ON FUZZY BOUNDARIES AND RAZOR SHARP BOUNDARIES, AND SOMETHING ON BARI

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Abstract

This is a review article of Fs. Loprieno. Antonio Loprieno's career spans many nations and languages; considering 1) all this internationality, 2) my own native tongue, and 3) BiOr's base in Holland, it seemed almost tempting to entitle this article in Dutch as "Over ruige grenzen en haarscherpe grenzen, en iets over Bari." Fewer than half of the 61(!) articles in this volume pertain to topics that I have engaged in my own research. The focus of the article is therefore almost exclusively on them; I hope that someone else will pay proper tribute to the many others. Instead, I intend to present alternative lines of argument in order to put theirs in perspective by offering my own approach to the problems that they study. In counterpoint, as it were, to use a musical metaphor. I am not sure, though, about the extent of the harmony between the point and the counterpoint.

A professional review of a book is supposed to be critical. "Critical" derives from the Greek verb meaning "to distinguish." The critical approach requires one to be open to different lines of argument in order to put their lines of arguments head-on. Instead, I intend to present alternative lines of argument in order to put theirs in perspective by offering my own approach to the problems that they study. In counterpoint, as it were, to use a musical metaphor. I am not sure, though, about the extent of the harmony between the point and the counterpoint.

1. FUZZY BOUNDARIES

Two questions arise. First, to what do the "fuzzy boundaries" of Loprieno’s article make reference? Second, how did the expression end up in the title of his Festschrift?

In origin, the so-called fuzzy boundaries have everything to do with the particle *jw* in Egyptian. I have myself written an article specifically about *jw* entitled "The Meaning of Old and Middle Egyptian *jw* in light of the Distinction between Narration and Discussion" in a volume entitled Jerusalem Studies in Egyptology, which appeared in 1998. I have also discussed *jw* at length in an earlier article published in 1995 in the *Chronique d’Égypte* entitled "On the Empirical Distinctness of Certain Adverbial Clauses in Old and Middle Egyptian". Evidently, when it appeared that fuzzy boundaries were in origin all about *jw*, my attention was piqued as to what is going on here.

There is no one who doubts that, in the last three stages of Egyptian — Late Egyptian, Demotic, and Coptic — *jw* as a
rule marks subordinate circumstantial clauses and can be translated as “while” or the like. The particle jw is itself very generally, and vaguely, circumstantial and subordinate. In translation, it can be rendered in many different ways, for example as “because,” “since,” “although,” and so on. But these renditions all involve refinements introduced by translators to make a translation more nuanced. These refinements are in no way part of the meaning of jw itself.

Before Late Egyptian, in the first two stages of Egyptian, Old and Middle Egyptian, the behavior of jw is strikingly different. Everyone agrees and has always agreed that Old and Middle Egyptian jw can introduce independent sentences. But not always: everyone also agrees and has always agreed that Old and Middle Egyptian jw can also introduce subordinate clauses. Moreover, everyone agrees and has always agreed that Old and Middle Egyptian jw more often marks subordinate clauses when a suffix pronoun is attached to it than when it is followed by a noun or noun phrase.

The result is two acute problems involving what Aristotle repeatedly identified as the most fundamental law of rational thought, namely that it is not possible for anything to be something and not something — that is, its opposite — at the same time. Something cannot be a cow and not a cow at the same time. A correlate of Aristotle’s fundamental law of thought is as follows: Something is either a cow or not a cow. This is the so-called Law of the Excluded Middle.

What are the two problems?

The first problem is synchronic: jw apparently can at the same time signify both something and its opposite, that is, independence and dependence. When it is dependent, it is a marker of so-called circumstantial or adverbal clauses. How is that possible?

The second problem is diachronic. It involves the assumption that jw was in origin mainly, if not exclusively, of independence. The question arises: How can a linguistic element evolve from signifying something to signifying exactly the opposite of that something? James Allen has aptly described the problem as follows: “A syntactic marker of independence came to have exactly the opposite value in Late Egyptian is a challenge to explain on purely syntactic grounds”.

It so happens that I have proposed, more than two decades ago, in the afore-mentioned article entitled “On the Empirical Distinctness of Certain Adverbial Clauses in Old and Middle Egyptian,” what I believe to be a complete explanation of, 1), how jw can simultaneously denote either independence or dependence in Old and Middle Egyptian in strictly empirical terms and, 2), how jw can evolve from originally denoting mostly if not always independence to denoting always dependence in strictly empirical terms.

Loprieno’s solution is different from mine. He postulates a “grey area” between independence and dependence, something that is not quite independence and not quite dependence. As jw evolved from an Old and Middle Egyptian particle that could mark independence into a Late Egyptian particle that always marks dependence, it is assumed in Loprieno’s view to denote — in the transition from independence to dependence — something that is not quite either. According to this view, the boundary between independence and dependence is not sharp. In other words, it is not the case that dependence begins where independence ends, or vice versa. The boundary could therefore be described as fuzzy, a metaphorical term chosen by Loprieno. Hence the designation “fuzzy boundaries.”

I should make clear right at the outset that I am personally not inclined to believe that the phenomenon of fuzzy boundaries exists when it comes to the evolution of jw in Egyptian, nor in fact when it comes to the evolution of any other grammatical element. I am not sure what exactly my view implies for the many contributions in the volumes at hand that were inspired by the notion that fuzzy boundaries exist in Egyptian to seek boundaries, fuzzy or not, also elsewhere.

In my view, when Egyptians heard an instance of jw, they typically knew — on the basis of clear and undeniable empirical signals — that jw was either independent or dependent. How so?

To give proper perspective to the problem of jw, it is important to note that jw is definitely not the only prominent case in which modern interpreters of Old and Middle Egyptian texts struggle mightily, and have always struggled mightily, with assessing whether a certain string of words is an independent sentence or a dependent clause. So very often, it is not clear — and will never be certain — whether a given string of words introduced by a sḏm.f form or a sḏm.n.f form is independent or dependent. An interesting twist to the story is that the problem with jw and the problem with certain sḏm.f and sḏm.n.f forms are connected. They are part of a single story line. Not only do the two problems both involve difficulty assessing the presence of either independence or dependence. In addition, dependent clauses introduced by a sḏm.f form or by a sḏm.n.f form in Middle Egyptian are succeeded in Late Egyptian by clauses introduced by jw.

What is happening here?

For the sake of clarity, it will be best to summarize what I believe happened, as described at length in the afore-mentioned article entitled “On the Empirical Distinctness of Certain Adverbial Clauses, ...”.

Presumably, at the outset, when they came into existence, the non-relative and non-substantival sḏm.f forms and sḏm.n.f forms were always independent. But then, a process was set into motion by which they became increasingly more often dependent. The process involves an empirical mechanism by which what are independent sentences can often function as dependent sentences in many if not all languages of the world. The empirical mechanism occurs very frequently in English.

For example, it is possible to pronounce the two sentences “He arrived late at night” and “It was raining” as two distinct sentences separated by a pause or a falling tone or the like. The pause or falling tone or the like can be denoted by a period in writing, as in “He arrived late at night. It was raining.” However, it is equally possible to pronounce the two sentences without a pause or a falling tone or the like at the end of the first sentence separating the two, as in “He arrived late at night it was raining.” The effect is that the second sentence becomes dependent on the first sentence and functions as circumstantial or adverbial clauses do in Egyptian. The great frequency of the phenomenon is obscured by a standard convention of most modern written prose that one should typically not run on two independent sentences without punctuation separating them.

The result of omitting a pause or a falling tone or whatever empirically marked the end of one independent sentence and the beginning of another is that a following sentence becomes subordinate to a preceding sentence and not the other way around. And indeed, I believe that, when (non-relative or non-substantival) sḏm.f and sḏm.n.f are subordinate and part of an adverbial clause, they are always subordinate to a sentence that precedes and never to a sentence that follows.

That view is not universally shared. There are those that believe that a sḏm.n.f or a sḏm.f can be part of an adverbial clause that is subordinated to what follows. Perhaps most prominent among them in more recent times is Pascal Vernus in articles from the 1980s in the Revue d’Égyptologie and also elsewhere. I believe that all examples that have been adduced in favor of this view can be explained differently. What is more, in all of Late Egyptian and in all of Demotic, circumstantial or adverbial clauses — which are introduced by jw — are always subordinate to a sentence that precedes. Then why would adverbial clauses containing a sḏm.f or a sḏm.n.f be different?

What is more, it is generally assumed — as far as I can see — that, when Old and Middle Egyptian jw heads an adverbial clause, it is always subordinate to a sentence that precedes. The explanation proposed above also accounts for the fact that jw is more often perceived as being subordinate when it is followed by a suffix pronoun, as opposed to by a noun or noun phrase. Third person suffix pronouns refer to something mentioned in a preceding sentence and therefore typically exhibit a closer connection with what precedes than a noun or noun phrase.

It is not possible to observe pauses or any equivalent marking the end of a sentence in speech in dead languages that are attested in writing only. But it seems safe to assume that it was possible for speakers of Egyptian to hear when one independent sentence ends and another begins, whether by means of a pause or by any other empirical signal. It is impossible to establish what that empirical signal may have been exactly. But it seems safe to assume that there must have been one. Moreover, it seems safe to assume that it was possible to omit or cancel the signal.

In my afore-mentioned article for the Chronique d’Égypte of 1995, I described the empirical signal audibly marking the end of a sentence mainly as a pause. I should perhaps expand that definition to any audible empirical signal that marks the end of an independent sentence, whatever that empirical signal is, probably a pause, or at least something including a pause, or perhaps even something else, whatever it may be. In the end, it cannot be known what that signal exactly was in the case of a dead language. And it does not really matter. It remains safe to assume that there must have existed such a signal because Egyptians generally must have been able to hear when sentences end. What is more, I have proposed that it was possible to omit the signal, effectively turning an independent sentence into a dependent one.

It is impossible to directly or indirectly observe the presence of an audible empirical signal signifying the end of a sentence in a dead language. Then again, it does appear to be possible to indirectly observe the absence of such a signal. There are unambiguous cases in which a sḏm.f or a sḏm.n.f, or the negation thereof, cannot be independent, as I noted in the Cde article, at pp. 32-33. And yet, there is nothing in writing that marks the forms in question as dependent.

Three constructions are involved:

1) the verb forms in question are subordinated to a relative verb form;
2) the verb forms in question are emphasized;
3) the verb forms in question are circumstantial to what precedes in terms of tense.

Three examples are as follows: 1) h"w ntrw mšš.sn sw “he at the sight of whom the gods rejoice” (Coffin Texts ed. A. de Buck IV 610 1.2Lj); 2) ḫḏ.n.f nn ṭḥ.n.f ḫḏ.f “It is because he knew my character that he said this” (Sin. B32); 3) jr.jn ṣḥṭyṣ pn h"w r hrw 10 ṣr,rp n Nmty-nḥt pn n ṭḏ.n.f ṭḏ.f s r.s “This peasant spent ten days supplanting this N, without him inclining his ear to it” (Eloquent Peasant B1,31-32 = R80-81).

Another instance of 1) is as follows: h"t R mšš.f (s)y “the one at the sight of whom the gods rejoice” (literally, “the one [fem.] while seeing whom [her] the gods rejoice”) (CT III 294c GI1).

As H.J. Polotsky — who contributed more to the deep understanding of Egyptian and Coptic than anyone since Champollion, the decipherer of hieroglyphic writing — also noted, examples like the following only confirm the adverbial or circumstantial character of mšš.f: h"w ntrw mšš.f (the one at the sight of whom the gods rejoice) (literally, “in seeing whom [him] the gods rejoice”) (CT VI 270h GI1); h"t ḫḏwṯw mšš.f “the one at the sight of whom the netherworld dwellers rejoice” (literally, “in seeing whom [him] the netherworld dwellers rejoice”) (CT III 302g T3C). It is more than evident that mšš.f and mšš.s are adverbial. Then why would mšš.sn not be?

What matters by far the most in the present context is that, while there is no self-evident signal whatsoever of the boundary that makes mšš.f subordinate, the boundary is not fuzzy at all, even if there is every temptation to consider it fuzzy because nothing seems to overtly mark the subordination in writing. Everyone agrees that there is no empirically verifiable sentence boundary at mšš.sn sw, ṭḥ.n.f, n ṭḏ.n.f, and mšš.f (s)y.

My own explanation is that the audible marker signifying independence is missing. In writing, the empirical distinction between the presence of that audible marker and the absence of it is lost. This loss makes the boundary artificially fuzzy in writing. But the fuzziness never existed in speech. It was presumably always clear whether a string of words was either independent or subordinate.

One more interesting instance of case 2) above is as follows: ḫḏ.n.f ṭḥ.n.f ḫḏ.f “It is so that you might take me along and I might see your beauty that I have come, O lord” (literally, “That I have come to you, O lord, is while (so that) you might bring me and while (so that) I might see your beauty”) (Book of the Dead, Chapter 125, introduction. ed. Budge [blackbound edition], 249, 7-8).

