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NATIONAL SECURITY AGENCY CENTRAL SECURITY SERVICE FORT GEORGE G. MEADE, MARYLAND 20755-6000

> FOIA Case: 101982A 4 August 2017

JOHN GREENEWALD

Dear Mr. Greenewald:

This responds to your Freedom of Information Act (FOIA) request of 27 June 2017 for "a copy of records, electronic or otherwise, pertaining to the Voynich manuscript. The Voynich manuscript is an illustrated codex and-written in an unknown writing system. The vellum on which it is written has been carbon-dated to the early 15th century (1404-1438), and is may have been composed in Northern Italy during the Italian Renaissance. The manuscript is named after Wilfrid Voynich, a Polish book dealer who purchased it in 1912. I know that there should be at least one responsive record, which is DocRefiD A2229436 and DocID 2625882. Please include all documents that may be responsive to my request, which include, but are not limited to, reports, letters, memos, etc." Your request was received on 28 June 2017, and has been assigned Case Number 101982.

For purposes of this request and based on the information you provided in your letter, you are considered an "all other" requester. There are no assessable fees for this request. Your request has been processed under the FOIA and some of the documents you requested are enclosed. Certain information, however, has been deleted from the enclosures.

This Agency is authorized by various statutes to protect certain information concerning its activities. We have determined that such information exists in this document. Accordingly, those portions are exempt from disclosure pursuant to the third exemption of the FOIA, which provides for the withholding of information specifically protected from disclosure by statute. The specific statutes applicable in this case are Title 18 U.S. Code 798; Title 50 U.S. Code 3024(i); and Section 6, Public Law 86-36 (50 U.S. Code 3605).

Since these deletions may be construed as a partial denial of your request, you are hereby advised of this Agency's appeal procedures.

You may appeal this decision. If you decide to appeal, you should do so in the manner outlined below.

• The appeal must be sent via U.S. postal mail, fax, or electronic delivery (e-mail) and addressed to:

NSA/CSS FOIA/PA Appeal Authority (P132) National Security Agency 9800 Savage Road STE 6932 Fort George G. Meade, MD 20755-6932

The facsimile number is (443)479-3612, The appropriate email address to submit an appeal is <u>FOIARSC@nsa.gov</u>,

- It must be postmarked or delivered electronically no later than 90 calendar days from the date of this letter. Decisions appealed after 90 days will not be addressed.
- Please include the case number provided above.
- Please describe with sufficient detail why you believe the denial of the requested information was unwarranted.

NSA will endeavor to respond within 20 working days of receiving your appeal, absent any unusual circumstances.

We have also determined that many of the records you seek are publicly available and can be accessed on nsa.gov by typing "Voynich" into the search box on the home screen.

You may also contact our FOIA Public Liaison at <u>foialo@nsa.gov</u> for any further assistance and to discuss any aspect of your request. Additionally, you may contact the Office of Government Information Services (OGIS) at the National Archives and Records Administration to inquire about the FOIA mediation services they offer. The contact information for OGIS is as follows:

Office of Government Information Services National Archives and Records Administration 8601 Adelphi Rd- OGIS College Park, MD 20740 <u>ogis@nara.gov</u> (877) 684-6448 (202) 741-5770 Fax (202) 741-5769

Sincerely,

CX LOOKON TOR

JOHN R. CHAPMAN Chief, FOIA/PA Office NSA Initial Denial Authority

Encls: a/s

Posted by @ 2008-11-19	 (b) (6)
Tags: voynich manuscript	
Mood: contemplative (b) (3) -P.L. 86-36	

(U) The Voynich Manuscript

(U) I have had the pleasure of reading "The Voynich Manuscript: An Elegant Enigma" both while I was stationed at Meade and then over NSANet when CCH converted it to a digital copy. The concept of a several hundred year old ciphered Manuscript intrigues me and I would like to study a copy of the manuscript by itself, without any commentary, but have had no luck. I was wondering if anybody knew where a copy of the original manuscript was available to either buy or download.

@ 2008-11-19 (b) (6) 1625

(U) Kevin Knight from ISI has an interesting talk on the Voynich manuscript. He links to a German website at the end of his talk, which seems to have what you're looking for. Kevin's talk is on the Internet at: [http://www.isi.edu/natural-language/people /voynich.pdf]. (b) (3) -P.L. 86-36

1626	bdcarle @ 2008-11-19		• • • • • • • • • • • • • • • • • • • •	(b)	(6	,)
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The Voynich Manuscript

For what it's worth, the Yale University library has the original version of the manuscript. Don't know if they make copies available to the public.

1627 re 1626 @ 2008-1	1-19	(b) (6)
Re: The Voynich Manuscript	(b) (3)-P.L. 86-36	

(U) That looks like a perfect resource for the Google Books Project to make available online.

written with Text Helper

(b) (3) -P.L. 86-36 @ 2008-11-21 04:05:43

Thank you all for your replies. I've gotten in touch with Yale and they DO carry photocopies of the original documents. As for some of the other websites, thank you for providing them! They provided just that much more information for me to draw upon.

@ 2008-11-24 (b) (3)-P.L. 86-36

Voynich

1628

was an expert on several languages, and he spent many years NSA retiree studying the Voynich. He came up with a very interesting theory about the manuscript. He still lives in the area, and folks have thought about getting him to come to NSA for a talk to present his ideas. Maybe the history folks would be interested in inviting him for a talk. (b) (3)-P.L. 86-36

Approved for Release by NSA on 07-18-2017, FOIA Case # 101982

(b) (6)

1696		@ 2008-11-28	(b) (6)
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links from wikipedia (b) (3)-P.L. 86-36

(U//FOUC) There are links to the source material on wikipedia, including a PDF from a .ru domain - which I didn't dare try to access from work!

Dear Dr. Evans:

Doc ID: 6588663

Item So. 160 in the Fabyan Collection, now in the Rare Book Room of the Library of Congress is Cave Book's <u>The Universal</u> <u>Character</u>, etc., printed in London in 1657.

