunciation in Qirqisani" [in Hebrew], in *Mehqarim Ve'iyunim* (Tel-Aviv: Mahbarot Lesifrut, 1954), 320–28.

<sup>48</sup> I. e., closer to Babylonian Aramaic. The use of the term Nabatean for Iraqi Aramaic was very common in this period (see *ibid.*, 326n34).

to the Nabatean language. Similarly we find the reading of the people of every place becoming close to the language in which they have been brought up, e. g., the people of Hijaz and Yemen do not maintain *veth*, pronouncing in its place *beth*, and the reason is that they grew up among Arabs and became accustomed to their language, since there is no *veth* in Arabic. [...] *And similarly also the Byzantines*<sup>49</sup> *have come to the point where they do not retain qameš because it is not in the Byzantine language. [...] And many of the Jews of Iraq who grew up among Nabateans substitute qadeš for qadoš. [...] And there does not remain in this generation even one philologist or grammarian of the men of Işfahan, Başrah, Tustar or anywhere else, who do not prefer the Palestinian reading, and who do not recognize that it is the true one, and who do not see that the true nature of grammar only becomes clear in accordance with it. Indeed a group of their elders, who do not read Palestinian, and who read Babylonian, and have heard about Palestinian only through rumour, when they wish to speak of matters concerning language and grammar, they speak only of the Palestinian language and none other* (italics mine).<sup>50</sup>

The implications of the first italicized passage seem clear enough. Qirqisani's main objective is to prove the secondary and corrupt nature of the Babylonian reading. Although Qirqisani quotes mispronunciation of *qamets* in Byzantine as an example of corruption and moreover gives a different example of corruption of Hebrew in Babylonia, he does not adduce mispronunciation of *qamets* in Babylonia. One may surely deduce that in Qirqisani's opinion the Babylonians had at least not obliterated the correct realization of *qamets*, to wit Tiberian *â*. So, indeed, deduced Klar.<sup>51</sup>

The second italicized passage caused the difficulty with Klar's interpretation. Klar apparently thought that the "preference" mentioned in the preceding sentence meant that they actually adopted the Palestinian reading. He therefore concluded that there was an "academic pronunciation of Hebrew" in Babylonia similar to that of Europe.<sup>52</sup> Consequently, he added in parentheses the word "naturally" after the words "who do not read Palestinian," taking the passage to mean that these sages *do* read Tiberian, albeit as a sort of affectation. Yalon therefore

<sup>49</sup> Lit., "Romans," i. e., Jews of Byzantine and perhaps Christian Europe (see *ibid.*, 325n31).

<sup>50</sup> Ya'qūb ibn Işhāq Qirqisānī, *Kitāb al-anwār wal-marāqib: Code of Karaite Law*, ed. Leon Nemoy, vol. 2 (New York: Alexander Kohut Memorial Foundation, 1940), 140: "wa-ka-dālika 'ayḏan šāra r-rūmu lā yuqimūna l-qāmišata li-annah laysat fi luḡati r-rūmiyyi [...] wa-kaṭīrun min yahūdi l-'irāqi l-laḏi naša'ū bayna n-nabaṭi yaj'alūna maqāma qādoš qādeš [...] wa-lam yabqa fi hāda l-'ašri man yata'atā 'ilma l-luḡati wa-d-diqduq mina l-'iṣfahāniyyina wa-l-bašriyyina wa-t-tustariyyina wa-ḡayrihim 'illā wa-huwa yufaḏḏilu qirā'ata š-šāmiyyi wa-yuṣaḥḥihūhā wa-yarā 'anna d-diqduq lā yatašarraxu ḥaḡiqatuhā 'illā 'alayhā na'am ḥattā jamā'atin min mašā'ixihimi l-laḏi lam yaqra'ū šāmiyyan wa-'innamā yaqra'ū 'irāqiyyan wa-'innamā sami'ū bi-š-šāmiyyi 'alā [jihati] l-xabari 'idā 'arādu yatakallamū 'alā l-luḡati wa-d-diqduq lam yatakallamū 'illā 'alā luḡati š-šāmiyyati dūna ḡayrihā."

Professor Richard Steiner collated my translation of Klar's Hebrew with the original Arabic and made many important corrections. I checked some points with Professor Moshe Zucker also.

<sup>51</sup> Klar, "Matters of Massorah," 327n37.