A particularly piquant example of case 2) is the following, first pointed out by H.J. Polotsky in his unpublished notebook⁶). It is the answer to the following question: ḫḏ.k ḫḏ.k ḫḏ.k ḫḏ.k “Do (Did?) you say that you would cross (a substantival future sḏm.f, I am convinced) to the eastern side of heaven?” The answer is as follows: ḫḏ.k ḫḏ.k ḫḏ.k ḫḏ.k “(In order) to do what is it that you will cross?” (literally, “That you will cross is while (so that) you will do what?”)

(CT V 103e, f T1C). The significance of “prothetic” i is not fully clear. Grammatically even more spicy — a veritable delicacy — is the following example, also unearthed by H.J. Polotsky, as documented in his unpublished notebooks: 1) mrr jr.k jr.k n.f. jist (CT V 122b, d G1T). It is difficult to establish whether jr.k is 1) present and circumstantial or 2) future and circumstantial. Both forms are written alike, though the latter exhibits occasionally a double reed leaf. There seems otherwise to be no doubt that mrr is substantive (“emphatic”) and as such emphasizes adverbial jr.k.

If 1) applies, then the translation is more or less as follows: “What is that you do that your father loves you (so much)?” (literally, “That your father loves you is while you do what for him?”). If 2) applies, then the translation is as follows: “Your father loves you. But for you to do what for him (in exchange)?” (literally, “That your father loves you is so that you might do what for him?”).

In sum, it is a fact that — in written Old and Middle Egyptian — there are many instances in which no empirical markers signify that a string of words is either independent or dependent. But it is possible to prove in some of those cases that, even in the absence of a written marker, there must have been some marker in speech signifying that a string of words is dependent.

These cases would otherwise have been instances in which it is very tempting to presume, on the basis of just the writing, the presence of a fuzzy boundary: a string of words is after all not marked in writing as either independent or dependent. However, there is in fact definitely no such boundary in such instances. The strings of words in question are dependent in spite of the absence of a marker. It is therefore possible to extrapolate from such instances the eminent possibility that there are no fuzzy boundaries anywhere in Egyptian. If they definitely do not exist in instances in which there is every temptation to assume that they do, then why would they exist anywhere else? There is no reason to assume that speakers of Middle Egyptian could not discern whether a sentence had come to an end or whether it had not.

The evolution of (non-substantial and non-relative) sdm.n.f. and sdm.f. from independent verb forms to dependent verb form was presumably gradual. For a long time, dependency was marked by absence of a pause. But presumably, at some point, the verb forms in question were always dependent. As such, the need arose to use particles such as jw(f) and b’n to explicitly mark sdm.f. and sdm,n.f. as as independent. As a consequence, at some point, the mere absence of such particles would mark the verb forms as dependent, as Polotsky has already surmised.

It cannot be excluded that, because the contrast between presence and absence of a pause is fairly subtle in speech, Egyptians might on occasion have misheard whether speakers intended to leave a pause or not. As sounds travel from the mouth of a speaker to the ear of a hearer, the signal might not have been loud enough or there may have been interference from other sounds so that a hearer might not be certain whether independence or dependence was intended. To such a hearer, the signal might indeed be fuzzy. Is this what in the end proves the existence of the fuzziness that is so eagerly sought? It is not. What matters is the intention of the speaker.

This intention is not diminished in any way if it is garbled by acoustic circumstances.

In sum, the fuzziness in question affects, I believe, the modern interpretation of the Egyptian language. It is not a problem of the Egyptian language itself — at most a problem relating to the fact that the language only survives in writing.

2. INTERNATIONALITY, AND NATIONALITY, IN ANTONIO LOPRIENO’S CAREER — AND SOMETHING ON BARI

But we should not let fuzzy boundaries get in the way of joining the celebration of Antonio Loprieno’s career. In regard to his life’s history, I could not fail to notice a gaping void in these festive volumes. A dominant theme of the Festschrift is the international character of Professor Loprieno’s career, which spans several countries. Quite a few contributors note, and praise, the internationality of his career. And yet, no one seems to pay any attention as to where Antonio Loprieno came from. He was born and raised in Bari, the capital of the Italian region of Puglia, so I learned from the introduction to this Festschrift. Why is Puglia completely left out of the picture?

True, Italy’s Mezzogiorno (“midday,” but really “south,” because due south is where the sun stands at noon) has been for a couple of centuries, and still is, going through some tough times. Then again, Puglia — more specifically, the township of San Vito dei Normanni “San Vito of the Normans” (earlier, “degli Schiavoni” “… of the Slavs”) near Brindisi — is the birthplace of him whom I believe to be the second greatest composer of music of all time, Leonardo Leo (1694-1744).

Leo had his career at Naples (at that time the second largest city of Europe after Paris). Naples is also where the greatest composer of music of all time (at least, I believe so), Leo’s younger contemporary and friend, was active. Leo is still there. But his friend is transcendental. In this age of worship of Mozart and Bach, Beethoven, and Brahms (and I have to admit, they are really good, geniuses even), such a statement may seem anathema. And yet, Leo was the first to truly master counterpoint. Sir Isaac Newton had no clue of quantum theory. Still, he is considered the greatest scientist of all time. The world seems to have completely forgotten about Lionardo (sic) Orsonzo Salvatore de Leo.

Therefore, where everyone else seems to think of Turin, Göttingen, Perugia, Los Angeles, or Basel in relation to Antonio Loprieno, to me he will forever proudly be the fellow Pugliese of the inimitable Leonardo Leo. Someday, the world will truly recognize what the stellar Leo and his transcendental friend did.

And finally, I cannot resist noting that Leo’s first serious biographer, the Pugliese Giuseppe Pastore who in recent years passed away at a very high age, notes — without providing the source — that Leo’s most prominent student (alongside Niccolò Jommelli, if indeed he too was Leo’s student), the brilliant Niccolò Piccinni, whose La Cecchina may well have been the most popular opera buffa of all of the eighteenth century, called his teacher “the greatest composer of all time (il più grande Maestro di tutti i tempi)”1). Clearly,

1) Giuseppe Pastore, Leonardo Leo, s.l. (Editore Pajano – Galatino), 1957, 133.
Piccinii thought: “I am really good” (and he was). But he could not figure out how Leo did what he did. Piccinii had no difficulty recognizing Leo as his superior in the art of music. Piccinii’s honesty should serve as a model. Anyone leading a life of the mind can only benefit from feverishly searching for minds (dead or alive, mostly dead) that one has no difficulty in recognizing in all objectivity as being superior to one’s own. Nothing is more inspiring than to seek to emulate them. It does not really matter whether one succeeds or not. Trying is everything.

Piccinii isn’t Leo. But he is phenomenally good and one can discern the teachings of Leo, and in general the unique conventions of the Neapolitan school of the settecento, in his music. And Piccinii was born in — you guessed it — Bari. Surely, Bach, Mozart, and Beethoven were geniuses. But as far as I am concerned, the history of Western music exhibits huge gaps (Italian: buchi). I permit myself to quote the celebrated Benedetto Croce, a mezzogiorno intellectual like Loprieno. In the dedication to Alessandro Ademollo placed at the outset of his I teatri di Napoli (1891), Croce expressed the sentiment that it would be easy to fill the gaps in the history of Italian literature (sic) oziose. Basta contare, per esempio, le centinaia e centinaia di pubblicazioni, che si fanno ogni anno su Dante, per convincersi di questo spreco di forse. — Ma studiare Dante è un dovere! —Dovere è leggere e rileggere Dante; non già scrivere libri inutili intorno a lui.

Well said. Why write even more about Bach, Mozart, and Beethoven? Is it not time to write a little more about GBP and Leo? In fact, is it not time to rewrite the entire history of Western music?

3. PROLOGUE OF THE FESTSCHRIFT: ON SETTING BOUNDARIES AND TRANSCENDING BOUNDARIES

At this point, it would seem to be incumbent upon me as a reviewer of the volumes at hand to portray and, if necessary, to critique their contents. So it happens that Antonio Loprieno has admirers in many academic fields. As a result, about half of the contributions to his Festschrift concern fields in which I can claim no competence. In regard to these contributions, general observations will need to suffice. Moreover, a detailed review of the other thirty articles to which I can relate more directly or so would take up too much space. I will just parade the honor list of author names at the end. But I would like to stand still a little longer at the “prologue” that heads the two volumes and a couple of other articles.

The prologue (volume I, pp. 21-36) is entitled “Of the Bifurcation (Zwiespalt) of Boundaries; On the Need to Both Set and Transcend Boundaries (Vom Zwiespalt der Grenze: Über die Notwendigkeit, Grenzen zu setzen und Grenzen zu überschreiten).” The author is the eminent Emil Angehrn, professor of philosophy at Basel and earlier elsewhere. Angehrn begins by asking whether either what is bounded or what is unbounded constitute the ultimate truth. And he notes that there has never been any consensus among philosophers as to which is the case.

Emil Angehrn is a philosopher. The business of philosophers is typically not to answer questions but to improve on their formulation. Still, Angehrn proposes an answer to the question at hand and it is expressed in the title of the paper. Although I am not a philosopher, it so happens that I have myself been preoccupied with the same question in what might be called my extracurricular activities). Much to my surprise, I found many points of overlap between Angehrn’s thinking and my own. Just one example. There are several references to Aristotle’s fundamental axiom of thought in Angehrn’s article. I happen to be working on a long paper on the mathematical formulation of the three fundamental axioms of thought that developed out of thinking about Aristotle’s thinking in the Middle Ages. Simply put, they are the so-called Law of Identity (“A cow is a cow”), the Law of (Non-)Contradiction (“Something cannot be a cow and not a cow at the same time”), and the Law of the Excluded Middle (“Something is either a cow or not a cow”). I believe that the ultimate formulation of these three laws of thought must be mathematical, and not — as is commonly thought — philosophical or logical or psychological or the like. And I intend to prove it.

What is more, the notion of boundaries has preoccupied me just as much as it has Emil Angehrn and many contributors to the present Festschrift. Angehrn’s approach is philosophical. Mine is strictly physical and mathematical. Part of my own interest in boundaries takes its point of departure from the following fundamental observation: Everyone will readily agree that there are boundaries to what the brain can do. Who could deny that there are certain things that the brain can do and certain things that the brain cannot do? The need is therefore for finding and defining the boundary between what the brain can do and what it cannot do. Like a car engine, the brain is a mechanical (or rather, a biochemical) tool of limited extent that can do certain things and not other things. The need is for finding the boundary that separates what the brain can do from what it cannot do. That is a fundamental way in which I understand Angehrn’s expression “setting boundaries.” And even though there is no way of locating this boundary exactly at this time, I believe that there will in the end turn out to be nothing fuzzy at all about it.

This much for “setting boundaries.” But what about Angehrn’s “transcending boundaries”? In my own physical and mathematical approach to the problem, there are boundaries that can simply not be transcended. The brain is subject to absolute physical and mathematical limitations. There are places where it cannot go. By contrast, the universal impression seems to be that there are no limits to what the brain can do. It is true that the imagination seems to know no limits. One is free to speculate about just about anything. Then again, rational human intelligence is a kind of operating system of the brain. It is a machinery that operates strictly within its own limits. And that operating system can be described in its entirety. I believe. Then again, there is nothing against letting the imagination free range. But in doing so, one will tend to think about what one cannot really know.

Infinity epitomizes what transcends boundaries. Its existence cannot possibly be denied and yet its true nature cannot be comprehended. Just consider infinite series. Leonhard Euler, the most prolific mathematician of all time, considered them to be one of the greatest mathematical discoveries. And it is indeed stunning how infinite series involve all kinds of the wildest mathematical developments. It seems appropriate to observe in the present context that Euler is a son of Basel. No one doubts the following equation: \( 1 + \frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \frac{1}{16} + \ldots = 2. \) But to truly comprehend the infinite sum, it would be necessary to follow the sum into all eternity. That is something that rational human intelligence cannot do. In the afore-mentioned book, I make this view into the basis of a radically new interpretation of the prime sequence (2, 3, 5, 7, 11, 13, 17, 19, and so on). Understanding the prime sequence seems to be a kind of holy grail of mathematics. In my opinion, there are absolute boundaries involved in a final understanding of the prime sequence. And these boundaries are by no means fuzzy.

4. THE FOUR MAJOR COMPONENTS OF THE FESTSCHRIFT

Beyond the Foreword (vol. I, pp. 11-13), brief descriptions of the authors (vol. I, pp. 1-17), and Angehrn’s Prologue, the Festschrift consists of four components. The first component (vol. I, pp. 39-331) is entitled “Language and Writing (Sprache und Schrift)” and contains 16 articles. Of the four components, this one is by far the closest to my own interests.

The second component (vol. I, pp. 335-459) is entitled “Literature and Image (Literatur und Bild)” and contains 10 articles. Ten figures on four plates, two in the first plate pertaining to Ghislain Widmer’s article and eight in three plates pertaining to Rolf Stucky’s article, conclude volume I.

The third component (vol. II, pp. 463-843) is entitled “History/ies (Geschichtes(n))” and contains 16 articles. Ten figures on four plates, one each pertaining to the articles by Martin Bommas, Maria Luiselli, Stuart Tyson Smith, and Alex Eberle conclude volume II.