As you perhaps know, a few of my associates and I have been interested, for a musber of years and as a purely leisure-time home project, in trying to make some sense of the so-called Voynich Manuscript, which contains cipher writings willegedly attributed to Roger Bacon. Having reached some tentative conclusions with respect to the basic nature of the cryptographic system underlying the writing in the Voynich MSS, I have been searching through the literature for certain types of systems suggested in the 16th and 17th Centuries as "universal languages." The Cave Beak book recently came to ay attention and I would like very much to have a copy of it for detailed and careful study of its comtents. It seems difficult and expensive to obtain the book, and in lieu of the book itself, a photostatic copy would do. But the personal expense of photostating it at the Library makes me hesitate. On the other hand, I could probably have the work done with our own facilities, if the book could be made available for just one day, and could make more than one copy for the use of my associates in the preject.

In view of the fact that it was through my intersession that the Library acquired the Fabyan Collection, could you possibly make an emseption to the Library's rules and let me borrow the book for the time and purpose indicated?

Assuring you of my appreciation of your kind consideration of my request and with cordial greetings, I am

Sincerely yours,

WILLIAM F. FRIEDMAN Chief, Technical Division

AFSA-14/mer

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Dr. Luther H. Evans Librarian of Congress Washington, D. C.

Approved for Release by NSA on 07-18-2017, FOIA Case # 101982

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DOCINDEX

Report Run Date: 2010/07/19

Case # 44035

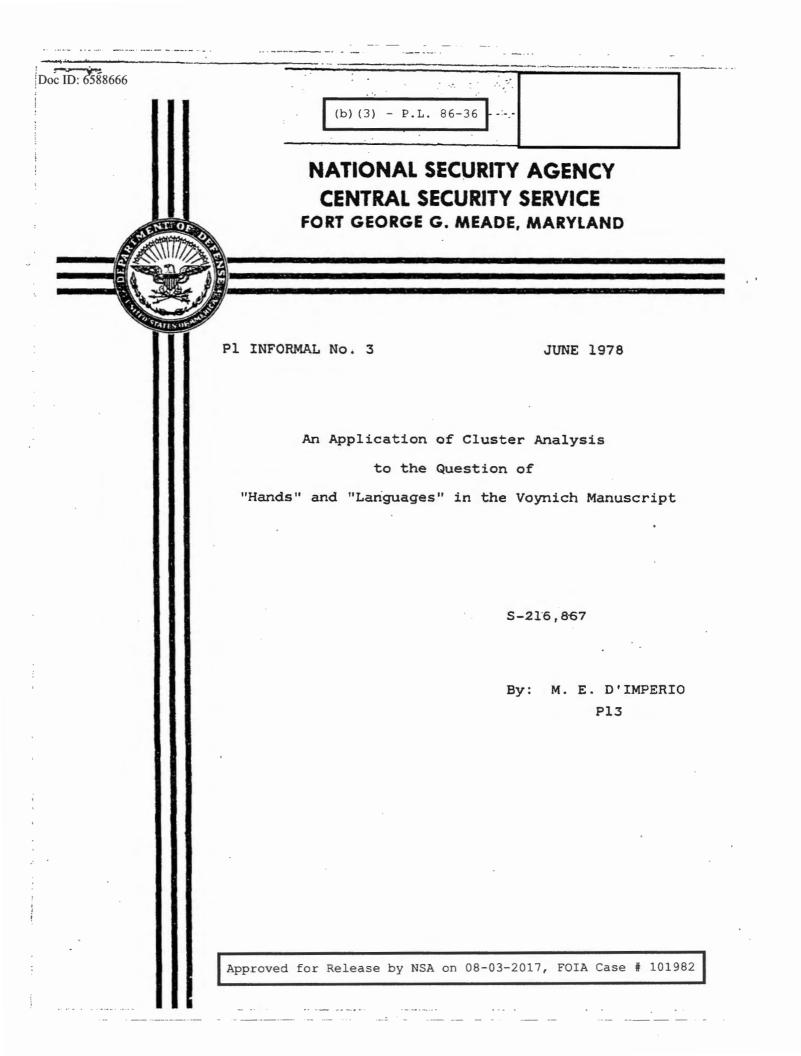
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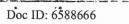
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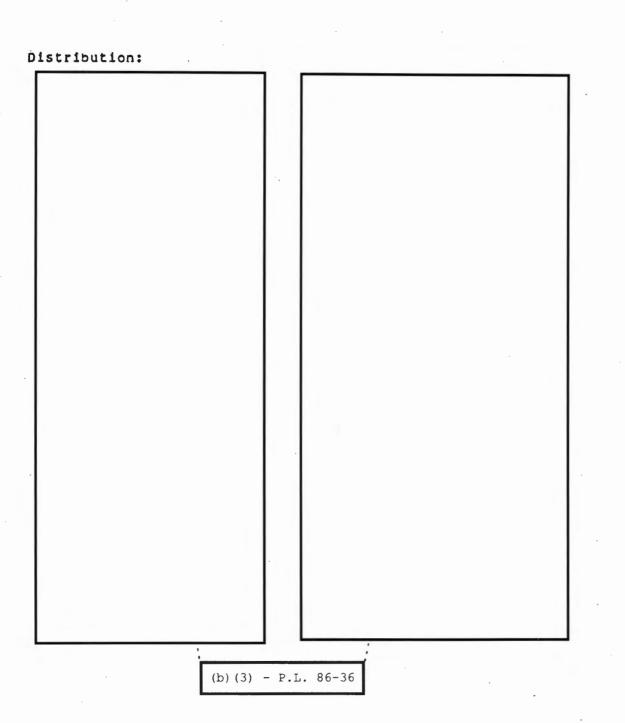
DOCUMENT REF ID	TITLE	DATE	
A127519	The Voynich Manuscript: "The Most Mysterious Manuscript in the World"	3/4/1967	
A2383707	NSA: The Voynich Manuscript: An Elegant Enigma	1/1/1978	
A636899	The Voynich Manuscript Revisited	10/30/2001	
A636902	An Application of Cluster Analysis and Multidimensional Scaling to the Question of "Hands and "Languages" in the Voynich Manuscript		

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I am reasonably certain that few readers of this paper will require much of an introduction to the topic of the Voynich Manuscript. Brigadier John Tiltman's informative and enjoyable presentation on 17 November 1975, and the seminar on 30 November 1976 served to familiarize many with this cryptanalytic challenge from the late Middle Ages. There have also been several articles on the subject in Cryptolog during the last few years. For any reader who desires an overview of the topic and a summary of some recent research, I recommend the Proceedings of our 1976 seminar [4] a copy of which may be obtained from M. DImperio, R53/P13. Two presentations by Captain Prescott H. Currier constituted high points of that occasion: in them, and in the supporting paper printed as Appendix A of the Proceedings, he set forth his theory that there were several different scribes involved in the production of the Voynich Manuscript, and that their individuality was attested not only by characteristic "hands", reliably distinguishable by eye, but also by statistically distinct "languages". If this hypothesis could be confirmed, it would provide students of the Voynich Manuscript with an important new insight into the problem. This paper describes the results of an exploratory study of Currier's theory using cluster analysis and multiple scaling. It has another purpose as well, primarily tutorial, in that I felt a detailed description of an application of these techniques to a relatively clear-cut problem might prove useful to others considering them for use in operational contexts.