<sup>52</sup> *Ibid.*, 328n44.

discounted the significance of the first passage, claiming that Qirqisani absolves the Babylonians of guilt in regard to the *qamets* by virtue of their sages.<sup>53</sup> The passage, however, does not require the addition of the word “naturally” because “preference” here does not imply adoption. These philologists, grammarians, and sages did not read Tiberian at all, as Qirqisani says clearly when not parenthetically embellished. The first group paid mere lip service to the superiority of the Tiberian reading, while the latter carried on their grammatical discussions following its rules. None of these groups had an “academic pronunciation,” and therefore if Qirqisani does not take the Babylonians to task for mispronouncing *qamets*, we may safely conclude that according to his knowledge, *â* existed there as in Tiberias.<sup>54</sup>

(2) The second type of evidence in this matter consists of vulgar texts from Babylonia in which *w* (*vav*) is “miswritten” in place of historical \**â* and in some of which the *qamets* sign is used for \**o*. There are three main sources of this type of spelling: a Hebrew incantation found in the Cairo Genizah with some vocalization, a schoolboy’s copy of some Hebrew prayers with vocalization (also from the Genizah), and mixed Aramaic and Hebrew incantations written on excavated bowls. The former two, although vocalized with Tiberian signs, are proven Babylonian by *segol-patakh* inversions.<sup>55</sup>

In the Hebrew incantation published by Gottheil and Worrell,<sup>56</sup> *w* is used quite extensively for \**â* and *qamets* for *o*. There can be little doubt that in the Hebrew pronunciation of the scribe \**â* and \**o* had merged, surely arguing for an earlier [â] or the like. Yalon tries to discount this argument claiming that “this text is no support for a Babylonian *qamets* equaling *ḥolam*; quite the contrary, the abundance of inversions shows the opposite.”<sup>57</sup> If I understand Yalon’s cryptic statement correctly, he wishes to say that since for this scribe \**â* and \**o* were merged completely, and since there is no other evidence of such a general merger in Babylonia, we are dealing with a special case, a local dialectal or even idiolectal phenomenon. This same problem will be accounted for quite differently below (see sec. 3.4.3). Identical considerations will apply to the fragment of Hebrew liturgy published by Sharvit, in which the same inversions certainly prove that for its writer as well *qamets* and *holem* were one.

The other type of evidence of this sort, namely the Aramaic-Hebrew incantation bowls is somewhat more difficult to interpret. There are no vocalization signs

<sup>53</sup> Yalon, “*Qamets*,” 277.

<sup>54</sup> I thank Professor Steiner for this insight.

<sup>55</sup> Yalon, “*Qamets*,” 277 and Shimon Sharvit, “Lack of Contrast Between *Qamets* and *Holem*, and *Segol* and *Patakh* in a MS Whose Pronunciation Is Babylonian” [in Hebrew], in *Sefer Hanokh Yalon*, ed. Edward Yechezkel Kutscher, Saul Lieberman, and Menachem Zevi Kaddari (Ramat Gan: Qiryat Sefer, 1974), 553–54.

<sup>56</sup> Richard Gottheil and William H. Worell, *Fragments from the Cairo Genizah in the Freer Collection* (New York: Macmillan, 1927), 76–81.

<sup>57</sup> Yalon, “*Qamets*,” 277.

in these texts and therefore no inverse spellings of *qamets* for *o* to prove merger.<sup>58</sup> There is, however, extensive use of *w* where historically \*ā obtained. These spellings show that at least some reflexes of \*ā had merged with *o* or were phonetically close to that vowel.<sup>59</sup> Yalon has also tried to dismiss the evidence of these texts on the grounds that the phenomenon occurs in only a minority of them.<sup>60</sup> Lack of an inverted or naive spelling in a text, however, is never evidence of nonmerger of phonemes. Since there is no counter-evidence, as shown above, there seems to be no reason to posit special dialectal status for these and the above-discussed texts. They are special only in that their writers were clearly from among the less tutored members of Babylonian Jewry.

(3) We turn finally to the use of *qamets* to indicate labialization of an original *a* before *w*.<sup>61</sup> Morag already argued from such use of *miqpatš puma* before *w* in HP that it must have been then a back round vowel. Morag's argument may be supported by the Babylonian biblical vocalization where the same is true. Thus \*mawēθ and \*ʾawēn are *māwēθ* and *ʾāwēn* in Babylonian Hebrew just as they are in Tiberian.<sup>62</sup> We conclude that the Babylonian Masoretes were aware of the rule

<sup>58</sup> See my forthcoming article in *Israel Oriental Studies*, "Variable Rules in Philology," for the special status of inverse spellings to prove phonemic merger [editor's note: "Variable Rules in Philology" (paper presented at the Summer Meeting of the Linguistic Society, Oswego, NY, 1976) was published with some revisions as "The Low Vowel System of Gaonic Aramaic," *Israel Oriental Studies* 8 (1978): 129–41. References to that paper were replaced here by references to the published article].