5. THE NEGATION n_sdm,fsdm.n.f js…

5.1. A Topic of Shared Interest

The very first article after the Prologue is James Allen’s “Fuzzy Negations” (vol. I, pp. 39-45). It consists of a set of miscellaneous remarks about negations, with special attention to n_sdm,fsdm.n.f js … I comment on it at much greater length for four reasons.

First, there are many other articles in these volumes about which I cannot possibly say anything that attempts to advance the cause of higher learning.

Second, I have written about js before\(^{19}\) and a much more extensive study of js has not (yet) come to fruition.

Third, the negation n… js has been studied intently by the honoree of these festal volumes himself\(^{19}\). Allen calls Loprieno’s article “groundbreaking” (p. 39). Perhaps, the following newly proposed observations and explanations will not fail to attract his interest. Consider it a small tribute of my own to the honoree.

And fourth, there is an article later in the Festschrift on an intimately related topic, the negation n-js, by Pascal Vernus. Vernus’s article is discussed below.

5.2. The Function of the Negation n_sdm,fsdm.n.f js …

The true function or meaning of the negation n_sdm,fsdm.n.f js … was discovered by H.J. Polotsky and his school around the 1960s and the 1970s. Mordechai Gilula played a critical role in this process. Helmut Satzinger also made critical contributions in those days. The function of the negation in question is to emphasize an adverbial element.

In that regard, I assume that n jr.n.j js jr.f in PT 477.8 (p. 40) somehow means “It is not against him that I did that” or “I did not do that against him,” as distinct from Allen’s emphasisless “I did not do that to him.” If the negation could mean what Allen says it does in this case, then its meaning would indeed be fuzzy — as he says it is. I otherwise do not fully understand the context of the example. But such a predicament is hardly unusual in the case of the Pyramid Texts.

Before H.J. Polotsky, no one knew what to do with the negation n_sdm,fsdm.n.f js …, especially with how it differs from negations such as n and nn. It was H.J. Polotsky and his school who established its specific meaning. It is not clear to me whether there is much more to add. But there have been attempts to say more.

5.3. The Syntax of n_sdm,fsdm.n.f js…

One issue that deserves further reflection is the syntax of n_sdm,fsdm.n.f js…, independent of its meaning or function. I am not aware of H.J. Polotsky writing anything about the matter. But then, Polotsky did not like to speculate. I agree with Allen that, as part of n… js, js “functions… as a subordinate element in exactly the same manner that it does in other constructions” (p. 43). The exact purport of “subordination” is not entirely clear to me. I am not even sure that “subordination” is the desired term. But I leave that matter aside at present.

In any event, the other side of the coin of Allen’s and my own view is that all of n_sdm,fsdm.n.f js… exhibits a syntactic relation of a certain kind, yet to be determined (though I have my own views on the matter), with the negation n. In other words, n_sdm,fsdm.n.f js… form a syntactic unit as distinct from n itself.

The question arises: How can the analysis that Allen and myself favor be proven? In what follows, I will adduce four arguments that provide critical support to the analysis.

The four arguments pertain to certain undeniable and striking facts characterizing the syntagm n_sdm.n.f js… The first argument pertains to js. The other three arguments pertain to n_sdm.n.f; of these three, two pertain to n and one pertains to sdm.n.f.

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\(^{19}\) Antonio Loprieno, “Focus, Mood and Negative Forms: Middle Egyptian Syntactic Paradigms and Diachrony,” Lingua Aegyptia 1, 1991, 201-226.
The first argument concerns the position of enclitic js in the sentence. Its very position indicates that a new unit begins with sḏm.f/sḏm.n.f in n sḏm.f/sḏm.n.f js…. How so?

It is an undeniable fact that js, when it does not accompany the negation n, is an enclitic particle. An enclitic particle typically occupies second position in a sentence. If it follows two or more words, then this means that these words form a unit with a single accent and therefore behave for all practical purposes as a single word does.

Why would js not also be an enclitic particle when it is used in conjunction with the negation n? Now, it is a fact that the enclitic particle gr(t) can intervene between n and sḏm.n.f/sḏm.f js in n sḏm.n.f/sḏm.f js…., though Gilula was able to find only two examples, Heqanakhite II recto 38 and Urkunden I 264,1312. Are there more? The second position is therefore the position immediately following n. But js does not appear there. And yet, it must appear in second position. It can be concluded that the syntactic unit in whose second position js appears begins with sḏm.f/sḏm.n.f. In other words, if a new syntactic unit begins with sḏm.f/sḏm.n.f, the position of js is exactly as expected. QED.

The three other arguments do not pertain to js but to n sḏm.f/sḏm.n.f js. The second and third arguments pertain to undeniable facts characterizing the behavior of the negation n, as follows:

Fact 1) n does not negate sḏm.n.f;
Fact 2) n negates an adverb or adverbial phrase following sḏm.n.f.

The two facts are more than two sides of the same coin. The fact that n does not negate sḏm.n.f does not explain how it negates an adverbial phrase and not the verb form that it accompanies. The latter fact is deserving of an explanation in its own right.

The third argument involves an undeniable fact pertaining to the sḏm.n.f:
Fact 3) The tense of sḏm.n.f is past.

These facts are particularly striking if one compares the behavior of n sḏm.n.f js contrastively with the behavior of n sḏm.n.f without js:
Fact 1') n does not negate sḏm.n.f;
Fact 2') n does not narrow negate an adverb or adverbial phrase following sḏm.n.f;
Fact 3') the tense of (n) sḏm.n.f is general present or aorist.

Fact 3’ was discovered by Battiscombe Gunn and published in 1924 and is hence called Gunn’s Rule. Fact 3, a kind of exception to Gunn’s Rule, was discovered a little later by the Danish Egyptologist Constantin Emil Sander-Hansen (1905-1963). Sander-Hansen edited the celebrated commentaries to the Pyramid Texts by his teacher Kurt Sethe—who was also Polotsky’s teacher—after Sethe’s death in 1934. In the commentary to PT 134a, Sethe credits Sander-Hansen for the discovery. So one assumes that Sander-Hansen made the discovery sometime in the decade from 1924 to 1934, apparently in his twenties.

How do these three facts imply that a new syntactic unit begins with sḏm.n.f in n sḏm.n.f js?

First the second argument, involving Fact 1.

As per Fact 1’, when n negates sḏm.n.f in n sḏm.n.f without js, there is a kind of semantic continuity because n affects sḏm.n.f in the sense of negating it.

In fact, in later Egyptian, n is attached prosodically to sḏm.n.f, for example in Late Egyptian and Demotic bw.jr.f sḏm “He does not hear.” In Middle Egyptian, it is not (always). In Gunn’s Syntax, I find n sḏm.n.f (Kahun 30)1313. In any event, this evolutionary trend points to a close association between n and sḏm.n.f in n sḏm.n.f without js.

Consequently, if — per Fact 1’ — there is continuity in n sḏm.n.f without js, then — per Fact 1 — the opposite is presumably the case in n sḏm.n.f with js: discontinuity. Discontinuity points to a kind of new beginning at sḏm.n.f, QED.

On to the third argument, involving Fact 3. I treat Fact 3 before Fact 2 because Fact 3 needs to be interpreted in light of Fact 1. I do not consider this argument as strong as the previous two. But it complements the other two. It is a fact that sḏm.n.f is past in n sḏm.n.f js but general present in n sḏm.n.f without js. Yet, the sḏm.n.f form must be the same. It is the (non-relative) sḏm.n.f that allows verbs of motion, exemplified by a form such as jj.n.f. The question arises: How could ancient Egyptians distinguish n sḏm.n.f as general present and n sḏm.n.f as past before the sentence reaches the next word? The form is not the differentiating feature. So what is? There must be something, If not form, then I propose syntax. How exactly? Difficult to know. At least, this argument suggests that there may some kind of syntactic differentiation. Continuity versus discontinuity at sḏm.n.f is of a syntactic nature. Partial QED.

The same third argument may be applied to the negation.

How could someone only hearing n sḏm.n.f know which way the negation was going to go?

The fourth argument involves the fact that n does not negate the sḏm.n.f of n sḏm.n.f js but rather an adverbial phrase that follows. How so? On the one hand, Allen and I postulate that sḏm.n.f js and what follows it is subordinated to the negation n. On the other hand, it is a fact that an adverb or an adverbial phrase is emphasized. How can both be the case at the same time? A possible answer is: on condition that sḏm.n.f js… is an adversative sentence. The negation of the adversative sentence mk mwt.k ḥnꜥ.k “You know with you” is n n mwt.k ḥnꜣ.k “Your mother is not with you.” Both the adversative sentence as a whole and the adverbial phrase are negated. But is sḏm.n.f js… an adversative sentence? In fact, that is exactly how H.J. Polotsky analyzed a substantival sḏm.n.f emphasizing an adverb or adverbial phrase. I do not think that Polotsky ever applied his analysis to n sḏm.n.f js… One does not find his analysis mentioned that often anymore. But I cannot see what is wrong with it. In Late Egyptian and Demotic, emphatic verb forms always emphasize adverbs or adverbial phrases. By the time of Coptic, they not always do but still mostly do. The association of emphatic verb forms or second tenses with the adversative sentence is therefore weakened in Coptic. In sum, Polotsky’s analysis perfectly complements, 1), the fact that an adversative phrase is emphasized and, 2), the assumption that sḏm.n.f is the beginning of that sentence.

5.4. Grouping in n sḏm.f/sḏm.n.f js…

It is customary to think of sḏm.f/sḏm.n.f as standing between n and js in n sḏm.f/sḏm.n.f js…, sḏm.f/sḏm.n.f being flanked by n and js as it were. However, if the above

131) Mordechai Gilula, Enclitic Particles in Middle Egyptian (in Hebrew), diss. Hebrew University of Jerusalem, 1968, 89 and 94.
analysis applies, then this spatial conceptualization is not appropriate.

Rather, what is the case is that sḏm.f/sḏm.n.f is initial (that is, in first place) and precedes the enclitic particle js, just as js follows sḏm.f/sḏm.n.f, assuming second place, as enclitic particles are wont to do. What is more, sḏm.f/sḏm.n.f and js together follow n. In that sense, it cannot be said that sḏm.f/sḏm.n.f follows n. Rather, sḏm.f/sḏm.n.f and js together do.

The difference can perhaps be clarified as follows. Consider three people, Jill, Jack, and Jane, standing in a line with Jill in first place. In a sense, Jack stands between Jill and Jane. However, what if Jack and Jane are a married couple? Then they follow Jill together as a couple and Jack does not quite stand between Jill and Jane. The latter serves as a metaphor for what happens in n sḏm.f/sḏm.n.f js …

5.5. The Locus of What is Negated by n… js

It is a fact that the negation n… js commonly negates nominal sentences, as in n jnk js pw “It is not me” (CT VI 338). In all those many instances, n… js negates the element that stands between n and js, in this case jnk. By contrast, in n sḏm.f/sḏm.n.f js… n and js do not flank the element that they negate. The negated element comes after js. How can this be?

The adverbial sentence structure, described above, already accounts for this. But if it had not, another phenomenon would have instead. I have observed for the first time — and tried to explain — that emphasis draws negation on to itself even if it is not accompanied by the negation

Consider the sentence “Paris is not the capital of Germany.” The sentence communicates one of the things that Paris is not. The negation clearly does not apply to Paris. However, if one applies contrastive emphasis to “Paris,” as in “Paris is not the capital of Germany,” then the negation now somehow applies to Paris. The sentence now communicates one of the things that the capital of Germany is not and is more or less equivalent to “The capital of Germany is not Paris.”

6. The Negation n-js

6.1. Another Topic of Shared Interest

While James Allen discussed the negation n… js earlier in the volume, Pascal Vernus turns his attention to the related negation n-js in an article entitled “The Fundamental Semantics of the Negation n-js (Le sémantisme fondamental de la négation n-js)” (I, pp. 289-300). Vernus’s articles on grammar are known for their rich documentation, the evident result of years — indeed, decades — of scouring the entire corpus of ancient Egyptian texts for evidence of all kinds of grammatical phenomena. I have been reading Vernus’s work on Egyptian grammar for decades now. I cannot help but admire its vast empirical footprint. His arguments often enlighten. I occasionally find myself in disagreement with them. But in the present case, I am struggling to merely follow the line of argument.

I comment at greater length on this topic for the same reasons as I did on n… js above. First, there are many other articles in these volumes about which I cannot competently say much. Second, I have written about n-js before

Third, the negation n-js has been studied by the honoree himself in the same articles in which he examines n… js. And fourth, there is the related article by Allen on n… js discussed above.

6.2. Ellipsis and the Relation between n-js and n… js

Much has been written about n-js. But what is the difference between n-js, in which nothing intervenes between n and js, and n… js, in which something does? Mordechai Gilula was the first to formulate clearly that both n-js and n… js involve contrastive emphasis affecting an adverb or adverbial phrase. There is no doubt in my mind that they do. In a way, both have the same function.

