Papers on the Voynich Manuscript

Captain Prescott H. Currier

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Captain Prescott H. Currier (USN Ret.)

Captain Currier received an A.B. in Romance Languages at George Washington University, and a Diploma in Comparative Philology at the University of London. He began his cryptologic career in 1935, and was called to active duty with the Navy in 1940. He has served in many distinguished capacities in the field, and from 1948 to 1950, was Director of Research, Naval Security Group. Since his retirement in 1962, he has continued to serve as a consultant. His interest in the Voynich manuscript has been of very long standing, and he has devoted an impressive amount of rigorously scientific analytic effort to the problem in recent years. We are fortunate indeed that Captain Currier has consented to come from his lovely home in Maine to speak to us today about his research.

Some Important New Statistical Findings

I will start out by saying that I don't have any ''solution.'' I have a succession of what I consider to be rather important facts which I would like to review briefly. The two most important findings that I think I have made are the identification of more than one hand and the identification of more than one ''language.'' The reason they are important is that, if the manuscript were to be considered a hoax as it is by some, it's much more difficult to explain this if you consider that there was more than one individual involved, and that there is more than one ''language'' involved. These findings also make it seem much less likely that the manuscript itself is meaningless.

Two Hands and two ''Languages'' in the Herbal Section.

When I first looked at the manuscript, I was principally considering the initial (roughly) fifty folios, constituting the herbal section. The first twenty-five folios in the herbal section are obviously in one hand and one ''language,'' which I called ''A.'' (It could have been called anything at all; it was just the first one I came to.) The second twenty-five or so folios are in two hands, very obviously the work of at least two different men. In addition to this fact, the text of this second portion of the herbal section (that is, the next twenty-five of thirty folios) is in two ''languages,'' and each ''language'' is in its own hand. This means that, there being two authors of the second part of the herbal section, each one wrote in his own ''language.'' Now, I'm stretching a point a bit, I'm aware; my use of the word language is convenient, but it does not have the same connotations as it would have in normal use. Still, it is a convenient word, and I see no reason not to continue using it.

''Languages'' A and B Statistically Distinct.

Now with this information available, I went through the rest of the manuscript — some two hundred and ten pages — and in four other places I discovered the same phenomena I had associated with ''language'' B. Before I go on, the characteristics of ''languages'' A and B are obviously statistical. (I can't show you what they are here, as I don't have slides prepared. We can go into this matter in much greater detail in the discussions this afternoon.) Suffice it to say, the differences are obvious and statistically significant. There are two different series of agglomerations of symbols or letters, so that there are in fact two statistically distinguishable ''languages.''

Hands and ''Languages'' Elsewhere in the Manuscript.

Now to go briefly through the manuscript: in the *astrological section*, there seemed to be no real differences that I could detect. The *biological section** is all in one ''language'' (B) and one hand. The next section in which I noted a difference was the

*i.e., those folios featuring female figures. -Ed.

pharmaceutical section. Right in the middle of it, with ten folios on one side and ten on the other, there are six pages (two folios, folded so that there are three pages on each) which show a very obvious difference in hand: cramped, slanted, having quite a different character, very obvious even to the untrained eye. The frequency counts on this material bore out pretty much the same sort of findings that I had gotten in the herbal section. So we now have, in the pharmaceutical section, two ‘‘languages’’ and two hands. The *recipe section* at the end of the manuscript is somewhat of a mixture and didn’t show the differences so neatly. It contains only one folio on which the writing differs noticeably to the eye from that on the other folios; the statistical evidence gives some support to a ‘‘language’’ difference as well.

How Many Scribes Were There All Together?

Summarizing, we have, in the herbal section, two ‘‘languages’’ which I call ‘‘Herbal A and B,’’ and in the pharmaceutical section, two large samples, one in one ‘‘language’’ and one in the other, but in new and different hands. Now the fact of different ‘‘languages’’ and different hands should encourage us to go on and try to discover whether there were in fact *only* two different hands, or whether there may have been more. A closer examination of many sections of the manuscript revealed to me that there were not only two different hands; there were, in fact, only two ‘‘languages,’’ but perhaps as many as eight or a dozen different identifiable *hands.* Some of these distinctions may be illusory, but in the majority of cases I feel that they are valid. Particularly in the pharmaceutical section, where the first ten folios are in a hand different from the middle six pages, I cannot say with any degree of confidence that the last ten pages are in fact in the same hand as the first ten.