<sup>59</sup> It appears now that under various sociolinguistic conditions even unsophisticated speakers are consciously aware of allophonic differences and phonetic values and, therefore, that spelling can sometimes show synchronic and diachronic phonetic changes. See my article referred to in note 58 above and references cited in that work.

<sup>60</sup> Specifically, Yalon claims as to the corpus of texts which he examined (William H. Rossell, *A Handbook of Aramaic Magical Texts* [Ringwood Borough, NJ: Shelton College, 1953]) "in four of them there are an abundance of examples for our matter [...] the exceptional minority, four out of thirty-two, prove the rule about the majority" (Yalon, "Qamets," 276). This statement raises several problems, aside from the fact that it is based on a particularly weak argument from silence. First, the corpus examined by Yalon is not exhaustive by any means, so statistically based arguments are suspect. Secondly, these spellings appear in somewhat more than four of those texts; ten would be a more appropriate estimate (give or take one or two because of problems of reading and/or interpretation). It is just that in these additional texts there are not an "abundance" of examples. There seems therefore little reason to regard these as some sort of exceptional phenomenon.

The corpus of bowls examined for this paper include all Jewish bowls published in the twentieth century which were known to me. All have been collated with the original photographs and/or hand copies, with the exception of five bowls from I. Jeruzalmi's book (Isak Jeruzalmi, "Les coupes magiques araméennes de Mésopotamie" [PhD diss., Université de Paris, 1963]) for which I have relied on copies in Charles D. Isbell, *Corpus of the Aramaic Incantation Bowls* (Missoula, Montana: Society of Biblical Literature, 1975), since I could not obtain Jeruzalmi's work.

<sup>61</sup> Morag, "Vocalization of the Babylonian Talmud," 85n75.

<sup>62</sup> Israel Yeivin, *The Tradition of the Hebrew Language Reflected in the Babylonian Vocalization* [in Hebrew] (Jerusalem: The Academy of the Hebrew Language, 1973), 209.

of labialization and chose to show its effect by using *miqpatš puma*, which must have marked therefore a labial (i. e., back round) vowel in their reading.<sup>63</sup>

3.3.4 I believe, therefore, that it is with some confidence that we can establish the value of *miftah puma* as a low front vowel and *miqpatš puma* as a low back round vowel, in accordance with the views of Klar and Morag.<sup>64</sup> Since we have argued that this vocalization manifests the vocalic system of BJA at a relatively early period, we conclude that BJA had at an early date a six vowel system (fig. 4). This system did not remain stable however.

*The Archaic Babylonian Aramaic Vowel System*

|       |       |
|-------|-------|
| i (1) | u (6) |
| e (2) | o (5) |
| ä (3) | â (4) |

FIG. 4<sup>65</sup>

### 3.4 The Merger of *a* and *ä*

3.4.1 In the manuscript of HP, *miqpatš puma* and *miftah puma* are never in a position of contrast (except for one minor exception, which can, moreover, be explained phonetically, see below). In certain positions, *miqpatš puma* is uniquely determined, viz. in word final accented position and in penultimate syllables followed only by /ʔ/ and a back vowel. The occurrence of /â/ in these positions seems phonetically conditioned, in the former case by word final lengthening leading to raising and rounding and in the latter case by vowel-vowel assimilation.<sup>66</sup>

<sup>63</sup> Morag supports this view as well by the names of this vowel in Babylonia, *miqpatš puma* = closure of the mouth, *ʔimša* = tightening, both of which seem to refer to a higher or closer vowel. My feeling is that he is on safer ground interpreting the vowel names on the basis of his identification of the phonetic value of the vowel than the reverse. Compare, for instance, the Syriac “long” for short (see note 41 above). The comparison is not strictly relevant, of course, but, nevertheless, suggests caution in deriving phonetic information from ancient terminology (see also note 75 below).