Now, Vernus (I, p. 289) cites Gilula’s opinion that n-js is just “another way [in addition to n… js] to negate the adverbial predicate of an Emphatic sentence”

But if both n… js and n-js express the same, then what is the difference?

Many years ago, I must have been in the 1980s, Polotsky — who was both Gilula’s and my own teacher — orally conveyed to me a certain concern that his student Gilula had not appreciated to the fullest extent his own view that n-js can simply be obtained by eliding something between n and js.

Consider a well-known example of n… js such as

$\text{n m.n.k } n'th.t(j) n \text{m.n.k } mt.t(j) \quad (\text{CT I 187d-e B10C})$

“You have gone (that is, died). You have not gone dead.” (That is, you will live after death.)

Nothing may have seemed more natural to ancient Egyptians, as it is to modern English speakers, than to omit the second occurrence of the same verb form:

$\text{*n.m.n.k } n'th.t(j) n\text{-js } mt.t(j)$

“You have gone away alive. Not dead.”

There is something eminently simple and evident about ellipsis as the phenomenon that differentiates n-js from n… js.

It is true that, as Vernus points out in his article, it is not always possible to reconstruct grammatically what is being elided. And this concern leads him to discard this analysis (see below). But I assume that, in such cases, the use of n-js has been generalized and the negation involves the non-repetition of a preceding thought. The elided thought may not fit grammatically into n… js. Still, n-js can still mark the non-repetition of the thought by the mere empirical absence of the thought. After all, there is no need for formulating the thought grammatically if it is not even uttered.

In this connection, my principal criticism of all that has been written about n-js is the absence of the following crucial concept. N-js marks, at least in origin, the beginning of a new sentence. It does not belong to the preceding sentence, as many descriptions of the negation seem to presuppose. Consider the sentence “When did he arrive? Yesterday. Not today.” The elliptical sentences “Yesterday” and “Not


today," for non-elliptical "He arrived yesterday" and "He did not arrive today," are new sentences. And so is n-js, at least in origin.

As to the above analysis involving elision, I presented it in an article more than two decades ago\(^\text{17}\). I am not sure that I can take any credit for it as my teacher Polotsky conveyed its essence to me.

6.3. A “Brilliant Hypothesis,” but Whose?

It came as a bit of a surprise to me that Vernus presents the analysis described in § 6.2 as a “brilliant hypothesis (brillante hypothèse)” (I, p. 294).

But three other pronouncements relating to this analysis by Vernus were equally surprising.

First, the “brilliant” thesis is attributed to Antonio Loprieno (I, pp. 289-290). Vernus refers to two articles by Loprieno of 1991. But I cannot readily find the thesis in question in either. Nor can I find the thesis in a more recent definition of the function of n-js by Loprieno in his well-known linguistic introduction to ancient Egyptian\(^\text{18}\). I am kind of curious where Vernus finds this thesis in Loprieno’s work.

I refrain from trying to establish how exactly Loprieno defines the function of n-js. But I have the impression that my interpretation differs radically from his. In one of the two afore-mentioned articles, Loprieno translates an example of n-js differently from how I would. The example is CT III 336f-g, as follows:


Loprieno translates this passage as follows\(^\text{19}\):

“Not my father gave (it) to me, not my mother gave (it) to me.”

I do not fully understand why the passage does not read \[n\text{-}js mwt.J rdt n.J\], with feminine ending t. Coffin S1C is the only witness and its version is on the whole quite good.

I would translate as follows:

“Not while/because my father gave (it) to me or while/because my mother gave it to me.”

I consider the sentences as a kind of afterthought or add-ons to what precedes. What precedes is elided.

My translation and Loprieno’s seem to be quite different. This is not the place to enter into a detailed discussion. Still, one implication of my position is that Loprieno’s English translation does not match the Egyptian original. An obvious question arises: How would speakers of Middle Egyptian express Loprieno’s English translation?

It is not easy to find evidence in any of the standard works of Egyptian grammar if anywhere. But in fact, sufficient evidence has been gathered many years ago by Mordechai Gilula in his Hebrew University doctoral dissertation of 1968\(^\text{20}\). It should be noted that this dissertation is unpublished and written in Hebrew. It so happens that I own a photocopy of this dissertation and I read Hebrew (Jewish). But I wonder how often this evidence is accessed. And yet, it had never been gathered before and has never been since.

In any event, the evidence gathered by Gilula leaves no doubt in my mind that speakers of Middle Egyptian would have rendered Loprieno’s translation as follows:


\[\text{In introduces a so-called “participial statement,” which is for all practical purposes a cleft sentence.}\]

This type of cleft sentence can only emphasize the actor. There is another way of constructing the cleft sentence, involving the “copula” \(\text{pw}\). It can emphasize any noun or noun phrase performing any function in a sentence. The only collection of evidence for this type is Polotsky’s in his \textit{Transpositions} \(^\text{21}\). Evidence had never been gathered systematically before and has never been since.

The affirmative equivalent of Loprieno’s translation is as follows:


I have not been able to locate negated examples. One perhaps expects:


The second pronouncement by Vernus about Loprieno’s “brilliant thesis” that took me by surprise is that he deems it “seductive,” but only in “a generative approach” (I, p. 290).

I have already expressed doubt as to whether the thesis is in fact Loprieno’s. Evidently, how could he have formulated the thesis in a generative approach if he did not formulate it in the first place? Anyhow, it is not clear to me what is particularly generative about postulating ellipsis. Ellipsis is a verifiable empirical phenomenon that exists independently of any linguistic theory. Consider the elliptical sentence “Not yesterday” in “He came today. Not yesterday.” I cannot discern anything generative about the omission of “He did (not) come” in “Not yesterday.” It is not necessary to know anything about linguistic theory to recognize ellipsis for what it is.

My third surprise was that Vernus completely rejects what he himself calls a “brilliant hypothesis,” just like that. The question arises: How can a hypothesis that is wrong be brilliant? So what does Vernus replace it with? Something more than brilliant? In all brevity, he proposes that n-js denotes “restriction” (p. 298). It marks exceptions to something stated before.

In fact, I agree with Vernus’s proposal, but to a degree. I believe that translating n-js into English as a marker of exceptions is altogether suitable. The important question, to me, is: By what syntactic mechanism did n-js mark something that can be styled in English translation as an exception? The mechanism is most obviously that of an elliptic emphatic construction, so it seems to me. What Vernus calls the “fundamental semantics of the negation n-js” seems to me more a like feature of translation than a property of the original Egyptian.

This is where Vernus’s article takes a most unusual turn. One might consider this twist the main characteristic of his article. Vernus rejects the association of n-js with so-called


\(^{20}\) Mordechai Gilula, \textit{Enclitic particles in Middle Egyptian} (in Hebrew), diss. Hebrew University of Jerusalem, 1968, 60-64.

emphatic constructions. He may be the only one to ever do so. I have to confess: nothing seems more self-evident to me than the close association of n-ju with contrastive emphasis and therefore with emphatic constructions.

The element that follows n-ju is no doubt emphasized in the exact same way as the element that follows n-ju. This identity seems lost in Vernus’s analysis.

But Vernus takes matters a step further.

6.4. Obsessive Maniacs on the Loose in Egyptian Linguistics?

Vernus is more than happy to uncouple n-ju from emphatic constructions because they, “at a certain time, weighed on Egyptian linguistics in the manner of an obsessive mania (à une certaine époque, pesaient sur la linguistique égyptienne à la manière d’une manie obsessionnelle).” Is Vernus trying to save the field from obsessive mania?

It is a fact that, in everyday conversation in all languages of the world, contrastive emphasis is used in almost every other sentence, and almost always in questions for specification and their answers. Contrastive emphasis is everywhere. Get used to it. But in most languages of the world, it is almost always expressed by pitch and not by morphology or syntax and therefore does not show up in writing. In general, the awareness of contrastive emphasis as a distinct facet of the human condition is almost close to zero. There is probably no phenomenon of human intelligence that is so omnipresent yet so little recognized.

Egyptian is truly remarkable (and less so French, Vernus’s mother tongue) in that it expresses emphasis almost always by means of morphology and syntax. Emphasis therefore shows up everywhere in speech. It would also in writing — and in fact does so in Late Egyptian, Demotic, and Coptic. But the lack of vowels in hieroglyphic writing hides it to a considerable extent from empirical observation in Old and Middle Egyptian.

To the student of many ancient languages like Latin and Greek, emphasis means very little. To the student of Egyptian, it means everything. The unconscious reaction may be: If I can speak English or French or German and study Latin or Greek or Hebrew without ever hearing a peep about emphasis, then why suddenly this onslaught of emphasis when I turn to Egyptian? What on earth is wrong with Egyptian grammar? Such a reaction would be altogether natural.

In the history of Egyptian grammar, emphatic constructions were completely unrecognized or misunderstood until the later 1930s and 1940s. Suddenly, something that had been absolutely nowhere was suddenly everywhere. What is more, emphatic constructions are not the easiest of grammatical topics and are to this day to a great extent still either poorly understood or entirely misunderstood. They may well constitute Egyptian and Coptic grammar’s most difficult topic. And it does not help that one cannot see half of them in Old and Middle Egyptian.

The result was not a little bit of intellectual turmoil in the 1960s, 1970s, 1980s, and beyond, especially since the author of the most used grammar of Middle Egyptian, Alan H. Gardner, rejected much of the basic tenets of the theory that explains emphatic constructions. It seemed all quite overwhelming to many if not most. What was the result? Struggle? Yes. Frustration? Definitely. Strife? Clearly some of that. Despair? Maybe on occasion. But obsession? Not as far as I can see. Manic behavior weighing down the field of Egyptian grammar? God forbid.

Then again, can we now perhaps document an acute allergic reaction to emphatic constructions? It looks like it to me. Let us hope for a speedy recovery because, as far as I am concerned, I am only getting started. I can’t wait to keep pushing my physical and mathematical and digital and Boolean explanation of emphasis. Stay tuned.

7. The Rest of the Section

“Language and Writing” (pp. 39-331)

There are 14 articles in the section “Language and Writing” in addition to the two articles authored by Allen and Vernus, for a total of 16. I discussed the latter two articles at greater length in part because they concern topics that both the honoree and myself had written about. It seemed like an opportunity to engage the honoree’s intellectual legacy.

The 14 articles also deal with language and thought in one way or another, except the one by Ghislaine Widmer (f, pp. 301-309), which concerns the remarkable phenomenon of Demotic texts inscribed on animal bones. Remarks on the other 13 articles follow below.

7.1. The Beginning of, and Evolution of, Rational Human Intelligence

Giorgio Buccellati writes cogently about when and how the human brain acquired reason in “The Transcendental Revolution” (1, pp. 47-54). He feels (p. 49) that it needs to be dated to about 60,000 years ago. Maybe.

He also prominently cites (p. 49, note 3) Noam Chomsky’s opinion that the event was “effectively instantaneous, in a single individual, who was instantly endowed with intellectual capacities far superior to those of others, transmitted to offspring and coming to predominate.” Maybe. But maybe not.

What empirical evidence is there for the evolution of rational human intelligence before 3000 BCE? Effectively none, as far as I know.

To the Egyptologist and the Assyriologist, the earliest written records evidence human intelligence more or less as it still is today. Remarkable. Was there no change whatsoever? Did human beings get smarter in the last 5000 years? It is difficult to put one’s finger on any positive evidence for change in the constitution of rational human intelligence.

Then again, I pointed to one distinct phenomenon that may evidence such change — and I am not aware that anyone else ever did something similar). The phenomenon in question is the complete loss of the so-called balanced sentence (Wechselsatz) after Middle Egyptian and its replacement by clauses introduced by Old and Middle Egyptian jr “when” and its successors. There are those that claim to have spotted balanced sentences after Middle Egyptian. Strange. I have personally never seen any.

In addition, to me personally, one of the most important changes of the last 5,000 years in human intelligence is how the brain thinks of itself more and more as an individual, culminating in (or descending into) the contemporary

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incessant navel-gazing and total self-absorption. One might call it the rise of “Me, me, me!” One wonders whether, when this historical process is complete, human intelligence will forever have lost any awareness of The Other and be perpetually glued to a smart phone screen.

7.2. Scratch Marks in Early Dynastic Egypt

Eva-Maria Engel’s “Writing or Marking (Schrift oder Marke)” (I, pp. 55-70) presents an ambitious new approach to a peculiar and very distinctive set of “scratch marks (Ritzmarke)” inscribed on various surfaces in Egypt’s Early Dynastic period. Engel counts about 8000 instances from about 20 sites involving about 150 signs. The question has always been: How should one read these signs? They are definitely not your regular hieroglyphic writing even as they seem intimately related to it. So how to read them? Readers may find Engel’s proposals innovative.