Taken all together, it looks to me as if there were an absolute minimum of four different hands in the pharmaceutical section. I don’t know whether they are different than those two which I previously mentioned as being in the herbal section, but they are certainly different from each other. So there are either *four* or *six* hands altogether at this point. The final section of the manuscript contains only one folio which is obviously in a different hand than all the rest, and a count of the material in that one folio supports this; it *is* different, markedly different. I’m also positive it’s different from anything I had seen before. So now we have a total of something like five or six to seven or eight different identifiable hands in the manuscript. This gives us a total of two ‘‘languages’’ and six to eight scribes (copyists, encipherers, call them what you will).

A New Slant on the Problem.

These findings put an entirely different complexion on this problem than any that I think I have noted before in any other discussions or solutions. It’s curious to me that a calligraphic or paleographic expert in one of the writings I have seen* says that the

* ‘‘Some Impressions of the Voynich Manuscript,’’ unpublished notes by Prof. A. H. Carter (Former technical historian, Army Security Agency), 1946, p. 1. -Ed.

writing is consistent throughout, and is obviously the work of one man. Well, it obviously *isn't*, and I don't see how anyone who had any training could make any such statement, but there it is!

The Line Is a Functional Entity.

In addition to my findings about ‘‘languages’’ and hands, there are two other points that I'd like to touch on very briefly. Neither of these has, I think, been discussed by anyone else before. The first point is that the line is a functional entity in the manuscript on all those pages where the text is presented linearly. There are three things about the lines that make me believe the line itself is a functional unit. The frequency counts of the beginnings and endings of lines are markedly different from the counts of the same characters internally. There are, for instance, some characters that may not occur initially in a line. There are others whose occurrence as the initial syllable of the first ‘‘word’’ of a line is about one hundredth of the expected. This by the way, is based on large samples (the biggest sample is 15,000 ‘‘words’’), so that I consider the sample to be big enough so that these statistics are significant.

The ends of the lines contain what seem to be, in many cases, meaningless symbols: little groups of letters which don't occur anywhere else, and just look as if they were added to fill out the line to the margin. Although this isn't always true, it frequently happens. There is, for instance, one symbol that, while it does occur elsewhere, occurs at the end of the last ‘‘words’’ of lines 85% of the time. One more fact: I have three computer runs of the herbal material and of the biological material. In all of that, which is almost 25,000 ‘‘words,’’ there is *not one single case* of a repeat going over the end of a line to the beginning of the next; not one. This is a large sample, too. These three findings have convinced me that the line is a functional entity, (what its function is, I don't know), and that the occurrence of certain symbols is governed by the position of a ‘‘word’’ in a line. For instance, there is a particular symbol which almost never occurs as the first letter of a ‘‘word’’ in a line except when it is followed by the letter that looks like ‘‘o.’’

Effect of ‘‘Word’’-Final Symbols on the Initial Symbol of the Following ‘‘Word.’’

The final point I will make concerns restrictions I noticed, especially in the Biological section, on symbols that can end one ‘‘word’’ and symbols that begin the next ‘‘word.’’ This occurs in other sections of the manuscript, especially in ‘‘language’’ B, but not as definitely as in ‘‘Biological B.’’*

* See Appendix A for details. -Ed.

These Findings Should be Considered by Anyone Who Studies the Manuscript.

These findings are definite enough, I think, to warrant much further study by anyone who is going to be involved in seriously attacking the text of the Voynich manuscript. I have no interpretations of them, by the way; I have no solutions. All I know is that they are significant — and *damn* significant. Anyone who attempts to work on the text without considering these, ignores them at his own peril. They are *there*, and they are very definite. No matter which one of the forms that Mary [D'Imperio] originally mentioned** [earlier on during the seminar] the material is considered to be, all of these other facts must be taken into consideration before anyone continues. The validity of text produced by any method at all must, I think, be judged against this statistical background,

That, I think, is all that I am prepared to say now, but this afternoon any of you who do come can review the points and ask me any questions you choose. I have a fairly large collection of statistical charts which will bear out most of the points that I have made. These have been reproduced, and with them my very brief notes on the four points I have made this morning.** Some of you now have copies of them. I think that the discussions this afternoon can be, indeed, quite fruitful if those of you who do have copies of my material would undertake to go through it and make up in your own minds any questions or discussions that you'd like to go into this afternoon. Thanks very much.