Currier's Hypothesis

The Voynich Manuscript is a rather long document, comprising some 210 pages of writing in an unknown script liberally interspersed with colored drawings of a wide range of subjects and exhibiting (at least to us, today) a highly bizarre nature. The manuscript is considered to contain several sections, presumably dealing with different subject matter, as judged by the nature of the drawings. A long initial "herbal" section is profusely illustrated with representations of fanciful plants; an "astrological" section shows zodiacal diagrams and many illustrations featuring stars, suns, moons, and other cosmological elements; a "biological" section is marked by strange associations of naked female figures and objects like pipes, pools, and platforms; other sections are similarly distinguished by their illustrations. Currier's findings concern contrasts he has seen between sets of pages in certain sections of the manuscript, leading him to classify the pages into sub-groupings: an approach quite different from that of other students of the manuscript, who almost invariably consider it the monolithic production of one author.

Here are a few highlights drawn from Currier's exposition of his theory at the 1976 conference:

"The first twenty-five folios in the herbal section are obviously in one hand and one 'language', which I call 'A'. ...The second twenty-five folios are in two hands, very obviously the work of at least two different men [{A and B]. In addition to this fact, the text of this second portion of the herbal section (that is, the next twenty-five or thirty folios) is in two 'languages' [A and B], 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 with this information available, I went through the rest of the manuscript...and in four other places I discovered the same phenomena I associated with 'language' B...The biological section is all in one 'language' (B) and one hand." [4, p.20ff.]

While he finds indications of different hands and "languages" in other sections of the manuscript (the pharmaceutical, astrological, and "recipe" sections), these seem much less distinct and clear-cut. It should be noted that in using the word "language" in this context, Currier does not necessarily mean to imply that he has found different underlying natural languages (e.g., Greek as against Latin, or German as against French). He is referring to patterns of statistical characteristics that seem to be consistently associated with Hand A as opposed to Hand 8: certain symbols are more likely to occur together or to appear more frequently in certain positions in the "words" of the Voynich text in folios showing one hand than in folios showing the other. An inspection of his extensive monographic, digraphic, and trigraphic counts, and his studies of symbol clusters in various positions of a "word", has convinced him of the presence of at least two clearly distinct bodies of text. In these two corpora the symbols show certain consistently different and characteristic distributions, associated with the visible differences in writing style and formation of symbols marking the hands of two different writers (writer A and writer B). Currier refers to the two bodies of text as "languages" A and B. In summing up his findings, he indicates that he feels quite certain of at least five, and perhaps as many as eight, different hands in the manuscript as a whole, but only two statistical "languages".

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This, then, is the exciting hypothesis put forward by Currier. Several of us, after attending his presentation, confirmed his suggestions to our own satisfaction by replicating his original procedure of choosing some pages showing obviously different writing styles in the large herbal section (where the contrasts between scribes A and B are especially striking), and verifying both the consistent differences in hand and certain clear accompanying differences in symbol patterns. Nevertheless, since so many other approaches to the problem posed by the Voynich Manuscript have been fraught with subjectivity and self-delusion, it seemed important to place Currier's findings on a more objectively demonstrable and secure basis, and to attempt to confirm or disconfirm them by an independent statistical study.

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Cluster Analysis

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In considering statistical tools for investigating Currier's hypothesis, I decided upon that of cluster analysis as an appropriate method. Cluster analysis algorithms are available as computer programs and are widely employed in the social and natural sciences for classifying collections of objects into subsets based on similarities and dissimilarities with respect to a list of scores or observations. The methods can also be used to reveal which of a group of objects is most like another single object in the group. So long as a set of observations has been made, such that every object under study has been scored, rated, or labelled for all the same properties or "variables", the clustering techniques can be applied to reveal subgroups among the objects. Within each cluster, objects are more like each other than like objects in other clusters. This methodology seemed to me a good choice for revealing the sets of similar pages within the Voynich manuscript that Currier's theory called for, if in fact they were present. For the reader interested in knowing more about it, a number of more or less readable works are available. Cluster analysis has been investigated by R51 for possible applications to Agency problems, and two excellent survey papers by Douglas A. Cope provide a summary of various clustering algorithms [3], and multiple scaling and related techniques [2]. A number of good reference works are available in the open literature; two I found particularly useful were Everitt 1974 [5] and Anderberg 1973 [1].

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There are numerous ways of carrying out cluster analysis, and the published computer programs embody various combinations of these, considered by their designers to offer some special advantage for certain applications. In general, however, the analysis involves the following stages: 1) deciding upon a group of objects that constitutes a good sample of the groupings or clusters hypothesized by the analyst; 2) deciding upon the observations to be made across all the objects; 3) taking the measurements, scores, rankings, labellings, etc. of each object with respect to each observation; 4) choosing a measure of "distance" (dissimilarity) or, alternatively, a measure of association (similarity) appropriate to the case; 5) computing the distances (or associations) between each object and every other with respect to the observations; and, finally, 6) applying the clustering algorithm to the triangular matrix of distances or associations resulting from step 5. The clustering procedures may be "agglomerative" (beginning with one object and iteratively joining other objects to it to form a cluster, as if crystalizing around a nucleus), or "divisive" (starting with all the objects in one big group and successively splitting them into dissimilar subgroups until no further splits can be made). Within the agglomerative methods, there are further choices among methods of linking new objects to existing clusters: "single linkage" methods focus upon the dissimilarity between nearest neighbors in a cluster, "complete linkage" methods upon the dissimilarity between the farthest neighbors, and "average linkage" methods upon the average dissimilarity among neighbors. As might be imagined, some methods are better at stringing out objects in long, thin chains, while others are better at dealing with globular clumps.