7.3. Non-Canonical and Canonical Agreement in Indo-European

Silvia Luraghi’s “From Non-Canonical to Canonical Agreement” (I, pp. 71-88) is all about agreement, as between an adjective and the noun that it accompanies, across many many Indo-European languages. The scope is breathtaking. I was looking for a reflection of my own impression, inspired by such Egyptologists as H.J. Polotsky and Batisscom Gunn, that a substantive denotes one thing where the adjective denotes two things. But I could not readily find it. For example, Latin *vir* means “man, male”; it refers to an entity and only to an entity. But *bonus* can by itself mean “good one” refers to two entities, both an entity (“one”) and a property (“good”). Therefore, in *vir bonus* “good man,” the agreement of *bonus* with *vir* in gender is number is a function of one of the two things that an adjective denotes. The author follows G. Corbett’s definition of “canonicity” and “non-canonicity.” I have not had the time to immerse myself into how Corbett precisely understands this distinction. But it seems worth exploring.

7.4. On the Sharp Boundary between Property and Circumstance (in spite of a Certain Loss of Sharpness in Boundary between Adjective and Adverb in Coptic)

Much of Matthias Müller’s contribution entitled “Empiric and Construing Categories: Fuzzy Boundaries and Fuzzy Categories in Egyptian-Coptic Syntax (Empirie vs. Kategoriennbildung: Fuzzy boundaries and fuzzy categories in der ägyptisch-koptischen Syntax)” concerns the detailed documentation of a phenomenon that has not received due attention in grammatical studies. It is the appearance of adverbs or adverbial phrases where one expects a noun or noun phrase that is often for all practical purposes the equivalent of an English adjective. It is important to have this record of a neglected phenomenon.

Müller’s first example (I, p. 92) is *mḥōn mfrḥ* ṭnɔyɔɔwɔr ənɔk “Am I like a dog, me?” (jinkim dots belonging at ʌ, m, h, and ʌ have been omitted) (1 Samuel 17:43). The sentence exhibits the pattern of the nominal sentence, in which the second component is a rule a noun or noun phrase. Then what does an adverbial phrase like *mfrḥ* ṭnɔyɔɔwɔr do in a position in which one expects a nominal expression? And what is the difference with expressing the same thought simply by means of a nominal sentence, as follows: *mḥ ənɔk ɔyɔɔwɔr* “Am I a dog, me?”

Müller’s carefully considered analysis implies a loss of the sharp boundary between adverbs and adverbial phrases, on the one hand, and nouns and adjectives, on the other hand. One assumes that the study was selected for inclusion in Fs. Loprieno because its topic concerns boundaries, and more specifically a fuzzy boundary.

My explanation of the phenomenon differs from Müller’s. I recognize that, when an adverb can all of a sudden start appearing where an adjectival expression is normally found, some kind of boundary between adverb and adjective has become fuzzy. Then again, adverbs and adjectives exist in order to denote two fundamental facets of the human condition. When it comes to these two fundamental facets, I have the impression that the boundary between them is as sharp as ever. What are these two facets?

My point of departure is the question: What is the difference between adjectives and adverbs or adverbial phrases? I want to keep it simple — not simplistic.

What is the difference between “The car is blue,” featuring an adjective, and “The car is in the street,” featuring an adverbial phrase? The difference is that an adjective denotes what I call a property and the adverb denotes what I call a circumstance. The difference between property and circumstance occupies a central place in my textbook of Middle Egyptian2). A property is a characteristic that remains typically attached to an entity whereas an entity can easily change its circumstances. In short, the car typically remains blue. But it can easily change its location — from the street to, say, in the garage.

The difference can also be exemplified by means of the English sentences “I am good” and “I am well.” The former expresses something that is typically perceived as permanent. The latter expresses something that may change. I should add that in colloquial Amercian, it is possible to use the expression “I am good” in the sense of “I am well.” But I have the impression that one has to pronounce “I am good” in a certain way to endow it with more or less the meaning of “I am well.”

I therefore propose that, when an adverbial phrase is used in Coptic where one might find an adjective, the circumstance is presented as a property, as something that is viewed as more permanently associated with an entity. In that regard, the boundary between property and circumstance has not become fuzzier. It is as sharp as ever. But the boundary between adjectives and nouns, on the one hand, and adverbs, on the other hand, as word types is less sharp. The reason is, as Müller suggests, that adverbs or adverbial phrase can sometimes express something that is an enduring property.

So when an adverb is used where an adjectival expression is normally expected in the example cited above, “Am I a dog, me?”, then “being like a dog” is in my opinion presented as something more enduring and permanent. Once the boundary between adjectives and adverbs becomes just a little less sharp, the sharp boundary between property and circumstance is not lost.

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But what differentiates properties from circumstances is in such cases no longer word classes such as adjectives and adverbs but rather the syntactic constructions of the nominal sentence and the adverbial sentence.

It is worthy of note that the phenomenon studied by Müller is adverbs being used like adjectives and not adjectives used like adverbs. It seems natural to assume that a temporary circumstance more readily turns into a more enduring property than an enduring property turning into something that is more passing.

7.5. Verbs and Prepositions in Chinese

Fabrizio Angelo Pennachietti, who counts Antonio Loprieno among his first students in the 1970s, is a Semitist who took up the study of Chinese and writes about it in an article entitled “The Fuzzy Boundary between Verb and Preposition” (I, pp. 119-129). The tiny amount of Chinese that I studied does not permit me to comment on this article knowledgeable, even if I did commission a Chinese translation, for publication, of parts of my recent book, *Prolegomena to the Complete Physical and Mathematical Theory of Rational Human Intelligence* (2015). China is on the rise.

7.6. Egyptologese

Carsten Peust’s article (I, pp. 131-148) deals with the modern classroom pronunciation of ancient Egyptian. Peust calls it Egyptologese and this term also serves as the title of his article.

There is much about the pronunciation of ancient Egyptian that we do not know — especially, but not only, the vowels. Ancient Egyptian hieroglyphic writing typically does not represent vowels for the most part and the exact pronunciation of certain consonants is controversial. As a result, the modern classroom pronunciation of Egyptian is to a large extent artificial. It is difficult to know to what extent that pronunciation approximates the actual ancient pronunciation.

All this also means that ancient Egyptian is pronounced differently in different countries and by different people in modern times. The differences seem so random and arbitrary and have nothing to do with any factual reality that one wonders what the point is of presenting a scientific description of something that is an artificial construct.

Egyptologese is a topic in which I have myself taken a certain interest. Many, many grammars have been written of Middle Egyptian. But mine is the only one, so it seems to me, that provides an Egyptologese equivalent in the answer key to the first reading exercises, at pp. 579-584.28) I also discuss Egyptologese at pp. 23-24. Peust’s article deals mainly with the Egyptologese of France, Germany, and Great Britain. It means that the artificial pronunciation of ancient Egyptian in the United States is beyond the purview of the article.

Peust insists very much on cataloguing Egyptologese as a living and breathing entity that evolves almost like a natural language. There is indeed much variation in Egyptologese across nations and individuals. And it seems to evolve over time in certain respects. The existence of the empirical phenomenon is undeniable. Everything empirical would seem to be worthy of scientific description.

Then again, the primary task of the Egyptologist — professional task, I might add — is to understand ancient Egypt on its own terms. There is no doubt that the study of the peculiarities of Egyptologese — if considered independent from the study of how Egyptian was actually pronounced — involves the description of an authentic empirical phenomenon. However, there is also no doubt that the study of Egyptologese contributes little or nothing to a better understanding of ancient Egyptian itself. Indeed, to the extent that Egyptologese is distinctive from how Egyptian was actually pronounced, it is a necessary distortion. What is more, the mere fact that different Egyptologists pronounce ancient Egyptian differently is itself a kind of distortion because one likes to think that Egyptian words were in any one place and at any one time presumably pronounced in one single way and not two or more different ways.

Does Egyptologese then in any way matter at all to the greater cause of Egyptology as an academic field? Does it really matter that one Egyptologist pronounces ḫb “rise (said of the sun)” as ḫb-en and another Egyptologist as ḫb-wn? There is no doubt that the study of Egyptologese is peripheral to the cause of Egyptology as an academic field. But should it be completely dismissed as utterly irrelevant? A different way of putting the question is as follows: Does the professional student of ancient Egyptian suffer any deficit in understanding the language by completely disregarding the analysis of Egyptologese? The answer would appear to be: No.

In spite of all of this, dismissing the study of Egyptologese as irrelevant seems like going too far. The history of Egyptologese exhibits all kinds of ties with the history of the study of the ancient Egyptian language and of ancient Egypt itself. And as Goethe said (if he said it), the history of a science is the science itself. Egyptologese is too deeply embedded into what it means to be an Egyptologist to be totally undeserving of any attention in its own right.

7.7. Foundations of the Analysis of Hieroglyphic Writing

The next contribution in volume I, Stéphane Polis’s and Serge Rosmorduc’s “The Hieroglyphic Sign Functions: Suggestions for a Revised Taxonomy,” much excited my interest. The article concerns the classification of the function of hieroglyphic signs and contains many insights on numerous points. No one doubts that there are different types of hieroglyphs. Then again, as I emphasized in my own afore-mentioned grammar, at p. 51, there are strictly speaking only different types of functions of hieroglyphs and not different types of functions because a single hieroglyph can often have more than one function. In fact, it is not uncommon for a single hieroglyph to have all possible functions.

I was also reminded by the article at hand that I had written six times on the subject of hieroglyphic writing29). In


these articles, and in my grammar, I proposed a theory of how hieroglyphic writing should be analyzed and how hieroglyphs should be classified. No one was more influential in my thinking than Ferdinand de Saussure. Many histories of linguistics characterize de Saussure as its founder. Yet, I have the impression that his legacy seems to have vanished from linguistic practice. Who still attaches any importance to the “linguistic sign (signe linguistique)?” And yet, it is — I believe — the cornerstone of the analysis of human language. I also have the impression that, since my earlier articles, I have been able to root the analysis of hieroglyphic writing and of the linguistic sign in a physical and mathematical theory of rational human intelligence (see above).

So how does my own analysis compare to that of Polis and Rosmorduc? The two exhibit differences. I distinguish the classical three types. Polis and Rosmorduc distinguish six types. I only address the most fundamental difference here. My analysis is squarely based on Saussure’s linguistic sign, theirs is not. Then again, no one else’s is either. I am not sure whether my desire that the linguistic sign become widely accepted as the foundation of the analysis of hieroglyphic writing will ever be realized. In my opinion, it is the only positive scientific foundation possible. And I seem to be all alone in defending it and my confidence in it has only grown stronger over the years.

The authors are trying to pay homage to all that has been written about hieroglyphic writing earlier. This is admirable. Being cognizant of all that has been done on a subject demonstrates professionalism. But I have also known many an occasion on which large amounts of citations prevent a line of argument from moving forward from first principles in clear and distinct steps towards a well-defined point, a quod est demonstrandum, aduding what has been said by others earlier exclusively and only in subservience to the line of argument, a bit as in an Euclidean proof. Actually, is there any other kind of viable proof? There is a certain danger of everyone citing everyone else in an endless game of footnote diplomacy. But maybe this is what committees approving grants and promotions want to see.

7.8. More on the Old and Middle Egyptian Passive by Reintges

The contribution by Chris Reintges entitled “The Early Egyptian $sḏm.w$ of Passive Revisited” (I, pp. 175-226), is quite long. The term “revisited” is to the point because Reintges has written much about the passive in Old and Middle Egyptian.

In the study of Egyptian grammar, there has been a sharp divide in the last couple of decades between what might be called linguistic grammarians and what might be called philological grammarians. Most grammatical studies can be assigned to one group or the other. Just take those contributors to the present Festschrift whose contributions are grammatical. Squarly belonging in the philological camp are the following: James Allen, Matthias Müller, Carsten Peust, Helmut Satzinger, Pascal Vernus, and Jean Winand. The contribution by Stéphane Polis and Serge Rosmorduc on script is mostly philological though Polis by himself often tends to be much more linguistic. The contribution by Ulijas does not cite linguistics. But Ulijas often succeeds in being quite theoretical in the linguistic vein without needing to cite linguistics.

Antonio Loprieno himself belongs more or less in the linguistic camp. In that regard, there are two contributions that are linguistic in the strict sense: the one by Andreas Stauder and even more so the one by Reintges. Some of the work by Reintges and Stauder seems to me to be even hyperlinguistic.

Rigid philologists will always look in grammatical studies for new facts that deepen our understanding of what the ancient Egyptians are trying to tell us. They will typically not find linguistic studies all that helpful in this regard. Linguistic studies are more aimed at understanding language better as a phenomenon in general.

The present article is based on two unusual premises:
1) the honoree will agree with almost none of what Reintges writes (p. 174);
2) the Egyptian passive $sḏm.f$ is somehow historically and genetically related to the Semitic passive.

As regards 1), I am aware that debate may be futile. But in the end, the truth still has to count for something. Disagreement should not be pursued for its own sake. Wherever two disagree on scientific facts, one of the two must be wrong. Pursuing disagreement for its own sake therefore sounds a little like purposefully pursuing falsehood.

Because of all the uncertainties affecting the analysis of the Old and Middle Egyptian verbal system (largely because vowels are not written), there has been an unusually large amount of writing on the passive in recent times. There is no denying that one can say an enormous number of things about the passive in Old and Middle Egyptian without risking positive falsification, simply because hieroglyphic writing obscures so much of the possible.