<sup>64</sup> Z. Ben-Hayyim (“The System of Vowels of R. Sa’adya Gaon” [in Hebrew], *Leshonenu* 18 [1953]: 89–96, esp. 90–91) and A. Dotan (“Masorah,” *Encyclopedia Judaica* [Jerusalem: Keter, 1972], 1443) also interpret the “Babylonian *qamets*” as *â*. However, even such recent writers as Yeivin (“Babylonian Vocalization,” 44) and Sharvit (“*Qamets* and *Holem*, and *Segol* and *Patakh*,” 554) have regarded the question as a standoff.

<sup>65</sup> This vowel chart represents to a certain extent an idealization. The /â/ may have been higher; the /ä/ may have been further back. In view of the evidence for fronting of the /o/, the relevant contract may not have been front-back so much as round-spread (see also Shelomo Morag, “The Tiberian Tradition of Biblical Hebrew: Homogeneous and Heterogeneous Features” [in Hebrew], *Peraqim* 2 [1974]: 104–44, esp. 127–28). Compare Modern Azerbaijani Jewish Aramaic where the back vowels vary “freely from back to central to centralized front in N (= the northern dialects)” (see Irene Garbell, *The Jewish Neo-Aramaic Dialect of Persian Azerbaijan: Linguistic Analysis and Folkloristic Texts* [The Hague: Mouton, 1965], 26).

<sup>66</sup> See my “Low Vowel System,” 135–38 (sec. 2.4).

In all other phonetic surroundings where *miqats puma* appears, it is as a variable conditioned variant of *miftah puma*. The basic conditioning factor is that the following consonant must be a voiced continuant (or  $\theta$ ). As discussed at length in my forthcoming paper “Low Vowel System” (see n. 58 above), these consonants have a cross-linguistic tendency to lengthen preceding vowels, and phonetically longer vowels often approximate to the outside of the vowel envelope and rise. The precise effect of some of these consonants, e. g., *n* and *r* in raising *a* to *o* in Babylonian Aramaic and other Aramaic dialects is well known.<sup>67</sup> Morag already discussed this effect with regard to following /w/, where of course assimilatory rounding plays an even stronger role.

What is most interesting with regard to this effect in HP, however, is the fact that secondary conditioning factors can also be isolated, i. e., given the presence of the prime factor (the following consonant), other phonetic features in the word increase or decrease the frequency of *â*. Thus if the preceding consonant or next following vowel are labial, /m, w,  $\beta$ , u, o,  $\text{â}$ /, the numbers of *miqats puma* are dramatically increased. If the next following vowel is a front vowel, however, incidence of *miqats puma* is reduced almost to zero.<sup>68</sup>

The only occurrences of *miqats puma* in the MS which do not seem to be clearly phonetically conditioned are in the so-called infinitive absolute, e. g., *mitâ $\beta$  ya $\theta$  $\beta$ inan*, “sitting we sit” (p. 214), as opposed to the normal infinitive used as a verbal complement, for example, *l $\text{m}$ it $\beta$ al*, “to immerse” (p. 214). However, this should also be explained as synchronically phonetic conditioning. First, from a historical point of view it is clear that the “infinitive absolute” arose as a special form because of phonetic conditioning. To the best of our knowledge, earlier Aramaic had only one form of the infinitive with a short *a*. The only dialects in which the lengthened \* $\text{â}$  is known are the Targum vocalization and HP (and Y). I assume, therefore, that under the special circumstances of syntactic emphasis for which the infinitive absolute was employed, the vowel came to be lengthened. There is no reason not to believe that the same phonetic conditions still obtained at the time of HP.<sup>69</sup>

<sup>67</sup> See Nöldeke, *Syriac Grammar*, 31–32 and Jakob Nahum Epstein, “Zur Babylonisch-Aramäischen Lexikographie,” in *Festschrift Adolf Schwarz zum siebzigsten Geburtstag* (Berlin and Vienna: R. Löwit, 1917), 319–22.

<sup>68</sup> For a full discussion, examples, and statistical tests of the significance of these effects, see Boyarin, “Low Vowel System,” 133–35 (sec. 23). Here suffice it to say that the confidence levels achieved by approved statistical measures of significance were never larger than .01.

<sup>69</sup> Lest this be regarded as a *deus ex machina*, I refer to Goldenberg’s statement – made for purely grammatical reasons: “the front-extraposed infinitival subject [in BJA] is not marked as isolated by any specific particle, *but was in all probability made to be heard as such by dint of some characteristic intonation*” (Gideon Goldenberg, “Tautological Infinitive,” *Israel Oriental Studies* 1 [1971]: 44 [italics mine]). It is striking how the synchronic syntactic investigation and diachronic phonetic aspects coincide.