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Unavoidably, as is the case with many sophisticated statistical tools, there is a real danger of imposing spurious structure upon the data if the techniques are badly chosen or unintelligently used. A factor analysis or multiple correlation method will find "factors" of some sort in any data, however difficult they may be to interpret or put to use. Similarly, a cluster analysis will always find clusters, and it is up to us to pay attention to the indications of significance (the relative compactness of the clusters, the strength of their internal "bonds", and the relative distance between clusters) as shown by the statistical measures which the programs provide as a part of the printout. The interpretation of cluster analysis results is unavoidably circular; we propose a certain structure in the group of objects under study, we perform the computation, and we are happy if we see what we expected, or at least something that makes sense in terms of our original hypothesis, however revised. If the clusters we get bear little or no relation to any groupings we expected to see, and we can make little sense of them in the context of our understanding of the problem, we have some indication that our hypothesis about clusters in the data was not confirmed, but an attempt to reason from such unexpected and apparently meaningless structures backwards to the data may prove unrewarding.

I will not attempt here to go into the details of the cluster analysis algorithms or the various methods of computation; I urge the interested reader to consult the references mentioned above. Instead, I will provide some varied examples of applications in which cluster analysis has proven useful, as a means of communicating the "flavor" of these methods. A frequent use of cluster analysis is in studying the genetic similarities among species of plants or animals, based on some set of chemical or physiological properties. Cluster analysis has been employed in statistical pattern recognition, to discriminate printed letters, geometric shapes, or other visual forms. In archaeology, it has been used to classify groups of artifacts gathered by surface collection over a site; clusters of similar objects concentrated in certain areas within the site were found to indicate different human activities ("women's" activities: cooking, spinning, making pots, vs "mens'" activities: weapons manufacture, hunting, herding). A particularly interesting application of cluster analysis to Egyptian Archaeology, published in a recent issue of Science, deserves special mention [6]. I will describe it at somewhat more length, since it demonstrates so dramatically the usefulness and power of this methodology when intelligently employed.

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In 1898, a large cache of Egyptian royal mummies was found in the Valley of the Kings; these mummies, having been plundered and damaged by tomb robbers, had then been gathered together by a later Egyptian ruler, rewrapped, and deposited in two new hiding places. In the process of reburial, the identities of certain mummies were obscured (at least for the modern archaeologist). One in particular, referred to by archaeologists as the "elder lady", or more objectively, as Egyptian Museum Catalog Number 61070, was particularly interesting since it appeared from certain evidence (e.g., the position of the hands) to be that of a gueen. It was suggested that this lady might be Queen Hatshepsut or Queen Tiye (mother of the heretical pharaoh Akhenaton). A set of coordinated studies were undertaken, including data from conventional full-body x-rays, standardized x-rays of the head known as "cephalograms", and scanning electron microprobe comparison of hair samples known to be from Queen Tiye and those from the unidentified mummy. Several different cluster analysis algorithms were applied to sets of scores obtained from cephalogram studies of the unknown lady and ten other mummies of Egyptian queens. The analysis showed clearly that the head measurements of the unknown matched those of Queen Tiye's mother more closely than those of any other queen. This finding was strongly supported by the close match between the hair samples known to belong to Queen Tiye (and obtained from a keepsake in the tomb of another family member) and hair from the unidentified "elder lady".

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Application to Voynich Manuscript Pages

Selecting the objects. I was fortunate enough to have at my disposal a large corpus of text from the "herbal" and "biological" sections of the manuscript, transcribed according to the alphabet designed by Currier for computer processing of the Voynich symbols. Currier stated that he had found no page to be broken by a change of hand or "language", so that a set of samples, each taken from the text of a single page, should provide an appropriate test of Currier's theory. I selected forty segments of text, consisting of the first 350 to 400 characters from each of forty different pages. According to Currier's view, the text of these pages should fall into three major classes: herbal pages in "language" A and hand A, herbal pages in "language" B and hand B, and biological pages in "language" B and hand Z. These three classes will be called Herbal A, Herbal B, and Biological B for short in the remainder of this paper. Figure 6 shows a summary of pages from which samples were chosen.

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Making the Observations. I decided upon a simple monographic frequency count as a good starting point, since Currier had found a clear difference in the distribution of individual symbols between "languages" A and B. I made forty monographic distributions, one for each of the selected pages, including roughly the first 350 to 400 characters on each sample page (many pages did not contain more than 400 characters, and I wished the samples to be more or less equal in size).

Choosing a Measure of Association. Since my data consisted of frequency counts applied to a set of mutually exclusive, exhaustive events (the symbols of the Voynich script "alphabet") I could avoid the many scaling and normalization problems afflicting investigators employing cluster analysis for sets of observations comprising disparate measurements. My frequency counts constituted a set of discrete, countably infinite or finite variables, on a scale having a zero point and permitting proportional measurement (i.e., if xi and xj are two counts within one distribution, we can say that xj is n times as large as xi). Therefore, I could consider my analysis to involve a "ratio" scale, the strongest of the four possible scales (ratio, interval, ordinal, and nominal) on which observations can be made. This left me free to use a wide variety of cluster analysis programs, employing various association measures. The analysis takes place in the context of a sort of abstract "measurement space" or "metric", within which the objects (manuscript pages represented by frequency distributions) are "located" at various "distances" from each other to form the clusters. Different programs may use any of several possible association measures, among them the Euclidean distance measure, the "city-block" distance measure (both measures of distance, or dissimilarity), and the correlation coefficient (a measure of similarity).

Computing the Association Matrix and Clusters. Through the courtesy of Douglas A. Cope, R51, I was able to obtain runs of four different cluster analysis programs and one program for multiple scaling. These programs were as follows: the PEP-1 Guttman-Lingoes Graph Theoretic Clustering algorithm; HICLUS (Hierarchical Clustering), an agglomerative method using single and complete linkage; TAXMAP-2, an average and single-linkage approach to mode-seeking; Ling's (K,R)-clustering Algorithm, a hierarchical k-linkage method; and MINISSA (Minnesota-Israel-Netherlands Integrated Smallest Space Analysis), the multiple scaling program. These programs have all been adapted for the CDC-6600 computer by Mr. Cope and his colleagues, and are described in his papers [2, 3]. His ingenuity and helpfulness to users in applying the techniques to their problems and interpreting the results have also been a major asset.