As regards 2), no one in the history of Egyptian linguistics has ever stated that any Egyptian passive $sḏm.f$, or any $sḏm.f$ for that matter, is related historically to Semitic verb forms, though outlier theories may have escaped my attention.

7.9. Arabic Verbs Borrowed into Coptic

In his contribution “On the Fringes of Egyptian Language and Linguistics” (I, pp. 227-242), Tonio Sebastian Richter documents for the first time a phenomenon that seems to have gone entirely unnoticed in the study of the Coptic language, at least as far as the published record is concerned. Remarkable. It is the borrowing of Arabic verbs into Coptic. The borrowing of Arabic nouns into Coptic has been well documented over the decades. But verbs? There is not a peep about it anywhere in the literature. How can this be? The reason is that most borrowings of verbs occur in texts that remain largely unpublished, namely alchemic texts. Richter is working on systematically gathering and editing these texts. They typically date to about the tenth and eleventh centuries C.E. This is virgin territory and Richter has set out to conquer it. His results are eagerly awaited.

One question that Richter does not address is as follows: Why are verbs not as obviously borrowed as nouns into Coptic? The phenomenon does not seem to be limited to Coptic.
Part of the answer may be as follows. Nouns are designations of things or entities and verbs are descriptions of events.

Consider visiting another culture. It would be easy to spot, so it seems to me, things or entities that one culture has but another does not. It would be impossible to refer to these things without the names that they bear in that other culture. The only alternative would be to invent new names.

By contrast, events for the most part concern what happens to human bodies and what the human body does. Human bodies are the same everywhere and there is no way in which human bodies could behave or be affected by events much differently from one another in different cultures. An exception would be endeavors involving highly specialized techniques and actions, such as for example alchemy. Such endeavors would involve actions that are uniquely specific to that endeavor. Such actions would therefore become associated with verbs as technical terms, verbs used in a very narrow sense to denote a very specific action. If one culture adopts such an endeavor from another culture, the temptation would be to adopt a technical term along with it.

7.10. The Most Remarkable Property of Verbal Conjugation in Egyptian throughout Its History

7.10.1 Totally Dominant and Totally(?) Unique among the World’s Languages

In his contribution entitled “These Strange, Exotic Egyptian Verbal Formations” (I, pp. 243–255), Helmut Satzinger studies an obvious yet truly remarkable property of those Egyptian verbal conjugations in which suffix pronouns are used when the entity denoted by the verbal conjugation is strictly pronominal as opposed to nominal. In English “He runs,” the entity in question is pronominal; in “The man runs,” nominal.

The conjugations in question are traditionally called suffix conjugations. Satzinger (p. 243) objects to the term. I do not fully comprehend why. One might call them “suffix pronominal conjugations” instead. This term seems descriptively 100% accurate. It is then a small step to abbreviate the term to “suffix conjugations.” I do not see a problem.

(I personally prefer to call change according to person, gender, and number “conjugation” and change according to gender and number “declension.” In conjugation, nouns and noun phrases are associated with the third person.)

When I was Polotsky’s student, he used to direct my attention to this remarkable property. And I do take care to point it out to my own students when the occasion arises. The property is so obvious as to be easily overlooked. It is a good thing that Satzinger finally paid some attention to it.

There are three things that I was looking for in Satzinger’s article but could not find either sufficiently articulated or even at all present.

The first thing that I miss is a clearer acknowledgment of the vast scope of the suffix conjugation as a phenomenon of the Egyptian language. In earlier Egyptian, up to sometime in the Late Egyptian stage, most conjugations are suffix conjugations. The only exception is the stative. From then on, all the Egyptian conjugations are suffix conjugations except the bipartite. Because the suffix conjugation is so absolutely dominant throughout Egyptian, so is the specific property of it studied by Satzinger.

The second matter that I miss is a clearer acknowledgment of the uniqueness of the property to Egyptian as supposed to other languages. Satzinger is unable to document the property in any other languages. And he has investigated quite a few of them. This makes the property so far unique to Egyptian, until a language can be found that behaves like Egyptian. Satzinger uses the terms “strange” and “exotic” in the title of his article to describe Egyptian.

The first two matters together make for a striking coincidence of two facts regarding a certain property. The property is not only totally dominant in Egyptian. But it is also absolutely unique to Egyptian. These two facts combined may well make the property in question into the most striking and prominent characteristic of Egyptian as compared to the world’s other languages.

The third matter is that Satzinger does not try to explain why Egyptian behaves in this completely unique way among the world’s languages.

But what is this property in question?

7.10.2. Definition of the Property

The property in question is that any third person markers of pronominal conjugation are completely absent when the entity denoted by conjugation is nominal. One might say that the third person pronominal element is entirely replaced by the noun or noun phrase. Naturally, by the same token, one might say that the noun or noun phrase is entirely replaced by the pronominal element. In other words, either one or the other appears but never both at the same time. Perhaps, a happy way to express the fact is that the pronominal element alternates with a noun or noun phrase.

This is clearly not the case in most conjugations in other languages. Just consider English “He runs.” One marker of the third person singular is the ending s in “runs.” If the entity denoted by conjugation is nominal, for example, “the man,” this marker does not vanish in “The man runs.” And yet, there is strictly speaking no need for it. “The man run” would be fully comprehensible. The s serves no practical function. There seems to be no need for it. The result is a certain redundancy.

All this may seem rather self-evident. But nothing anywhere close to this is found in all of Egyptian and Coptic outside of the stative conjugation of earlier Egyptian. Egyptian is efficient to an extremely high degree, as it were. Satzinger calls Egyptian “strange” in this regard. But in some way, it is Egyptian that is normal and all the other languages that are strange.

Satzinger compares the suffix conjugation of Egyptian with verbal conjugation in many, many languages. His elaborate comparison makes it clear how very remarkable Egyptian verbal conjugation is. But what Satzinger does not do is ask, let alone try to answer, why the Egyptian language behaves the way in which it does. What follows is designed to provide some kind of an answer.

One likes to think that everything happens for a reason. The question arises: What caused Egyptian to exhibit the property defined above in such abundance and to even have it dominate 100% of its verbal system from sometime in Late Egyptian onward?

7.10.3. Explaining the Property by means of Explaining a Related Property

In order to explain the property at hand, it is not in the least necessary to try to explain the property itself. It is rather necessary to explain a closely related property. The closely related property automatically implies the property itself. If
one proves the closely related property, then one proves the property itself.

What is the closely related property?

The closely related property is that, in the Egyptian suffix conjugations, the pronoun is still fully interpreted, or “felt,” as a pronoun. The same cannot be said about the ending s in English “runs.”

What proves the fact that the marker of person, gender, and number in suffix conjugations is 100% a pronoun? It is a fact that, everywhere else in Egyptian itself and in all languages of the world, a third person pronoun alternates with a noun or noun phrase always and only when it fully functions as a pronoun. Then why would Egyptian suffix conjugations be any different?

It follows that, if one can explain the related property, then one has explained the property investigated by Satzinger.

But what is it exactly that one is trying to explain?

7.10.4. Asking the Right Question

The question is not: Why do the suffix pronouns marking verbal conjugation behave like pronouns? Pronouns obviously behave like pronouns. What else would they behave like? The question is rather: Why did the suffix pronouns in question not cease behaving like suffix pronouns and behave instead like the ending s in “runs”? There is no doubt that, when the suffix conjugation came into existence, the suffix pronouns marking conjugation at the time behaved like pronouns. But what did they retain this property throughout Egyptian history?

But first something more on the circumstances in which the Egyptian suffix conjugations came about.

7.10.5. The Passive Participle Theory of the Origin of the Suffix Conjugation

For many decades, the passive participle theory was by far the most popular theory about the origin of the suffix conjugations. The eminent Egyptologists Kurt Sethe and Alan H. Gardiner were its principal creators and promoters. According to this theory, there are two ways in which suffix conjugations derive from passive participles: 1) by direct attachment to the participle; 2) by mediation of the preposition n.

Now, there is no doubt that not every suffix conjugation can be derived smoothly from passive participles. The result has been a startling development in recent decades. Just about every grammian who has made a pronouncement on the issue has abandoned the passive participle theory. I somehow found myself in the position to be the only one still willing to defend it. And I did so, at some considerable length.

I pointed to the overwhelming presence of analogical formations in the evolution of languages. Analogical formations are in origin in a certain sense “mistakes.” There are just a number of arguments that make the passive participle theory by far the one that makes the most sense. How else can one possibly explain the “cornerstone” (Poliotsky) of the passive participle theory, namely that “what he likes is” is rendered by mret.f and not by *mret.s?

To me, the more recent abandonment of the passive participle theory is a showcase of throwing out the baby with the bathwater. I have personally no doubt that the passive participle theory is the correct theory.

7.10.6. The Two Original Usages of the Suffix Personal Pronouns

If one disregards verbal conjugation, what are the prominent usages of the suffix pronouns? There are two — only two in fact — but they are very prominent and omnipresent. One involves suffix pronouns following nouns or nominal expressions denoting possession or the like, as in pr.f “his house.” The other involves suffix pronouns following prepositions, as in n.f “for him.”

According to the passive participle theory, both these usages played a role in the formation of the suffix conjugation. Put differently, all usages of the suffix conjugation played a role in the formation of the suffix conjugation.

In fact, those who reject the passive participle theory probably also mostly assume that the same two usages of the suffix pronoun played a role in the formation of the suffix conjugation.

This same theory also implies that the use of the suffix pronouns to denote verbal conjugation is clearly secondary in the history of the suffix pronouns. In the beginning, there were the usages following nouns and prepositions. In fact, it may well be that prepositions derive from nouns. It seems to me quite probable that the preposition hr “on” derives from the noun hr “face.” What is more characteristic of the face in relation to the human body than the fact that it is on it? Presumably, in the beginning, the desire of human beings was to name things. Prepositions, one assumes, came about later.

In an Afroasian context, Egyptian shares the static conjugation with Semitic. The static conjugation must therefore belong to the common prehistoric language from which both Egyptian and Semitic derive. The so-called prefix conjugation must also belong to a common Afroasian ancestor because Semitic shares it with Berber. But there is hardly a trace of the suffix conjugation in other Afroasian languages outside Egyptian. There is something similar in Ethiopic, as pointed out by Satzinger. But this Ethiopic conjugation is clearly not genetically related to the Egyptian suffix conjugation. Most everyone therefore accepts that the Egyptian suffix conjugations are unique to Egyptian.

7.10.7. Historical Change and Causality: The Impossibility of Identifying Definite Causes

The principal design of the present section is to make clear my personal conviction that it is impossible to discern absolute and definite causes in historical processes. And that includes the history of languages. And one example of a historical linguistic process is the way in which the suffix pronouns retained their pronominal status throughout the history of the Egyptian language when marking verbal conjugation.

The best that one can do is to identify circumstances that seem ideally placed to trigger a certain historical process. But these circumstances derive their principal strength in negative fashion, as follows: if they had not been in place, then the process would not have taken place.

The key question at hand, then, is as follows: What prevented suffix pronouns in the suffix conjugation from no longer being treated as full-fledged pronouns but instead as verbal conjugational endings?

One likes to think that everything happens for a reason. The search is therefore for causes. But in historical processes, in my experience, it is mostly impossible to establish that one event directly caused another event. There is
always the possibility that the other event might not have occurred. I will evoke the impenetrable complexity of causality below by briefly digressing with a remark on chaos theory.

The false impression that causation is possible is derived from the fact that people and things do things to other people and things all the time, as "The baker baked the bread." This can be restated as "The baker baked the dough. The dough is baked." The first event may be considered the cause of the second event. But that is somewhat deceptive because only a single event is concerned. The two sentences denote two facets of the same event and not really one event causing another event.

But what about two events? Consider Saddam Hussein's invasion of Kuwait and the First Persian Gulf War, clearly two distinct events. Did the invasion cause the war? It might not have. And in other cases, an invasion may not trigger a war. But one thing is certain. There would have been no Persian Gulf war at that time if there had been no invasion. In other words, one cannot with certainty establish that the occurrence of one event must cause the occurrence of another event by some kind of absolute necessity, one can establish that the non-occurrence of one event would have caused another if one may call it that, the non-occurrence of another event.

But it is difficult to think of causation as something negative. Is there a way of stating in a positive way what has just been defined negatively? Let us again consider the invasion and the gulf war. One may be overwhelmed by the impression that it is difficult to see what made the war happen, even if the invasion cannot produce a war with mathematical necessity. But reality is so complex that it is impossible to comprehend it all at once. The best that one can do is to conclude that the invasion must have made some kind of a contribution. Because if there had been no invasion, there would have been no war. In that sense, the invasion with high probability played some kind of a role.

Then why do invasions sometimes produce wars and sometimes not? Why does the suffix conjugation remain directly and tightly associated with suffix pronouns throughout Egyptian history? The best answer that I can provide is that something else must have been present as well in addition to some obvious empirical conditions-without-which-not. But that something else is not retrievable. That is why identifying seamless full-fledged causality is impossible. It is as impossible as perfect weather prediction, which is a complete fantasy. What is that something else? Chaos theory provides an answer.