Thus I conclude that it is reasonable to assume that in the dialect of HP *miqpatš puma* and *miftah puma* represent allophones of a single phoneme. The phonemes /ä/ and /â/ of the earlier language had merged yielding a five vowel system (fig. 5).<sup>70</sup>

*The Later BJA Vowel System*

|          |       |
|----------|-------|
| i (1)    | u (6) |
| e (2)    | o (5) |
| a (3, 4) |       |

FIG. 5

The plausibility of a development of this sort having taken place is much increased by comparative evidence. In the Jewish modern Aramaic dialects of Azerbaijan, described by Garbell,<sup>71</sup> the same five vowel system obtains, having been generated by the same \*ä-\*a merger hypothesized for BJA. Unfortunately, I am not in a position to prove that the reflex of \*ä had been ä earlier in that language's history, but this seems at least very likely. Even more striking, however, is the fact that many of the identical allophonic rules occur. Thus the phoneme /a/, whose basic allophone is an "unrounded low front to front central a," is actualized in word final "as a rounded low back, slightly nasalized [ɔ] and by the female speakers of the U sub-dialect in N [Northern dialects] as a rounded back lower-mid [ɔ]." Furthermore, "[ɔ] is also an allophone of /a/ in the sequence /-ao-/ [bɔʔx]," to which compare the effect of *w* in HP. And also "a/ has a centralized allophone [ä] [...] in S [Southern dialects] preceding /-Ci-/: jariq [järiq] [...]"<sup>72</sup> to which compare the near total absence of *miqpatš puma* in precisely that environment in HP. Although Garbell claims these dialects are not direct descendants of BJA,<sup>73</sup> they must at very least be descended from closely related ones. These parallels, then, are more than typological and strengthen considerably the present theory.

3.4.2 This hypothesis affords a great simplification of the diachronic picture (see sec. 2 above). First, we need no longer posit an unmotivated and synchronically

<sup>70</sup> The proximate phonetic explanation for this merger is the fact that for a combination of articulatory perceptual reasons, languages prefer to have more front-back distinctions at higher than at low position (see Björn E. F. Lindblom, "Phonetics and the Description of Language," in *Proceedings of the Seventh International Congress of Phonetic Sciences*, ed. André Rigault and René Charbonneau [The Hague: Mouton, 1971], 86–87; I thank Ms. Malcah Yaeger for this reference). Note also that a similar merger has taken place in the Palestinian system, as well as in Eastern Syriac, as argued above (see Morag, *Hebrew Language Tradition*, 103).

<sup>71</sup> Garbell, *Jewish Neo-Aramaic Dialect*, 26.

<sup>72</sup> *Ibid.*

<sup>73</sup> Irene Garbell, "Flat Words and Syllables in Jewish East New Aramaic of Persian Azerbaijan and the Contiguous Districts: A Problem of Multilingualism," in *Studies in Egyptology and Linguistics in Honor of H. J. Polotsky*, ed. Haiim B. Rosén (Jerusalem: Israel Exploration Society, 1964), 102–3.

superfluous length distinction between *ā* and *ä*. Secondly, there is no paradoxical shortening of vowels under stress. Where I have posited stress lengthening (in word final position and the infinitive absolute), the increased duration has the plausible effect of allophonically backing the vowel (i. e., increasing its peripherality vis-à-vis the vowel envelope), with concomitant rounding and perhaps raising.<sup>74</sup> Thirdly, there are no ad hoc analogies required (see n. 20 above), and finally, it accounts for the unexplained cases of *miftah puma* for historical \**ā* and *miqpatš puma* for historical \**a*, e. g., *ʾəmāru*, “they said” < \**ʾamaru* (p. 236).

3.4.3 Assuming that my hypotheses are correct up to this point, there were two systems of vowels in use in Babylonia in the Gaonic period: the archaic six vowel system used for Biblical Hebrew, Targum, and liturgy, and the colloquial five vowel system. A great deal of effort would have been required of liturgical readers and scholars and the like not to merge the /*ā*/ with /*a*/ or /*o*/.<sup>75</sup>

Students and the relatively unlettered would very likely have heard the /*ā*/ as /*o*/. This explains the total confusion of /*ā*/ and /*o*/ manifested by the two Hebrew texts discussed above (sec. 3.3.3). Were /*ā*/ retained as a separate phoneme in the speech of the scribes of those documents, the inverted spellings of *miqpatš puma* for /*o*/ and *w* for /*ā*/ would of course be inconceivable. Since there is no evidence of a general /*ā*/-/*o*/ merger in BJA, the only remaining explanation is the one I have proposed – loss of /*ā*/ in speech and its identification with /*o*/ in liturgical use by the nonsophisticated. Of course the allophonic [ā] of BJA colloquial speech in this period, which has been argued for in the previous section, could also have been identified with /*o*/.