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I needed only to supply a hypothesis and the setof forty frequency distributions, and the programs then carried out all the computations of associations, finding the clusters, and providing statistical estimates of confidence for the strength of the clusters or the program's representation of the data. In general, each of the cluster analysis programs found a lower triangular matrix of associations (correlation coefficients in PEP-1, HICLUS, and the K,R Algorithm, and city-block distance in TAXMAP). Each association in the matrix measured the relation of one Voynich Manuscript page, as represented by its monographic counts, to another single page. Transformations were then applied iteratively to the rows and columns of this matrix so as to emphasize the similarities and differences between pages. In some cases, the programs actually shuffled the rows and columns to bring like objects closer together in a final output matrix display; this was true of the (K,R) algorithm. As each cluster was found, a confidence measure was computed and associated with it in the program output as an aid to interpretation. The MINISSA program employed a somewhat different statistical model of the data; instead of finding clusters of objects in an abstract "space", it mapped the "locations" of the objects within such a space: a "Euclidean metric space", whose two dimensions may be assigned a meaning in relation to the hypothesis held by the investigator.

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Interpreting the Results. Many programs provide a helpful graph or plot of the numerical results; in some cases, additional programs can be run on the outputs of a clustering algorithm to rearrange matrix rows and columns or provide graphic displays to aid the researcher. These visual representations are extremely helpful, and I found them almost necessary; unadorned lists of cluster members, ranged in dense rows down the pages of printout, can prove tedious and confusing indeed to the researcher. Since both the clustering and multiple scaling techniques are essentially applying a spatial model or "metaphor" to the problem posed by the investigator, a twodimensional graph or plot is often an appropriate display. Another useful display is a "tree" or "dendrogram" showing the familial relationships among the objects. Each program provides statistical measures, associated with the clusters, the nodes of a dendrogram where each cluster is split off, or with the entire representation of the data. These measures are intended to enable the researcher to assess the confidence he may have in the findings of the program. In the next section, the outputs of the five programs will be described in detail.

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Results of the Analysis

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1. PEP-1 Graph Theoretic Algorithm. PEP-1 provides a list of clusters in order as each subset of the objects is partitioned off from the rest. A "family tree" (shown in Figure 1) can be drawn from this output. At each node of the tree where a cluster or a single object branches off, an "edge connectivity probability" is shown; this is an estimate of the likelihood that the split could have happened by chance. Thus, the lower this estimate of probability (on a scale of 0.0 to 1.0), the more confidence we may have in the contrast of the pages in the cluster against the rest of the pages outside of it. The upper "stem" of the family tree shows a loose sequence of small clusters and isolated pages, all from the Herbal A pages except for two samples, TL and HD (pages 94 and 76), from Herbal B. The tree then separates into two main branches; the left branch seems to correspond roughly to Currier's "language" B, since it contains most of the B pages and none of the A pages; all the Biological B pages are clustered together at the lower left, along with one Herbal 8 page (59); another Herbal B page (79) is alone, and there is another cluster of seven Herbal 8 pages just above. The right branch contains the rest of the Herbal A pages and one oddball Herbal B page (sample TE, page 60). The "probability" statistics seem quite low everywhere except in the right branch, where they suddenly jump up from near zero to .5. Thus, this right branch, while strongly split away from the rest of the tree, seems very weakly subdivided, and should probably be regarded as one very diffuse cluster.

2. Ling's (K,R) Algorithm. Figure 2 shows two triangular matrices output by the program. The rows and columns of an original "similarity matrix" containing correlation coefficients have been rearranged to place similar manuscript pages closer together and dissimilar pages further apart. Symbols made up of overstrikes were printed in the cells of the matrix, so that the higher correlations are darker; thus the clusters showed up as darker triangles along the main diagonal of the big matrix. A matrix was produced for each of several "bond sizes" or values of a threshold K applied to links between objects in clusters. Higher levels of K represent more restriction

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on clusters, and a requirement for more strongly bonded clusters. Thus, for bond size 1 (k = 1), every object in a cluster must be joined to at least one other object in the same cluster by a link of the required closeness, and clusters must have at least two members. For bond sizes 2 and 3, every object must be linked to at least two or three others in the same cluster, and clusters must have at least three, or four, members. While nigher bond sizes could have been required, the algorithm produced three matrices, one for each of bond sizes 1, 2, and 3. The first two matrices were essentially alike, and were as shown in the left drawing of Figure 2. Except for four anomalous Herbal B pages (59, 60, 76, and 94), there appear to be three relatively clear clusters corresponding to the three classes of pages Currier's theory calls for: Biological B, Herbal B, and Herbal A. The matrix for bond size 3 is somewhat different; it seems clearly to show only two major clusters, corresponding to Currier's two "languages", with the exception of the three Herbal B pages (60, 76, 94) and one Biological B page mixed in with Herbal A.

HICLUS Agglomerative Cluster Analysis. The output of HICLUS includes a dendrogram in the form of a display similar to a bar graph. Boundaries between clusters can be seen where low points in the graph-like display leave deep columns of white paper between the relative peaks of the clusters. An accompanying page associates sigmages for cluster tightness to each cluster; the higher the statistic, the tighter the cluster. The vertical dimension of the graph shows descending correlation coefficient values, so that objects associated in a cluster at the top have higher correlations, while the correlations decrease down the page. Figure 4 shows a rough redrawing of this bar graph. We see a rather strong cluster on the far right containing seven Herbal B pages; in fact, they are the same seven as appeared in the 7-page Herbal B cluster on the left branch in the PEP family tree. It has a correlation coefficient level of no less than .959, and a sigmage of 5.3. In the middle is the Biological B cluster, containing all the Biological B pages plus two from Herbal B (59 and 79); its correlation level without page 79 is .960, and its sigmage 7.97; with page 79, the corresponding figures are .947 and 8.47. The left half of the graph tails off into a very loose conglomeration of small clusters comprising all the Herbal A pages with two Herbal B pages (60 and 76). Page 94 is alone as an "outlier" (an object not clustered with any other in the set) on the far right.