7.10.8. Chaos Theory and the Impossibility of Predicting an Event

Chaos theory shows that it is impossible to point to definite causes, events that automatically produce another event without fail, in human history. One of the tenets of chaos theory is that the smallest event can have the biggest consequences.

Chaos theory started with the computerized study of the weather and was pioneered by the meteorologist Edward N. Lorenz at MIT. The central idea is best exemplified by his 1972 world famous talk entitled "Predictability: Does the Flap of a Butterfly’s Wings in Brazil Set Off a Tornado in Texas?" It seems as if the possibility of a positive answer to the question would only invite disbelief. Yet, there is a way of making it much, much more apparent that such a tiny event could have such a big consequence.

A good example used by chaos theorists to convince the public at large about the validity of the theory concerns one’s own existence. What caused me to exist? Clearly, I would not exist if my parents had never met. But how did my parents get to know one another in the first place? They had to meet for the first time at a certain place and at a certain time. In that regard, it is impossible to imagine the infinity of circumstances that needed to take place to make the meeting possible.

One simple scenario amply suffices to drive home this point. Just assume that a tiny corrosion in a carburetor made a car break down and thus prevent a certain woman to go to a certain meeting and instead end up in a place where she first met your father. A simple nail in a car tire might also do the job. If such tiny events had not occurred, you would not exist.

The question arises: How many of these tiny events need to be in place for an event to occur? It would seem that they are uncountable and also for the most part irretrievable. That is also why, again, perfect weather prediction is a phantasm. And that is also why a full account of why the suffix conjugation remained associated throughout Egyptian history with the suffix pronouns is impossible.

7.10.9. Explanation of the Related Property and Partial Causality

How is what has been said above relevant to the present query? In my impression, establishing causation in language change, as in any historical change, in the sense of one event causing another event with mathematical necessity, is impossible. It would in fact amount to predicting the future.

It would amount to perfectly predicting the weather (which is, as Lorenz first abundantly made clear, pure fantasy). Predicting is assessing a set of circumstances and concluding that this set of circumstances will unavoidably and definitely lead to a certain event. That is simply humanly impossible.

The best that one can do is to detect circumstances that must have been the case for an event to take place. It cannot be said that these circumstances alone triggered the event because they might well not have. There was something else.

Still, they did play some kind of a role.

Still, I would like to introduce a concept that I would provisionally term “partial causality.” It is based on two undeniable facts.

The first fact is that detectable circumstances without which an event cannot take place do play some kind of a role. They somehow do something because, if they were absent, nothing would happen. Their mere presence already somehow does something.

So why can they not be described as full causes? That is because, strictly speaking, (a set of) full causes by themselves ought to directly trigger an event. But they do not do so by themselves alone. Something else complements them. What else? This is where the myriads of minuscule circumstances of chaos theory described in the previous section come into play. It is not possible to retrieve these.

There is therefore a distinction between detectable circumstances and undetectable circumstances. Truly determining causality would involve detecting and knowing all the circumstances leading to an event. Chaos theory states that this is impossible.
Still, identifying detectable circumstances leads to a better understanding of the evolution of Egyptian. If there are several of them, a favorable climate must have existed for a certain development to take place.

What are some of those detectable circumstances in the present case?

First is the relatively recent coming into existence of the suffix conjugation in the history of Afroasiatic. The suffix conjugation did not exist in common Afroasiatic. It therefore came into existence after Egyptian split off. That means less of a time horizon for change to come about, in the sense of suffix pronouns moving away from their origin as pronouns. Change takes time.

Second is the remarkable confluence of four facts:
1) the new markers of conjugation were the suffix personal pronouns (it did not have to be this way);
2) the new markers of conjugation involved all the prior usages of the suffix pronouns (it did not have to be this way);
3) the suffix pronouns are abundantly present throughout the language (it did not have to be this way); and
4) the suffix pronouns remained abundantly present throughout the history of the Egyptian language (it did not have to be this way).

Facts 1), 2), and 3) must have made for a very strong association at the outset between the new markers of conjugation and a specific set of pronouns when the suffix conjugations came into existence. In fact, the new markers of conjugation are the suffix pronouns. Fact 4) does much to explain why the association was never weakened.

7.11. The Relation between the Past Participle \(dns\) (“Who/Which Has Become Heavy”), the Stative \(dns\) (“He Having Become and Hence Now Being Heavy”), and the Adjective \(dns\) (“Heavy”)

7.11.1. What is “Semantic Transitivity”? \(^{38}\)

Andreas Stauder borrows the concept called “semantic transitivity” from general linguistics and applies it to Egyptian in a study entitled “Semantic Transitivity: The Case of the Pseudo-participle and of Qualitative Predication (La transitivité sémantique en égyptien: Le cas du pseudoparticipe et de la prédication de qualité)” (I, pp. 257-276).

I think that I know what transitivity is, I know it as a property of verbs. Transitivity can be, and is usually, defined as the ability of being followed by a direct object. It is therefore postulated a need for “semantic transitivity” in addition to “syntactic transitivity.”

The question is: What is “semantic transitivity”? I am not sure that I fully, or even partially, understand to which patent empirical phenomenon the concept of “semantic transitivity” refers.

I do know that there is a difference between transitivity in “The man killed the bear” and transitivity in “The man heard the whistle.” In the first example, the role of “The man” is rather active and dynamic; in the second example, rather passive. Does this difference belong to the domain of “semantic transitivity”? If it does, then “semantic transitivity” is concerned with certain nuances pertaining to “syntactic transitivity.” But is this what Stauder means by “semantic transitivity”?

I wish that the article had presented a more accessible definition of “semantic transitivity” with simple examples.

But in any event, Stauder introduces “semantic transitivity” as an explanatory principle pertaining to his article’s main design, that is, to explain a certain type of contrast between the participle and the stative. To obtain a complete picture, it is necessary to draw in the adjective, which Stauder does not do.

As it happens, my theoretical approach to the main topic of Stauder’s article is entirely different from his. It differs so radically that I am not sure exactly how to engage his. What I propose to do, therefore, as a reviewer is to present my own approach to the problem — though including the adjective — in detail.

All that I can offer readers is putting Stauder’s approach in perspective by presenting an approach that radically differs from it. Let others judge what to make of these two approaches.

7.11.2. A Specific Contrast between Participle and Stative and the Need for an Explanation

Participles and statives can both denote complete sentences together with a personal pronoun and, in the case of the stative, additionally a particle, like the participle in \(rh\ sw\ “He knows” and the stative in \(*jw.f rh\ “He knows.”\) Other paradigmatic examples might be \(*sl p sy\, featuring a participle, and \(*jw s stp.tfj\, featuring a stative. Both can be translated as “It is loaded.”

Alan H. Gardiner describes the expression featuring the participle as “the participle as adjectival predicate” in his celebrated Egyptian Grammar, at §374.B.

Stauder’s article revolves entirely around and about the difference between the two afore-mentioned expressions. What is the difference?

Again, Stauder’s approach differs from mine. First, he relies on the principle of “semantic transitivity.” I am not entirely sure what to make of this principle and I see no need for it in my own approach.

In addition, Stauder believes that the “predication of quality,” as expressed by a participle, is “not submitted to the flow of time (pas soumise au flux temporel),” whereas the stative is. But then, this interpretation leaves unaddressed the fact that both past and present participles appear in adjectival sentences. The contrast between past and present is quite real and empirical and it has everything to do with tense or the flow of time. I do not quite comprehend how Stauder is able to eliminate tense, just like that, entirely from the picture. I believe that tense, or the “flow of time,” is very much present. Therefore, in my opinion, in order to explain the difference between the afore-mentioned usages of the participle and the stative, some concept other than tense needs to be invoked. I believe that concept to be contrast between property, as expressed by adjectives, and circumstance, as expressed by adverbs.
7.11.3. The Difference between Property and Circumstance

Many years ago, I was induced to contemplate the difference between such simple expressions as *(jw gm.n.j) dpt ẖtp “I have found the loaded boat” and *(gm.n.j) dpt ẖtp.tj “I have found the boat loaded.” It is not even necessary to focus on the Egyptian expressions. If one can just define the difference between “the loaded boat” and “the boat loaded,” one would already come close to some kind of an understanding of the corresponding difference in Egyptian.

The first step in the present argument is the key observation that “the loaded boat” and “the boat loaded” can refer to the exact same state of affairs. The reality that they describe can be the same.

What is more, in both cases, the boat has been loaded at some point in the past. It is not being loaded in the present. There is clearly a reference to tense in both cases. The expression “the loaded boat” is as little disconnected from “the flux of time” as is “the boat loaded.”

So what is the difference between “the loaded boat” and “the boat loaded”? It evidently must have something to do with how the same reality is viewed differently by two different speakers or by one and the same speaker looking at the same reality in two different ways.

In §7.4, I have already at length commented on the difference between property and circumstance. I refrain from repeating from what I wrote there. As far as I am concerned, when it comes to the contrast between “the loaded boat” and “the boat loaded,” “the loaded boat” denotes a property and “boat loaded” a circumstance. A property is typically something more permanently associated with an entity. In that regard, “the loaded boat” can be associated with “the blue boat.” A blue boat typically stays blue more or less. By contrast, “the boat loaded” can be associated with “the boat in the harbor.” Boats typically do not permanently stay in a harbor.

The contrast between property and circumstance is one of the most fundamental properties of all languages. Just witness the omnipresence of adverbs and what is adverbial and of adjectives and what is adjectival in all languages of the world.

I conclude that the participle also denotes a property when it is used as part of the adjectival sentence. In the sentence *ḥtp sy “It is loaded,” the loadedness of the boat is presented as a property, something more permanently associated with the boat, a bit like an adjective. The boat has been loaded in the past. Hence the past passive participle. Tense is very much relevant. And its being loaded is presented as a property in the same way that being blue could be a property.

By contrast, in *(dpt ẖtp.tj “the boat loaded,” the loadedness denotes a circumstance, something more transiently associated with the boat, a bit like an adverb. The boat may right now be loaded. But it may have been unloaded before and may be unloaded again.

But what about *(jw dpt ẖtp.tj “The boat is loaded” or *(jw.s ẖtp.tj “It is loaded”? In this compound verb form, the adverbial character of the stative has become obscured. The adverbial stative has merged with the auxiliary to form a main verb form.

Stauder’s article revolves entirely around the contrast between an expression such as *ḥtp sy and an expression such as *ẖtp.tj. I say: the former denotes a property and the latter does not and this is what the difference is about. But is that all that the difference is about? I am not sure. Provisionally, I propose that the participle seems closer to a dynamic passive, as in English “It has been loaded” whereas the stative seems closer to a statal passive, as in English “It is loaded.”

7.11.4. The Most Striking Characteristic of Participles in Adjectival Sentences

There is no property of participles in adjectival sentences more striking than the following. And I would say that it does much to confirm what is proposed here: that the use of participles in adjectival sentences is all about presenting a verbal action as a property.

The striking use in question is in conjunction with the preposition r to express the comparative degree, as in *(r ṣ “greater than.” Stauder does not single out the frequency of this usage for special attention. But Gardiner already noted it when he states the following in his Egyptian Grammar at §374.B: “The examples show how often the construction is used in comparisons.” The frequency of occurrence is indeed remarkable.

An additional good example occurs in the Loyalist Teaching, version of Sehetepibre, §3, lines 12-13: swḏꜢ w(y) sw r ḫ/py ’j “Oh how he makes (things) more verdant than a great flood.”

A possible example is found in the very fragmentary Middle Egyptian version of the Tale of Horus and Seth, as preserved in Lahun Papyrus UC 32158 with related fragments UC 32148B and UC 32150A. The Late Egyptian version of this tale is evidently well known. It is inscribed on one of the best preserved — if not the best preserved — ancient Egyptian papyri, the Papyrus Chester Beatty I.

The Middle Egyptian example is as follows27: mk ṣ wn nḏm sy ẖḫ.f ṣ ḫ ṭ[r ḫ[.].] “Look, it is sweeter on his heart than the height [...].” The purport is sexual in nature and pertains to the seduction of Horus by Seth.

How to make sense of ṣ wn? Is it possible that ṣ wn is a participle and nḏm is a stative and that the two together are an adjectival conversion of ḫ ṭ[r ḫ]? If so, the form ṣ wn nḏm would lend a peculiar twist to the present discussion of the difference between participle and stative. It would somehow allow the stative to become part of the denotation of a property.

But why does this striking usage support the notion that the essence of participles is to denote properties?

The striking usage involves a comparison between two entities that exhibit the same characteristic. The comparison revolves entirely around the fact that one entity exhibits the characteristic in question to a higher degree than the other entity. But in order to make a comparison possible, the two entities need to exhibit the characteristic in fairly permanent fashion. What is the point of comparing if either entity can lose the characteristic because of a change in the state of affairs? I conclude that the characteristic must be a property, that is, a feature more permanently attached to an entity.