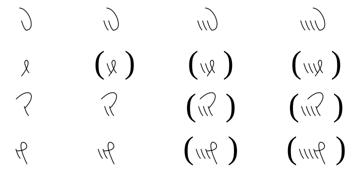
**See pp. 7-8 [of the original ''Proceedings''] above. -Ed

Further Details of New Statistical Findings.

1. The Nature of the Symbols.

I’ve looked at most of these letters under a magnifying glass, so I think I know how they were all actually made. These letters: , , , all seem to start with a ‘‘c’’-curve, which was made first, in this direction: [counter-clockwise], so we have: = [written first, counter-clockwise, next its mirror-image, clockwise], = [first, then horizontal line starting from its top, going right, looping up and counter-clockwise, continued with a down flourish ending below the line, to the left], = [first , then flourish at top of c, going down, ending below the line], = [first , then flourish at top of c, going up and counter-clockwise]. The forms all have counterparts starting with : , , , etc. We also have = +. All the letters containing an initial ‘‘c’’-curve are also the only letters that can be preceded in the same word by the little letter that looks like ‘‘c,’’ e.g. , . On the other hand, the letters and (which have very high frequencies) can *never* be preceded by , *ever*; they are instead preceded by .

The final letters (that is, the ones I call finals, although they can also occur elsewhere) are in two series, one preceded by and the other by , giving a series of sixteen:



The ones in parentheses are very low-frequency; the others all occur with respectable frequency. In addition, these combinations of symbols which appear as finals may occur separately — ‘‘unattached finals,’’ as I call them. A large number of unattached finals is a characteristic of ‘‘Language’’ B, and *not* ‘‘Language’’ A, by the way.

All this indicates to me that considerable thought was put into how this mess was made up. We have the fact that you can make up almost any of the other letters out of these two symbols and ; it doesn’t *mean* anything, but it’s interesting.

2. Origin of the Symbols.

This symbol is a common Latin abbreviation for CON, CUM, or -US, so that it can come at both the beginnings and ends of words. For example, ‘‘continuus’’ might be written ‘‘9tinu9.’’ Now is one of the few symbols in the manuscript that does in fact occur at beginnings and endings of frequent words, especially in combination with the , series. It looks as if whoever designed the alphabet used because this symbol resembled the one used throughout medieval Latin for CON, -US, a frequent initial and final. I think that’s the source of that particular letter.

As for , it is a frequent letter in Etruscan, in Lydian, and in the Lemnos alphabet, but there that letter always had the value ‘‘F,’’ never ‘‘S.’’ In medieval Latin on occasion it did represent ‘‘S.’’ This symbol could have been taken from these other alphabets.

You can pick out resemblances between Latin abbreviations and other alphabets for most symbols except for the series , , , . The symbol looks very much like a medieval Latin abbreviation for ‘‘tinus.’’ The last two look as if they are simply variations of the first two, with the second vertical stroke pushed back. They (,) appear 90-95% of the time in the first lines of paragraphs, in some 400 occurrences in one section of the manuscript.

One might conclude that , are an elaborate form of , , with the same value. This is often the case in medieval manuscripts, especially in illuminated ones; certain letters have magnified, aberrant, beautified forms. But, not true! These two letters , are *not* the same as those two , , as the statistics show. The letters , are followed anywhere in a ‘‘word’’ by our little friend about half the time (say 750 out of a total of 1500), including initially. These two, , , are *never, ever, anywhere* in the manuscript, followed by . These latter symbols are much less frequent than the first two, but their occurrence followed by is *zero*. I don’t have to calculate sigmages on that! Therefore, , are *not* aberrant or variant forms of , , but separate letters in their own right. This holds true through the whole manuscript. That is one of the peculiar things about the manuscript: we have two ‘‘languages’’ — they are definite, no doubt about it at all — but there are features like this that follow through from one ‘‘language’’ to another. That’s just an item of incidental intelligence; there it is, for what it’s worth.

Question (D’Imperio): I wonder about the cases where the two loops of and are separated from each other, and one end comes down in the middle of another word, often on top of that little letter like a table, ?