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TAXMAP-2 Clustering Program. While TAXMAP does not provide a graphic display of its results, Mr. Cope kindly ran its outputs through another program to create a two-dimensional "vector plot" similar in appearance to that produced by the MINISSA program discussed below. It should be noted at the outset that TAXMAP, alone of the programs run on my data, did not employ a correlation coefficient as a measure of association. Instead, a very different kind of measure was used: the "city-block" distance. In effect, this means that much of the information in my frequency count data on a ratio scale was disregarded; instead of comparing the profiles of peaks and valleys along the frequency distributions, a much cruder, less sensitive, and perhaps less appropriate measure of distance was used. This consideration may help to explain the differences in the results of TAXMAP as contrasted with those of all the other programs. The only cluster that shows up at all clearly contains ten Biological B pages. The Herbal A and B pages are scattered among small clusters and isolated individual pages in a manner that tells us relatively little that is useful.

MINISSA Multidimensional Scaling Program. Figure 3 shows a drawing adapted from the "vector plot" produced by the MINISSA program. Whatever feature of the metric space is represented on the horizontal axis, it seems to be related to the differences between Currier's "languages" A and B. The vertical axis is somewhat more problematical; it could pertain to the "subject" difference between Herbal abd Biological, or even to the "hand" difference between Currier's hands B and Z within "language" B. All the Biological B pages fall within a small, compact region in the lower center which also contains one anomalous Herbal B page, 59. A compact region above contains the same seven Herbal B pages we have seen grouped in a strong cluster by three of the four clustering algorithms. Page 94 is all alone on the extreme right, and page 76 is alone at some distance above and to the left of center, while pages 60 and 79 are around the edges of the Biological B region. All the Herbal A pages are scattered loosely over the leftmost third of the plot. In general, this program, in spite of its reliance on a somewhat different statistical model, appears to confirm the findings of PEP, the (K,R) Algorithm, and HICLUS.

- 11 -

1140 B. 4 4000

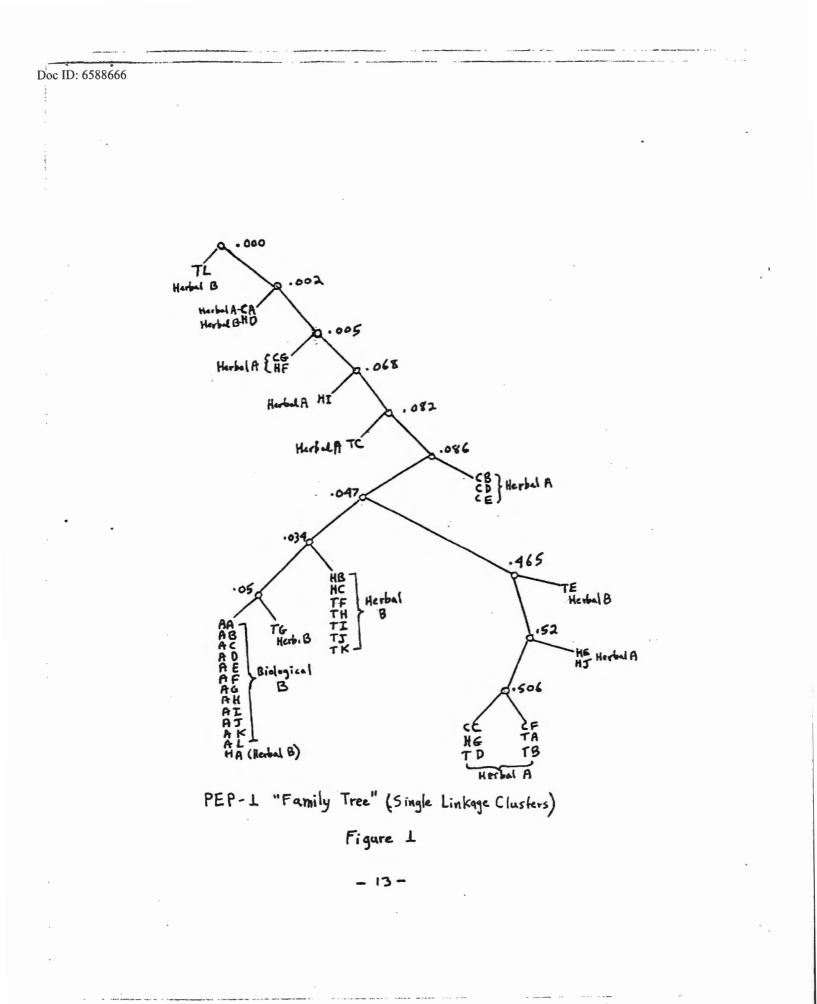
Conclusions

Figure 5 shows a rough summary of the groupings of pages found by the five programs. Except for TAXMAP, they all seem to reveal the same picture: A strong Biological B cluster including all the Biological B pages along with one Herbal B page, 59; another cluster containing seven or eight of the Herbal B pages; and a loose association of Herbal A pages mixed with the same few anomalous fugitives from Herbal B. The transcribed data at my disposal do not contain a broad enough sampling from all sections of the manuscript to support a full-scale analysis attempting to study all the "hand", "subject" and "language" contrasts. Nevertheless, the results of this exploratory study clearly seem to be sufficiently encouraging to warrant a more complete analysis when more text has been transcribed.