7.11.5. The Difference between Stative, (Past Passive) Participle, and Adjective

In what precedes, a proposal has been made as to how to differentiate stative and participle, both verb forms. The participle is an adjectival verb form. But how does the adjective relate to both the stative and the participle?

Distinctions are the stuff of which knowledge is made. It seems opportune to summarize in a table how all three — stative, participle, and adjective — relate to one another in relation to two distinctions.

<table>
<thead>
<tr>
<th>VERB FORMS</th>
<th>DISTINCTION 1</th>
<th>DISTINCTION 2</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>process</td>
<td>state</td>
</tr>
<tr>
<td>stative</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>participle</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>NOT A VERB FORM</td>
<td>adjective</td>
<td>NO</td>
</tr>
</tbody>
</table>

By “Distinction 1” and “Distinction 2,” I have nothing extremely theoretical in mind. My vision of these distinctions could not be more empirical.

As regards Distinction 1, what I see in my mind’s eye is the difference between “sitting down” and “being seated.” Something is moving in the former. That is a process, for lack of a better term. All is at rest in the latter. That is a state, for lack of a better term. What is a little confusing about the term “stative” is that it denotes, not only a stative, but also — in addition — a process. It needs to be admitted that there is a slight contradiction in calling something a stative that also denotes the opposite of a state.

As regards Distinction 2, what I see in my mind’s eye is the difference between “I am good” and “I am well.”

As to the difference between stative and participle, I would assign it entirely to the contrast between property and circumstance. Both are verb forms referring to both a process and a state. Something happened in the past. But all is now at rest.

But where is the adjective positioned in relation to these two?

The adjective is definitely closer to the participle than to the stative. It is differentiated from the participle according to only one of the two criteria. But it is differentiated from the stative according to both criteria. Though not always.

Only when the stative denotes a circumstance, which it not always does.

7.11.6. The Stative and the Adjective in Earlier Egyptian

It has been noted above that the stative may or may not denote a circumstance. In the transition from Middle Egyptian to Late Egyptian, the stative for the most part lost its ability to denote a circumstance by itself. But even in Middle Egyptian, it does not always denote a circumstance. When it does not, only a single difference sets it apart from the adjective. It denotes both a process and a state whereas the adjective denotes only a state.

A simple example. Take the root *dns* “become heavy.” It is used in the stative in a Middle Egyptian expression such as *jw.f dns* “He is heavy.” It is used as an adjective in an expression such as *dns sw* “He is heavy.”

The English translations deceptively make it seem as if the two expressions do not differ in meaning. But they do so in a radical way: *dns sw* denotes a pure state; but *jw.f dns* expresses the past process that led to the state. In that regard, *dns sw* means just “He is heavy,” but *jw.f dns* denotes literally something like “He has become heavy and hence now is heavy.” The stative clearly implies the past process that led to the state.

In this connection, it may be useful to point out explicitly, for the first time ever I believe, a remarkable historical fact. It is discussed in the next section.

7.11.7. Rapprochement of the Stative to the Adjective in Later Egyptian

In later Egyptian, especially Demotic and Coptic, the stative of so-called adjective verbs often came to denote a pure state. For example, in Coptic, statives of so-called transitive verbs often translate Greek adjectives. Old and Middle Egyptian statives are never equivalent in meaning to adjectives. They always express both a past process and the state to which the process led.

I have not found this remarkable fact noted anywhere in the literature.

When the stative of earlier Egyptian and the adjective relate to one another as marked form, denoting both a process and a state, and unmarked form, denoting only a state. It therefore appears that the stative could on occasion lose its distinctive mark and become the unmarked form. The marked form was now expressed by the past or perfect tense, *sdm.f* in Demotic and *jw.f* in Coptic. That became possible when, with the advent of Demotic, intransitive verbs could be used in the past *sdm.f*, which earlier they could not. The entry of intransitive verbs into the past *sdm.f* therefore went hand in hand with the loss of reference to a prior process on the part of the stative.

I have on quite a few occasions observed how, in the meanings of words, the evolution from marked to unmarked occurs quite frequently in the history of Egyptian29). I may well be the only Egyptologist who has in the past thirty years, if not ever, attempted to observe rule-determined processes in historical semantics30). Nor do I readily find observations on such processes in the literature on general linguistics. Egyptian is the longest attested of all the world’s languages. Surely, one would think that one might learn something from it about the evolution of the meanings of words. So far, I seem to be the only one to have taken advantage of this unique circumstance.

7.12. On the Distinction between Purpose and Result, or between Final Clauses and Consecutive Clauses

Sami Uljas’s contribution revolves entirely around the difference between what he calls purpose and result. In his


terminology, result is the same as consequence. Purpose is expressed by so-called final clauses and result is expressed by so-called consecutive clauses.

Uljas posits that the notion of purpose is well understood whereas the concept of result is much less so. The quest of his article is therefore a better understanding of consecutive clauses.

The question arises: What is the difference between purpose and result? The key differentiating notion appears to be intent. And this is apparently also how Uljas understands it. A purpose has it and a result does not, it would seem.

Final clauses and consecutive clauses both refer to events that relate in a certain way to an event denoted by the main clause to which they are subordinated.

If intent is the key distinctive criterion, then final clauses denote an event that is the intended or desired outcome of the event denoted by the main clause. By contrast, the event denoted by consecutive clauses is accidental to the main event.

According to this definition, final clauses denote presence of intent and consecutive clauses denote absence of intent. It is tempting to interpret the contrast between final clauses and consecutive clauses as a contrast between marked and unmarked. If so, all purposes would also be results but not all results would be purposes. But this contrast between marked and unmarked is not suitable here. The reason is that any intent ought to be conveyed already by the main clause. The event denoted by the main clause ought to exist in harmony with the event denoted by the subordinate clause as its desired intent. Therefore, the intent is very much part of the main clause. The subordinated clause associated with a main clause denoting intent cannot be a consecutive clause denoting an accidental result. There would be an internal contradiction between intent and absence of intent.

According to the proposed analysis, all comes down to detecting either the absence or the presence of intent. In that regard, Uljas’s means of detecting one or the other seem less than perfect.

For example, he contrasts (I. p. 277) the two sentences “Harry brought the board so that Hermione might read the text on it” and (Hermione said) “Clean the board, so that it will be readable.” He assumes that “so that it will be readable” denotes a result because “so that” can be replaced by “and.” However, is this not a function of the tense? If one makes the first sentence into future tense, as in “Harry will bring the board,” then “so that” can be replaced by “and,” as in “Harry will bring the board and Hermione will read the text on it.” The events are the same but the tense differs. The difference is that a clause referring to the future can denote an intent or a purpose where a clause referring to the past cannot because one already knows what happened.

Surely, certain events involve intent and others do not. But can the difference be conveyed linguistically? I was trained as a classicist and we were taught that the Latin conjunction ut followed by the conjunctive denotes both consecutive clauses denoting consequences or what Uljas calls results and final clauses. We were also taught that ut is typically negated in consecutive clauses by ut non and in final clauses typically by ne. It is what one finds in grammars. I have, however, never been comfortable with this distinction.

To a great if not exclusive extent, I suspect that the distinction between purpose and result is a factor of modern interpretation and is not at all reflected in the linguistic expression. My suspicion is only confirmed by what I perceive to be a distinct deficit in Uljas’s article. His design is to study the expression of purpose and the expression of result. The reader might expect two lists in such an article: a list of Egyptian linguistic expressions denoting purpose and a list of Egyptian linguistic expressions denoting result. The absence of such lists gives one the impression that the distinction does not exist, for the most part, in the Egyptian language but to a great extent only in the heads of modern interpreters. Modern interpreters make the decision as to whether there is or there is not intent.

7.13. Fuzzy Boundaries, Funny Syntax

In his contribution “Fuzzy Boundaries, Funny Syntax: Some Reflections on the Progressive and Other Constructions in Late Egyptian (Quelques réflexions sur le progresif et d’ autres constructions en néo-égyptien)” (I. pp. 311-331), whose main title is in English whereas its subtitle and the article itself are in French, Jean Winand takes the metaphor-Fest to the next level by diagnosing, not only things that are fuzzy, but also things that are funny. If one could just add what is plain silly, the farce would be complete.

Winand does sensibly state (I. p. 311) that the theory of fuzzy boundaries has itself been on occasion fuzzy. Fuzziness squared, as it were. Still, he accepts that there is such a thing as fuzzy boundaries as a property of reality whereas I am much more sceptical. I reiterate my belief that there is nothing fuzzy in the least about reality. I am therefore very tempted to locate the totality of the fuzziness in the minds of the authors of the volume.

I am not sure that I have been able to follow in greatest detail every facet of Winand’s argument. I was therefore looking for its basis in fact. Winand is a specialist of Late Egyptian. I have this impression that the article revolves entirely around a paramount and startling fact of Late Egyptian, as far as it makes itself manifest at the present time. The fact concerns the difference between the general present or aorist, as in “He (generally) eats,” and the actual present, as in “He is eating (right now).” It would appear that, in Late Egyptian, in the affirmative, both are expressed by the so-called First Present, namely twj (hr) sḏm.

One thing is certain. In Old and Middle Egyptian, the aorist is expressed by jw.f sḏm.f; in Demotic and Coptic, by hr jr.f sḏm/ga].[tcm (which derives from Middle and Late Egyptian hr sḏm.f). I have documented this crucial shift as part of a larger study that postulated the existence of contingent tenses, that is, tenses that are dependent on conditions.\(^{30})\)

Up to that time, it had not been fully clear in grammatical studies that Demotic hr jr.f sḏm is the clear and unambiguous successor of Middle Egyptian jw.f sḏm.f. Since that time, I have had the opportunity to consolidate my understanding of the phenomenon of the condition in physical and mathematical terms as part of a comprehensive theory of rational human intelligence. It has only much strengthened my belief in the existence of contingent tenses in Old and Middle Egyptian and perhaps to some extent also in Late Egyptian. The question arises: How was the aorist expressed in Late Egyptian? Middle Egyptian had become obsolete and hr sḏm.f was presumably still too contingent in flavor. Accordingly, the so-called First Present came to be used instead.

\(^{30}\) Leo Depuydt, Conjunction, Contiguity, Contingency, New York, 1993, 201-255.
Is this fuzzy? I do not think so. Reality is so diverse that human language can never express it in all its diversity. The shortcomings of language are not fuzziness. What matters is to determine sharply, without any fuzziness, what language can do and cannot do when it comes to denoting the tremendous variety of reality. There is nothing fuzzy about determining these facts. Language is a tool that comes with its limitations. I believe that it is wrong to describe these limitations as fuzziness. I rather prefer to describe the limitations as razor sharp facts.

The quest for knowledge is not served by the postulation of fuzziness.

8. Conclusion

I have made an attempt to acknowledge those contributions to this Festschrift with whose topics I am more familiar with a certain level of intellectual engagement. In doing so, I have covered about a quarter to a third of the Festschrift. At the present rate, if I had had the competence to keep going, I would have needed to write a book. But then, due to lack of expertise, I could not have dignified most of the other articles anyhow with more than a brief summary. I hope that, instead, there will be other reviewers of this important publication who will address other facets of this Festschrift.

It is time to wrap up. A mere superficial perusal of the rest of the Festschrift makes it very clear that there are a lot of people who have a lot of nice things to say about Antonio Loprieno. I will conclude by simply stating their names and encourage anyone to read all the interesting things that they have to say. The gallery of tributes is as follows.

The following persons contributed to the section “Literature and Image (Literatur und Bild)” (I, pp. 335-459): Marcelo Campagnino, Michael Cooperson, Alexander Honold, Hanna Jenni, Efraim Kristal, Ludwig Morenz, Herbert Morris, Miriam Ronsdorf, Thomas Schneider, and Rolf Stucky.

The contributors to the section “Histories (Geschichte(n))” (II, pp. 463-843) are as follows: Daniel Arpagaus, Jan Assmann, John Baines, Susanne Bickel, Alfred Bodenheimer, Martin Bonnmas. Mark Collier, Andreas Dorn, Madeleine Herren, Victoria Loprieno (Antonio Loprieno’s daughter), Maria Michela Luiselli, Ueli Mäder, Ronald Mellor, Gerald Moers, David Meyers, Rainer Nutz, Claudia Rapp, Maurus Reinkowski, William Schniedewind, Stuart Tyson Smith, Anthony Spalinger, Deborah Sweeney, Noëmi Villars, and Martin Wallraff.

The following acquaintances of Antonio Loprieno contributed to the section on “Academic Culture (Akademische Kultur)” (II, pp. 847-979): Hans Amstutz, Dominique Arlettaz, Crispino Bergamaschi, Mauro Dell’Ambrogio, Ulrich Druwe, Alex Eberle, Ernst Mohr, Georg Pfeiderer, Ursula Renold, and Ralf Simon. The last contribution by Simon is characterized in the table of contents, not as “(no.) 61,” but as “60+1.” Topical, cute.

All in all, as one can see, this is an impressive lineup of prominent minds joining forces to deliver a fitting tribute to an accomplished individual.