Currier: That may be a way of abbreviating two of those looped letters. It doesn’t happen frequently enough to bother me. (Example:)

3. Different Frequencies of Symbols at Beginnings, Middles, and Ends of Lines.

At beginnings and ends of lines, we have skewed frequencies. For example, let’s take these two letters and . (This letter , by the way, is in fact made like this:) Here are statistics from ‘‘Herbal A’’ material, about 6500 words, 1000 lines, averaging seven words per line:

‘‘word’’-initial symbols	total frequency as ‘‘word’’-initial	expected in any ‘‘word’’	actual, in first ‘‘word’’
	118	20	3
	212	38	26
	24	4,5	0
	45	10	10

If its occurrence as an initial were random, we would expect it to occur one seventh of the time in each word position of a line. Actually, it is a very infrequent word initial at the beginning of a line, except when there is an intercalated . This applies only to ‘‘Language’’ A, by the way; words with this initial group are low in ‘‘Language’’ B (, for example, occurs only 5 times in Herbal B, but 212 times in Herbal A).

4. The Nature of the Symbols , , , .

My next point concerns the so-called ‘‘ligatures’’ based, apparently, on the series , , , . They are made like this, by the way: , with , etc., written on top of it. In Herbal A material, in fact in all A material, this series is initially high; in B, it is very low — another way of identifying the two ‘‘languages.’’ In Herbal A, the word-initial occurrences are as follows:

symbol	all ‘‘word’’ initials	first ‘‘word’’ of line
	326	3
	67	1
	82	0
	14	0

These ‘‘ligatures’’ seem to behave almost, but not quite, like , . In contrast, whether or not followed by , , , or , the series , , , are *very* high in both ‘‘languages,’’ and frequently as paragraph and line initials. The ‘‘ligatures’’ can *never* occur as paragraph initial, and almost never line initial.

Therefore, , , and the like are symbols in their own right, and are *not* equal to or , etc. These statistical considerations are the reason why I made up my alphabet the way I did; I restricted it as much as possible to letters in their own right, not ligatures.

5. Effects of the Ending of One ‘‘Word’’ on the Beginning of the Next ‘‘Word.’’

You remember I mentioned that some ‘‘word’’-finals have an obvious and statistically-significant effect on the initial symbol of a following ‘‘word.’’ This is almost exclusively to be found in ‘‘Language’’ B, and especially in ‘‘Biological B’’ material. For example, we have:

‘‘words’’ ending in:	Next ‘‘word’’ begins with:		
	†o	ℓ or ʔ	cc or ɑc
ℓ series	13	7	91
ʔ series	10	2	68
ↄ series	23	0	275
9 series	592	184	168

(The above figures are condensed from Table 5A, Appendix A.)

‘‘Words’’ ending in the 9 sort of symbol, which is very frequent, are followed about four times as often by ‘‘words’’ beginning with †o. That is a fact, and it holds true throughout the entire twenty pages of ‘‘Biological B.’’ It’s something that has to be considered by anyone who does any work on the manuscript. These phenomena are *consistent, statistically significant,* and hold true throughout those areas of text where they are found. I can think of no linguistic explanation for this sort of phenomenon, not if we are dealing with words or phrases, or the syntax of a language where suffixes are present. In no language I know of does the suffix of a word have anything to do with the beginning of the next word.

(At this point, Captain Currier’s presentation was concluded, and questions were raised by listeners. The lengthy and interesting discussion that followed, transcribed in its entirety from our tape record, comprises the next section of these notes. — Ed.)

Questions and Discussion

Question (Speaker not identified): How do you account for the full-word repeats?

Currier: That’s just the point — they’re *not words!*

Child: I don’t think you can say that doesn’t happen. Now, it may not happen with the languages in a more or less consistent, normative writing system. But it does when a scribe is noting rapid speech, with all its slurs and elisions, rather than the facts of grammar. The sounds at the end of one word can influence those at the beginning of the next.

Currier: Not this much.

D’Imperio: Could I suggest that it may be related to the constraints on groups in a system like a code or synthetic language, when words from certain pages or parts of the code combine preferentially with words from certain other parts of the code?

Currier: Precisely, precisely; yes, right.

Valaki: What about sounds at the beginning of one word being changed by neighboring sounds, at the end of the previous word? This happens in some languages (examples from Greek which are not audible on the tape. —Ed.)

Currier: I don't think it would happen to this extent... Has anyone seen my computer run on ‘‘Biological B?’’

D'Imperio: I haven't seen that — I'd certainly like to get a copy!

Currier: ‘‘Biological B’’ is by far the most interesting; *very constrained*, very interesting from a statistical point of view. (Some examples, not clear on tape —Ed.) I have a whole notebook of statistical charts at home: things I want to look into, and took various samples of limited areas of text. But I think anyone who's really interested ought to do their own. These are the best kind of evidence for valid conclusions. If you want to make an assumption of a value for some particular symbol, with an index you can try it out and see what happens. Certain things will also arise from taking these statistics which will provide evidence for a new theory. If you view all these statistics as basic background evidence on which to base theories, you can come up with a hypothesis which can be tested, rather than *starting* with a hypothesis and then looking for evidence to back it up. This statistical background is the sort of evidence anyone who is going to work on this document should be aware of. It gives you something against which you can compare the material and test your hypotheses.