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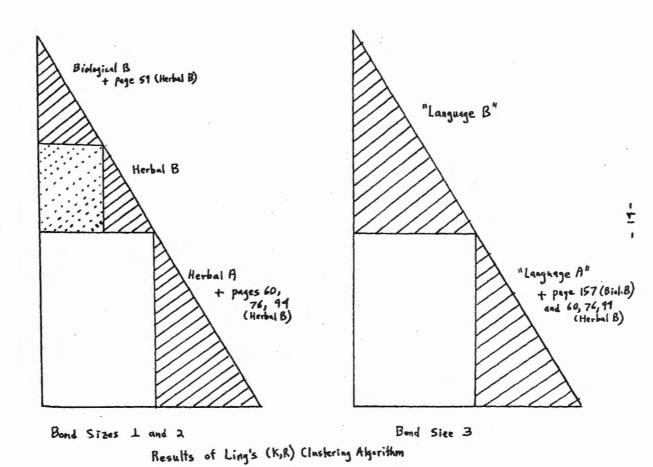
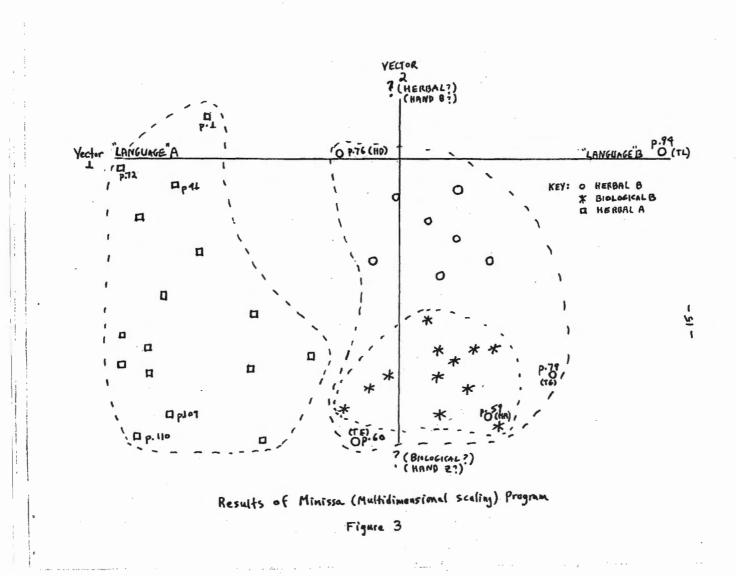
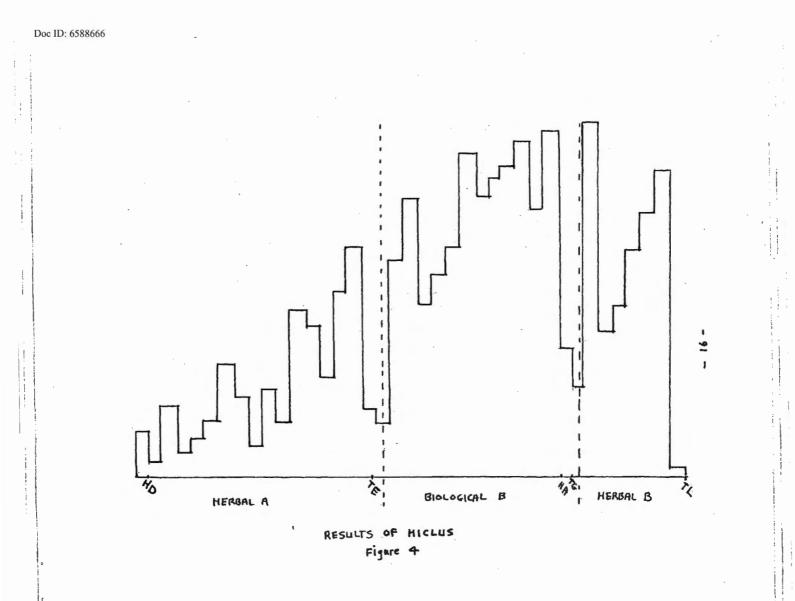
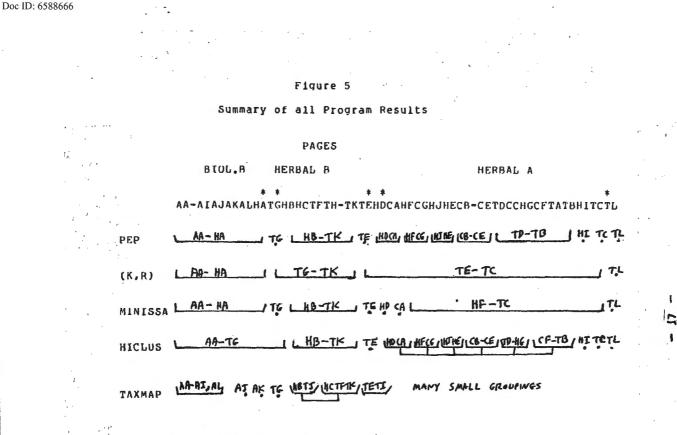


Figure 2







I.

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* indicates Herbal B pages which are sometimes separated from other Herbal B

and the second s

Figure 6

Summary of Samples from Manuscript Pages

** *... · ·* ** * · · ·

		Currier'	s		
no.	code	page no.	"subject"	"hand"	"language"
		2030			
1	AA	147			
	AB	156			
2. 3. 4.	AC	149			
	AD	151			
5 6	AE	148			
6	AF	150	Biological	Z	В
7	AG	152			
8	AH	153			
9	AI	154			
10	AJ	155			
11	AK	157			
12	AL	158			
13	CA	001	lie a h a l		
14	CB	005	Herbal	A	A
15	CC	015			
16 17	CD CE	045			
18	CF	039			
19	ĊĠ	041			
20	HA	059			
21	НВ	075			
22	HC	065	Herbal	В	B
23	HD	076			
24	HE	068			
25	HF	072			· ·
26	HG	095			
27	ΗI	110			
28	НJ	081			
29	TA	082	Herbal	A	A
30	тв	057			
31	TC	109	·		
32	TD	096			
33	TE	060			
34	TF	066			
35	TG	079			
36	TH	083	Horbal		В
37	TI	084	Herbal	В	D
38	TJ	089			
39	TK	090			
40	TL	094			

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3

28 October 1970

Dear Dr. Huttenhain:

By now you may be thoroughly immersed in the Voynich Manuscript and my enclosure may be of some use. Several of our people have made casual off-duty examinations of the manuscript but with no promising results. We have not attempted to mount an all-out or a systematic attack upon it although from time to time such an attack has been talked about, particularly by some of our alumni. The best available material on the manuscript is the work of Brigadier Tiltman whose reprinted article is enclosed.

I have not seen Dr. Going since he assumed your old position but I did have the occasion last week here in Washington to ask Brigadier General von Seydlitz to remember me to you. We had what I thought was a mutually profitable discussion.

With every wish for your good health and a busy and enjoyable retirement.