Turning, however, to the magic bowls, we find some problems. It may be fairly stated that most of the cases of *vav* for \**ā* are where they would be predicted by my hypothesis, i. e., in words whose form or context proves them Hebrew or Targumic. Thus we find:<sup>76</sup>

- (a) *ʾwdwn*, “lord”<sup>77</sup>
- (b) *wlʿlwm*, “and forever”<sup>78</sup>
- (c) *hʾwrš*, “the earth”<sup>79</sup>

<sup>74</sup> The idea that long *a*'s may be backed and raised to *ā*, *ɔ*, or even *o* will come as no surprise to Northwest Semitists. See note 72 above for a modern Aramaic parallel (i. e. the word-final raising) and Boyarin, “Low Vowel System,” 133n13 for more examples.

<sup>75</sup> It might be noted that this view would tend to support Morag's explanation of another name for *miqpatš puma*, namely *metsaf puma*. Morag explains this name as referring to the “special attention” required for the correct pronunciation of this vowel. If his etymology is correct, it would seem to be best explained if the vowel in question did not occur in the vernacular.

<sup>76</sup> I cite these forms in transliteration, grapheme for grapheme.

<sup>77</sup> Cyrus H. Gordon, “Aramaic Magical Bowls in the Istanbul and Baghdad Museums,” *Archiv Orientalní* 6 (1934): 321 (text A).

<sup>78</sup> *Ibid.*, 322 (text A).

<sup>79</sup> Cyrus H. Gordon, “Aramaic Incantation Bowls,” *Orientalia* 10 (1941): 117 (text 1).

- (d) *hwrwḥwt*, “spirits”<sup>80</sup>  
 (e) *hwrwḥ*, “spirit”<sup>81</sup>  
 (f) *gwʿwr*, “exorcised”<sup>82</sup>  
 (g) *byrwšwlym*, “in Jerusalem”<sup>83</sup>  
 (h) *bswtwry*, “with Satans”<sup>84</sup>  
 (i) *bwrwk*, “blessed”<sup>85</sup>  
 (j) *hwʿwlm*, “the world”<sup>86</sup>  
 (k) *ʿwlwk*, “on you”<sup>87</sup>

These forms are clearly Hebrew. We find also clear targumic forms:

- (l) *hwdyn*, “this”<sup>88</sup>  
 (m) *qdwmy*, “before him”<sup>89</sup>  
 (n) *ywtyh*, “him”<sup>90</sup>

Of the forms in the last group, Gordon writes, “Ginsberg is inclined to attribute the occurrences of such Official Aramaic forms to imitating the Targums,” and this without reference to the *w* spelling but only morphological features.<sup>91</sup>

Another group of forms, occurring in one bowl, cannot be accounted for in this way however:

- (o) *rwhyn byšwtʿ*, “evil spirits”<sup>92</sup>  
 (p) *ʿynwšʿ* (4×), “man”<sup>93</sup>  
 (q) *dšmwłʿ*, “of the left”<sup>94</sup>  
 (r) *dsmwłʿ*, “of the left”<sup>95</sup>

One notices here a tendency to hyper or pseudo-correction in this bowl, as shown by the Hebraizing spelling with š in (p) and the barbarism *rwhyn byšwtʿ*

<sup>80</sup> Ibid., 127 (text 7).

<sup>81</sup> Ibid.

<sup>82</sup> Ibid.

<sup>83</sup> Ibid.

<sup>84</sup> Ibid., 273 (text 11).

<sup>85</sup> Gordon, “Aramaic Magical Bowls,” 332 (Hyvernat’s bowl).

<sup>86</sup> Ibid.

<sup>87</sup> Isbell, *Aramaic Incantation Bowls*, 152.

<sup>88</sup> Gordon, “Aramaic Incantation Bowls,” 346 (text g).

<sup>89</sup> Ibid.

<sup>90</sup> Ibid.