Question (Speaker unidentified): Have there been any studies on the lengths of words?

Currier: Not specifically. I've got it all at home... but it hasn't suggested anything to me.

D'Imperio: I made a partial study of word lengths on a small scale (15,000 characters); few words were longer than seven or eight symbols or shorter than two.

Currier: But there *are* a lot that are exactly two long. (Examples from ‘‘Herbal A’’ and ‘‘Herbal B,’’ not audible on tape —Ed.) Certain groups — a different one in A than in B material — are repeated four times in a row; they would *have* to be numbers, I can't think of anything else. If the one were ‘‘zero’’ in ‘‘Herbal A,’’ the other might be ‘‘zero’’ in ‘‘Herbal B,’’ and this would be what you'd look up in your artificial language system. I don’t believe that, by the way.

This statistical data of mine is available — my notes and observations. I’ve come to no real conclusions, except that this can’t be, as far as I can see, a straightforward simple encipherment of any linguistic data; there has to be an intermediate step somewhere as far as I can see.

Question (Speaker unidentified): You said that each line was a separate sentence unto itself...

Currier: An annoying little circumstance: words beginning with $\overline{cc}$ almost never seem to occur first in a line. I thought perhaps I might try numerals one to ten for the letters that come before $\overline{cc}$ in line-initial position, but I can’t make it work. But this kind of thing makes it look as if the line is a functional entity; that is what bothers me. I can’t interpret the data!

Question (Speaker unidentified): Is that true all the way through the manuscript?

Currier: Yes, it is basically true, but especially in ‘‘Biological B.’’

D’Imperio: There seem to be very strong constraints in combinations of symbols; only a very limited number of letters occur with each other letter in certain positions of a ‘‘word.’’

Currier: Yes... (Examples, not clear on tape. —Ed.) By the way, if anyone does transcribe any more text, I wish they would use my alphabet; then we can put all the data and results together.

D’Imperio: I have a copy of Captain Currier’s alphabet and sorting sequence.

Currier: You don’t need to bother about the sorting sequence. I had a particular reason for it back when I did the earlier work but you don’t need it now. I’d like to see someone do more with the problem, in the ‘‘Recipe’’ section for example. You should be careful when you transcribe, though; you have to make some judgements of what a letter is, and it takes practice to get the hang of it.

Miller: I’d like to bring up something related to Mary’s introduction this morning, where she associated my name with the theory that the manuscript was meaningless. I would object to the phrase ‘‘meaningless doodles,’’ because I think this is *purposeful but inarticulate* writing; doodles are simply to pass the time away...

D'Imperio: But the point I was emphasizing was that this theory considered the manuscript meaningless within our context of *trying to decipher it...*

Miller: The meaning is irrecoverable. If there is such a school of thought, [of people who believe that the meaning of the manuscript is inherently and essentially irrecoverable —Ed.], who else is in it besides me?

D'Imperio: There are some people who come pretty close: Dr. MacClintock, for example, thinks it's almost entirely irrecoverable, I believe...

Miller: Has this been argued on the basis of a careful analysis of the text, or merely because it isn't readable? I don't think the thing is a hoax. But no details have been given of the theories (that the meaning is irrecoverable) and I would like to read more about it.

D'Imperio: I think it's primarily exasperation on the part of people that have been frustrated time and again in attempting to decipher it, and they just end up saying ''Oh, fooey! How can the thing mean anything, with all these weird repeats and such...?''

Miller: But with all these statistics that Captain Currier, Brigadier Tiltman, and Mr. Friedman have given — hasn't anyone...

D'Imperio: The trouble is, how *can you prove* that something is meaningless, or that its meaning is irrecoverable? That is just what is left after you've disproven all the specific positive decipherment theories you or anyone else has thought of so far. But another good one might still always come along. (Editorial comment: If we were to prove scientifically that a text's meaning is irrecoverable, we would require either (1) a theory that provided for certain observable criteria or characteristics that strings having recoverable meanings must have, and a proof that this particular text *does not* exhibit those criteria; or (2) a theory providing for certain observable criteria which strings having irrecoverable meanings must have, and a proof that this particular string before us *does* exhibit those criteria. This would constitute a sort of ''uncomputability'' or ''undecidability'' theory for the *semantics* of textual strings. Is this possible? At our present state of knowledge, I sincerely doubt it. Still, it raises some highly interesting philosophical questions that deserve further attention from someone qualified to explore them. There are, of course, tests for ''psychological random'' characteristics of various sorts, which would provide some strong support for a hypothesis that the text had been *fabricated*, independently of any semantic or linguistic structure having a recoverable meaning; these tests and hypotheses ought certainly to be applied to the Voynich text.)

Valaki: Some time ago I saw a screen for sale at a furniture store. It was a four-panel screen; on one panel there was writing in Greek, which I read and found to be one of Aesop's fables. When I tried to read the second panel, I couldn't make any sense out of it — nothing went with anything else. I finally realized that they were just individual Greek words copied off at random. The third panel was just Greek letters, and the fourth panel was imitation Greek letters!

D'Imperio: I wish you had bought it — what a beautiful test case! We could have made some frequency counts on it and...

Valaki: Maybe that's like the Voynich — it could turn out to be a good straight copying job.

D'Imperio: But still, back to Doris' point, how can we demonstrate that? You see, the way you realized that about the screen — the fact that the other panels were meaningless — was because you knew Greek and you read the fable on the first panel. Then, when you looked at the others, you saw the degradation...

Valaki: I really thought my Greek had gone! Nothing was matching anything else; words didn't go together. I sort of went backwards to attack it.

D'Imperio: Well, with the Voynich, we are in the position of having something we can't read any part of, to any degree, and that doesn't look like anything we've ever seen before. How can we show, demonstrate, that it is meaningless?

Miller: You don't have to demonstrate...

Currier: Nobody has tried, not that I know of.

D'Imperio: No, not that I've ever seen.

Currier: Evidence that it can't be ‘‘doodles’’ is the minimum of six people involved in their production. I can prove four beyond a shadow of a doubt. I'm not a paleographer; I wouldn't stand up in court and try to defend this against a paleographer. But I'm positive, particularly in the Herbal Section. I imagine it to have happened something like this: some sixty-five folios were prepared ahead of time with drawings on them. They were placed on a a table *so*. The first twenty-five folios were taken, one at a time, off the top and filled in with writing by one individual. At the end of those twenty-five, he got very tired and he called for help. Another man sat down opposite him at the same table. And they took them off, one at a time: one man took one off and did his thing, in his own ‘‘language,’’ while the other man did *his* thing with another in *his* ‘‘language.’’ And they went through the second stack and interleaved them; one man did it one way and the other did it the other way. When they were done, they had the Herbal Section!

Question (Speaker unidentified): Are you convinced that the page numbering is correct?

Currier: Yes. I am sure the page numbering is that of the original...

Question (Speaker unidentified): What about the fact that there were no erasures? That makes it look like a copying job.

Currier: It must be a copying job. But how do two people copying from a single source produce material in two different ‘‘languages’’ simultaneously? I can just see them sitting there! I’m absolutely positive this is the way it was done. The folios were prepared in advance by someone else with the drawings on them. Sometimes the writing overlaps the drawings somewhat. The pictures of the Herbal Section look as if they were drawn by a single individual, but this I couldn’t prove. The writing on folios 1 to 25 was done by one man. On folios 25 to 65, it was done by two men, one who worked a little faster (the man who did the first batch did more of the second batch; he was more experienced).

Buck: It was noted that some pages are missing, and the cover is missing. Do you have any ideas about the reason?

Currier: No, I have no theories.

Miller: Somebody stripped off the beautiful pictures!

Currier: Then he left a lot of beautiful pictures behind!

D’Imperio: One of the missing folios was for the zodiac signs of Capricorn and Aquarius; maybe that was somebody’s horoscope?

Question (Speaker unidentified): When a new hand takes over, do you see variations in the mode of writing the symbols?

Currier: Yes, but it’s the overall impression of the writing. In general, for example, in ‘‘Herbal A,’’ the writing is upright, rounded, lines are well-spaced, it looks clean, clear, with no extraneous material. ‘‘Herbal B,’’ in contrast, is uphill, slanted cramped writing.