<sup>91</sup> Ibid., 346n1. See also Rossell’s remark: “Note also that  $\hat{a} > \hat{o}$  (as shown by mater lectiones  $\text{ܐ}$ ) goes with the  $\text{ܐܘܝ}$  form (i. e.,  $\text{ܩܪܢܘܝܗ}$  versus  $\text{ܩܪܢܝܗ}$  where  $\hat{a}$  remains” (Rossell, *Aramaic Magical Texts*, 22). Also: “This usage of  $\text{ܐ}$  is a literary affectation from Biblical Aramaic or still more likely the Targumim [...]” (ibid., 37). See also Joshua Blau, *On Pseudo-Corrections in Some Semitic Languages* (Jerusalem: Israel Academy of Sciences and Humanities, 1970), 54–54, citing Rossell approvingly here.

<sup>92</sup> Cyrus H. Gordon, “Aramaic and Mandaic Magical Bowls,” *Archiv Orientalní* 9 (1937): 86 (text H).

<sup>93</sup> Ibid., 86–87 (text H).

<sup>94</sup> Ibid., 86 (text H).

<sup>95</sup> Ibid., 87 (text H).

of (o). We should expect either *ruḥin bišan* or *ruḥe bišaḥa*, of course.<sup>96</sup> Perhaps this Hebraizing or hyper-correcting tendency could explain the *w*'s here. We are still left with a residue of unexplained *w*'s however.

- (s) *ml'kw*?, “angel”<sup>97</sup>
- (t) *qynywnhwn*, “their property”<sup>98</sup>
- (u) *lyšwn* (3×), “tongue”<sup>99</sup>
- (v) *hwmry byšwt*?, “evil pebble-spirits”<sup>100</sup>

<sup>96</sup> See Blau, *On Pseudo-Corrections in Some Semitic Languages*, 24–25 for *s/š* hypercorrection in Medieval Hebrew. I interpret *rwḥyn* here as the effort of a scribe used only to the Eastern plural *ruḥe* attempting to write “correct” Aramaic and misusing the -in ending in the emphatic state – a classic case of “over-self-denial” in Blau’s terminology (*ibid.*, 15). For another sort of hypercorrection see below sec. 3.5, esp. the paragraph beginning “The most dramatic [...]”

<sup>97</sup> Gordon, “Aramaic Incantation Bowls,” 117 (text 1).

<sup>98</sup> *Ibid.*, 127 (text 7).

<sup>99</sup> *Ibid.*, 273–74 (text 11).

<sup>100</sup> *Ibid.*, 273 (text 11). It is not to be excluded that this form is the abstract noun, *bišuḥa*, in which case, of course, it is irrelevant here. The phrase would then be a construct-genitive construction. Likewise (o): *ruḥin bišuḥa* may be an error for *ruḥe bišuḥa*, “spirits of evil.” The noun *bišuḥa* is well attested in these texts. Note also that *hwmry byš't*, *zḥny't*, and *byry't* also occur in this text. I am inclined, therefore, to see *hwmry byšwt* as a syntactic variant of *hwmry byš'ta*, not orthographic or phonetic and therefore as irrelevant here.

Some other forms which have been cited in this regard are based on mistaken or ambiguous readings and/or interpretations. In the former category, we place such readings as *myd'w'm*, “something” (Gordon, “Aramaic Magical Bowls,” 328 [text D], cf. Pl. XIII), which certainly must be read *mydy'm*, yielding phonetic *miḏem*, precisely the expected form. The *yod* and *vav* are quite indistinguishable in most of these texts. Likewise Myhrman’s *wbwythyyn*, “and their houses” (see David W. Myhrman, “An Aramaic Incantation Text: Studies in Assyriology and Archeology,” in *Hilprecht Anniversary Volume* [Leipzig, 1909], 342–51, esp. 345) must be, of course, *wbytyhwn*, as seen by Isbell, *Aramaic Incantation Bowls*, 24. A more difficult case is posed by a series of forms in Gordon, “Aramaic Incantation Bowls,” 124–25 (text 6):

- (a) *zr'wh* (2×), “her seed”
- (b) *bytwḥ* (3×), “her house”
- (c) *pqynynwh* (2×), “and her property”