It’s obvious to me. The first thing I noted looking at the manuscript as a whole was this difference in the writing in the Herbal Section, before I had taken a single count. I separated the pages by sight first, then took a ten-page sample in each of the two separate writings, and made separate counts. It stared me in the face — there it was: all my selections were correct. It was a sufficiently controlled procedure to make me think these conclusions are valid. Anyone can see it — just lay the pages out and look. I can’t prove the pages are in the right order, but I just *feel* that they are. In the Astrological Section, the signs of the zodiac are in the right order.

D’Imperio: There is some evidence in the folio gathering — the numbers in the bottom corners of some pages, about every eight folios. They agree well with the folio numbering at the beginning of the manuscript, at least. They also show some relatively early forms of the numerals. This gives us a bit more evidence that some of the pages at least are in the right order.

Buck: I would like to speculate about where the missing pages are...

D’Imperio: Maybe they’ll show up some day, among somebody’s papers!

APPENDIX A

The VOYNICH MANUSCRIPT
Some Notes and Observations
October 1976

1. The matter of 'hands'

It was noted early in the study of the Herbal Section (pp 1-112) that the handwriting characteristics of several pairs of adjacent folios varied perceptibly, even to an untrained eye. A few elementary frequency counts showed that the statistical profiles of the textual material on these folios also differed significantly. Further investigation of all the folios in the section revealed that there were two different 'hands' in use throughout the entire section, each writing in its own 'language,' hereinafter called Languages A and B.

With this evidence at hand a check of the remaining sections of the Manuscript turned up the following:

(a) In the *Astrological Section* (pp 113-146) there seemed to be no significant difference in the writing on any of the folios except that there appeared to be a 'foreign' element evident in the inclusion of a few symbols which occur nowhere else in the Manuscript. The 'language' throughout is mostly A but without some of the more pronounced 'A' features found in Herbal A.

(b) The *Biological Section* (pp 147-166) appears to be the work of a single scribe, all in language B, with strong, sharply delineated statistical characteristics. The language of this section is more restricted, perhaps even more 'regular' than the language 'B' in other sections of the Manuscript. This could conceivably be the result of this section being the product of only one person.

(c) In the *Pharmaceutical Section* (pp 167-211), pp 167-173 and two folios (pp 193-198) in the mid-portion of the section are in Language 'B'; the remaining folios are in Language 'A.' An interesting point here is the fact that there seemed to be more than the expected two 'hands,' one for each 'language' as in the Herbal Section. The difference between the 'B' writing of the mid-portion (pp 193-198) and the 'A' writing of the surrounding folios (pp 179-192; pp 199-211) is obvious and easily discernible and was noted on the first quick pass through the Manuscript. But it is not at all clear that the initial Language 'B'-folios (pp 167-173) are in the same hand as pp 193-198 nor can it be said with certainty that the Language 'A'-folios (pp 179-192 and pp 199-211) are all the work of a single individual. Additionally, p 174 is in Language 'A' and in a hand different from any other in the Pharmaceutical Section.

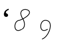
The Newbold foliation indicates that the Biological Section extends through ff 85-86 and it would appear from the illustrations that the Pharmaceutical Section does not begin until f 87. However, frequency counts before and after the break at f 84/f 85 indicate a change from Biological material to something else. For example, the final 'o89,' which does not occur in the Biol. B text, shows up in ff 85-86 with quite a respectable frequency and matches the frequency of this final in the Pharmaceutical ‘B’ text on ff 94-95. I am reasonably certain that the handwriting on ff 85-86 is not the same as that on ff 95-96 but I cannot be sure that it differs from the Biol. B hand. In sum, I would venture a guess that there are at least three and perhaps as many as five or six different hands in evidence in this section. On the other ‘hand’ it may all be an illusion.

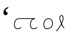
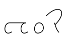
(d) The *Recipe Section* (pp 212-234) contains only one folio on which the writing differs noticeably from that on the other folios. This difference is supported to a degree by statistical evidence. The ‘language’ throughout the Section is ‘modified B’ (i.e., contains certain ‘A’ characteristics). It might be worth noting, however, that there seem to be some less discernible handwriting variations on many other folios in the Recipe Section. I cannot be sure that these are valid differences but the frequency counts of the material on the folios in question are just slightly supportive.

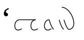
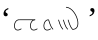
2. The matter of ‘language’

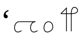
I should be noted before going on that the word ‘language’ is quite loosely used here and throughout these notes. It connotes only a marked statistical difference between two sets of text. It in no way implies the existence of any underlying language. Being convenient however, it will continue to be used.

As previously stated in para. 1 above, the Herbal Section contains both Language ‘A’ and ‘B.’ The principal differences between the two ‘languages’ in this Section are:

(a) Final ‘’ is very high in Language ‘B’; almost non-existent in Language ‘A.’

(b) The symbol groups ‘’ and ‘’ are very high in ‘A’ and often occur repeated; low in ‘B’.

(c) The symbol groups ‘’ and ‘’ rarely occur in ‘B’; medium frequency in ‘A.’

(d) Initial ‘’ high in ‘A’; rare in ‘B.’

(e) Initial ‘’ very high in ‘A’; very low in ‘B.’

(f) ‘Unattached’ finals scattered throughout Language ‘B’ texts in considerable profusion; generally *much* less noticeable in Language ‘A.’

These features are to be found generally in the other Sections of the manuscript although there are always local variations; which of course could imply a ‘subject-matter’ effect.

The discovery of the two ‘languages’ in the Herbal Section was the principal reason for transcribing and indexing this material. It was hoped that by application of comparative techniques to the Herbal A and B texts, ostensibly dealing with identical subject matter, some clue to the nature of the two ‘systems of writing’ might be forthcoming. The results were completely negative; there was no sign of parallel constructions or any other evidence that was useful in this regard. It was impossible not to conclude that (a) we were not dealing with a ‘linguistic’ recording of data and (b) the illustrations had little to do with the accompanying text. Study of other sections of the Manuscript where ‘A’ and ‘B’ texts are found has produced nothing to alter this conclusion. Further, it has so far proved impossible to categorize or to classify grammatically any series of ‘words’ or to discern any use patterns that that would suggest any recognizable syntactic arrangement of the underlying text. Perhaps even more important, I have been unable to identify ‘words’ or individual symbols in either ‘language’ to which I could assign even tentative numerical values. It seems quite incredible to me that any systems of writing (or a simple substitution thereof) would not betray one or both of the above features.

3. The effect of word-final symbols on the initial symbol of the following ‘word’

This ‘word-final effect’ first became evident in a study of the Biol. B index wherein it was noted that the final symbol of ‘words’ preceding ‘words’ with an initial ‘ ’ was restricted pretty largely to ‘ ’; and that initial ‘ / ’ was preceded much more frequently than expected by finals of the ‘ ’ series and the ‘ ’ series. Additionally, ‘words’ with initial ‘ / ’ occur in line-initial position far *less* frequently than expected, which perhaps might be construed as being preceded by an ‘initial nil.’

This phenomenon occurs in other sections of the Manuscript, especially in those ‘written’ in Language B, but in no case with quite the same definity as in Biological B. Language A texts are fairly close to expected in this respect.

I can think of no interpretation of this phenomenon, linguistic or otherwise. Inflexional endings would certainly not have this effect nor would any other grammatical feature that I know of if we assume that we are dealing with *words*. If, however, these word-appearing elements are something else, syllables, letters, even digits, restrictions of this sort might well occur.

4. The line as functional entity

As mentioned in para. 3 above, ‘words’ with initial ‘ / ’ are unexpectedly low in line initial position (on average about .1 of expected); other ‘words’ occur in this position far more frequently than expected, particularly ‘words’ with initial ‘ ,’ ‘ ’ etc., which have the appearance of ‘ ’-initial ‘words’ suitably modified for line-initial use. Symbol groups at the ends of lines are frequently of a character unlike those appearing in the body of the text sometimes having the appearance of fillers. Further, in only one instance so far noted has a repeated sequence (of ‘words’) extended beyond the end of one line into the beginning of the next.

All in all it is difficult not to assume that the line, on those pages on which the text has a linear arrangement, is a self-contained unit with a function yet to be discovered.

5. Appended Tables

Table A. Voynich Manuscript foliation-pagination concordance with an indication of ‘language’ and ‘hand’ where known.

Table 1. Frequency of initials with mesial ‘ ’ and ‘ ’ for all sections showing both total and line-initial frequencies.

Table 2. Frequency of finals following ‘ / ’ for all sections of the Manuscrip[t.

Table 3. Frequency of finals following medial ‘ ’ and ‘ ’ for Herb. A, Herb. B and Biol. B.

Table 4. Frequency of ‘ ’-medials (‘ ’ preceded by a single symbol) showing total and line-initial frequencies.

Table 5. Biol. B line-initial frequencies (all ‘words’) plus frequencies of finals preceding the listed initials.

Table 6. Biol. B—Effect of final on initial of following ‘word.’