Various strategies may be adopted to explain these forms. Gordon transcribes the *w* with *qa-mets* and calls these “double *matres lectionis*” (*ibid.*, 126). I deduce from this designation that he considers the original consonantal /h/ as apocopated. That being the case, one could regard this phenomenon as the word final allophonic raising of /a/ which I have posited (see also *ibid.*, 358, where Gordon seems explicitly to regard the final *h* as apocopated). A further possibility (not very convincing) would be to regard these forms as Hebraizing. The most likely explanation in my opinion is that *w* here is to be read *y* (a possibility considered by Gordon *ad loc.* as palaeographically possible). The forms would then represent the substitution of masculine *-eh* for feminine *-āh*, well documented for BJA in Shamma Friedman, “Three Studies in Babylonian Aramaic Grammar” [in Hebrew], *Tarbits* 43 (1973): 64–69. The least likely alternative is that in the BJA dialect of the writer the feminine suffix was *-ah* (as claimed by Rossell). There is simply no evidence for such a form in BJA (see Joseph L. Malone, “The Isolation of ‘Schematisierung’: A Service of Linguistics to Philology,” *Journal of the American Oriental Society* 94, no. 4 [1974]: 396–98).

Finally, the form *byšmwk*, “in your name” (Cyrus H. Gordon, “An Aramaic Exorcism,” *Archiv Orientalní* 6 [1934]: 467 [text G] and *passim*; cf. “Aramaic Incantation Bowls,” 120 [note on 2:1]), must be dealt with. If this is to be read with *w*, it is in any case Hebrew (or Hebraizing) as

- (w) *bw'wt'*, "request"<sup>101</sup>  
 (x) *'lmw'*, "the world"<sup>102</sup>

If I wished to maintain my explanation of all of these spellings as owing to lack of *â* in the scribe's speech and his consequent merger of the Hebrew and targumic phoneme with *o*, I would be hard pressed to account for this last group. However, another explanation may be offered for all these spellings (including perhaps those of the previous groups). They all occur in environments where allophonic *miqpatš puma* occurs in HP. They would then, nevertheless, support my general hypothesis. Such spellings of conditioned [ɔ] are well known from Gaonic writings as well and even from the Talmud<sup>103</sup> and therefore may be plausibly supposed here. A pseudo-correcting explanation may not be completely excluded, however, even for this last group.

All in all, our conclusion of a merger of /â/ and /ä/ in the BJA of the Gaonic period seems quite justified in the light of the distribution of *miqpatš puma* and *miftah puma* in HP. The conclusion seems well supported in addition by the /â/-/o/ confusions of certain uneducated scribes in writing Hebrew and "high" Aramaic. According to my hypothesis, the scribe of HP himself identified the allophonic [â] of his dialect with the phonemic /â/ of Targum and Hebrew and thereby adopted *miqpatš puma* to spell his allophone. Thus in Babylonia at that time there was a kind of Aramaic diglossia, with *a* and *â* as allophones in colloquial speech and in reading the living literary language but phonemically distributed in Targumic reading. This would have been the situation in the last centuries of living Aramaic speech and literary productivity among Babylonian Jewry.

### 3.5 Yemenite Re-creation of the Opposition

The Yemenite Jews received their literary and linguistic traditions from Babylonia at just about this time. This readily explains their realization of *qamets* as *â*, as well as the closeness of their talmudic reading to many aspects of *Halakot Pesuqot*. However, in their reading of the Talmud, *â* and *ä* are differently distrib-

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the context shows: *bišmæx 'ani 'oše*, "in your name I do." Although, to be sure, it does not always occur in this context, it does so often enough that it may be regarded as always Hebrew. It is not impossible, however, that the correct reading is *byšmyk*, in which case the form would be *bišmex* or *bišmix*, by no means a surprising form in BJA.

<sup>101</sup> Gordon, "Aramaic Incantation Bowls," 339 (text 19745).

<sup>102</sup> Isbell, *Aramaic Incantation Bowls*, 150.

<sup>103</sup> Epstein ("Gloses babylo-araméenes," *Revue d'études juives* 73 [1921]: 49 and "Gloses babylo-araméenes [pt. 2]," *Revue d'études juives* 74 [1922]: 50–51) has gathered examples of *vav* for \*â from Montgomery's texts and explicitly connected them with the conditioned shift before continuants discussed above and in his earlier work (Zur "Babylonisch-Aramäischen Lexikographie"). The form *swpryy'*, "scribes," which he cites (*ibid.*, 51) should be explained as assimilation to /f/, as likewise the form 'wpykh, "overturned" (Gordon, "Aramaic Incantation Bowls," 348 [text 9726]), where the vowel was originally short *a*, of course.