Sincerely,

Louis W. Tordella

Approved for Release by NSA on 07-18-2017, FOIA Case # 101982

Approved for Release by NSA on 07-18-2017, FOIA Case # 101982 2 march 1970 Gear Dr Tordella It was a supprize and a pleasure when me fuces presented me with the gift of the three volumes with the photocopies of the Voynich Manuscrypt on 22 Licember last year Please accept my sincere gratitude for this singular gift Of course I will act in such a way so that no difficulties will arise I will aways come to you first whenever 1 acquere new discoveries. The

hope of new descoveries is small is any case since outitanding experts in your circle have to far not found any solution revertheless of well be happy to apply myself to this publim especially in view of my retirement on I Televary I now have the time, as you have doubtless heard, Ar Going is my successor. Perkaps it would be useful if I could discuss this at some Juture time with one of your experts

-2-

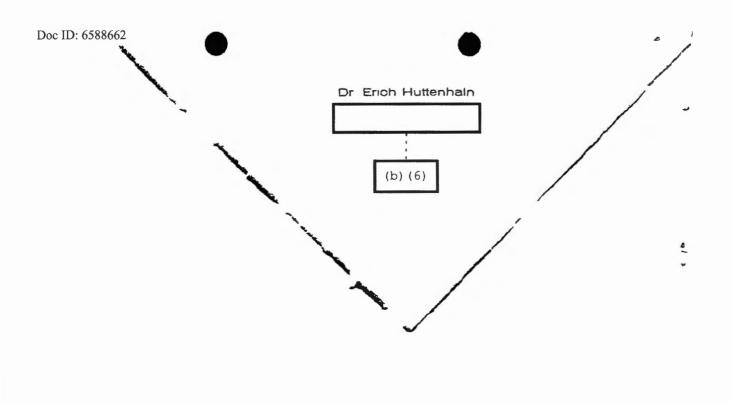
who has worked on the problem after I have studied the menuscripts for some time. - Clease convey my gratings and fest wester to Mr Raven as well I have foud memories of his visit to my office several years ago. Please accept, my sencere thenks and best wiskes Kespectfully yours --______/5/_____

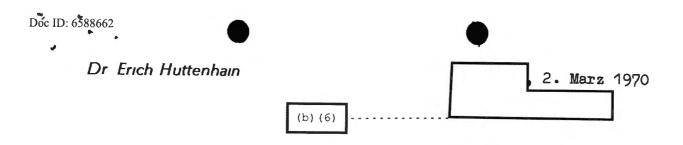
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Doc ID: 6588662	MEMORANDUM FOR. Dr Louis Tordella Deputy Director, NSA	
	Lou,	
	We received this in our pouch today for passing to you.	
	Carleton Swift	
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Dr Tordell**a**, pers





Sehr geehrter Dr. Tordelle!

Es war fur mich eine Überraschung und große Freude, als mir Mister Lucas am 22. Dezember des vergangenen Jahres die 3 Bande mit den Photokopien des Voynich-Manuscripts als Geschenk uberreichte. Nehmen Sie bitte meinen sehr herzlichen Dank fur diese einmalige Gabe entgegen.

Selbstverstandlich werde ich mich so verhalten, daß keinerlei Schwierigkeiten auftreten. Ich werde mich immer zuerst an Ihre Stelle wenden, wenn ich neue Erkenntnisse gewinnen sollte. Die Hoffnung zu neuen Erkenntnissen ist allerdings gering, da in Ihrem Bereich ja von erlesenen Fachkraften bisher keine Losung gefunden werden konnte. Dennoch werde ich mich gerne und immer wieder mit dem Problem beschaftigen, zumal ich seit meiner Pensionierung am 1. Februar die Zeit dafur habe. - Dr. Going ist, wie Sie sicherlich schon gehort haben, mein Nachfolger.

Vielleicht ware es zweckmaßig, wenn ich nach langerem Studium der Schriften mit einem Ihrer Fachkrafte, die sich mit dem Problem beschaftigt haben, zu gegebener Zeit sprechen konnte.

Darf ich Sie bitten, auch Mister Raven meine Gruße und Empfehlungen auszurichten. Ich erinnere mich gerne an seinen Besuch vor vielen Jahren in meiner Dienststelle.

Mit nochmaligem sehr herzlichen Dank und allen guten Wunschen verbleibe ich

Ihr Ihnen sehr ergebener

Hüllenham.

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Washington, D. C.

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NEW RESEARCH ON THE VOYNICH MANUSCRIPT

PROCEEDINGS OF A SEMINAR

30 November 1976

Washington, D.C.

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EDITORIAL PREFACE

These notes contain the proceedings of a one-day seminar on the Voynich manuscript, held in Washington, DC, on 30 November 1976. With the exception of Dr. Fairbanks' presentation (of which he provided a written version for inclusion in these proceedings), all the material was transcribed by me, with only minor editing, from a taped record of the sessions. I apologize in advance to those speakers during the discussion period who could not be identified (because I could not recognize their voices on the tape). I apologize also to anyone whose comments I may have inadvertently omitted, or who feels that his remarks may not have been transcribed correctly. I hope that these notes will serve as a faithful and valuable record of this seminar, and of the many interesting and important methodological points that were raised during the discussions.

M. E. D'Imperio

I.A. General Introduction. Vera Filby, Sponsor.

Good morning, and welcome to our seminar on the Voynich manuscript. A year ago this month, Brigadier Tiltman, who is here with us today in the front row, gave a talk on the Voynich manuscript - the most mysterious manuscript in the world. This talk so inspired two of our members that they have since engaged in serious research into the problem, within the rather considerable range of their own specialities: cryptanalysis in the one case and linguistics in the other. I knew of their work, and was keeping up with it, and it seemed to me that with reports on their research, the Voynich would again make an appropriate topic for a program. It seemed to me that there is never likely to be a better collection of the right kind of brains, talent, and training than we have right here and right now, and so I proposed this seminar. The history of attempts to break the "Cipher Manuscript" (as Wilfrid M. Voynich himself called it) has been a history of frustrations and even disasters, but maybe we can strike the right spark today; maybe we can open up the first real cracks.

I would certainly be proud if our initiative were to make such a contribution to the scholarly world; but if that is too sanguine a hope, we have the more modest wish that Mrs. Friedman offered in her letter to me a few days ago in response to my invitation to her to attend. She didn't feel well enough to come, but she did write, and she said, "Greetings to all of you, and may you be crowned with, at least, a glimmer of hope." And maybe

that's the best that we can really expect.

Now Ladies and Gentlemen, it is my pleasure to introduce our moderator for the Voynich seminar, Miss Mary D'Imperio. Mary is in the final stages of completing a monograph on the history of research on the Voynich manuscript; she calls it "The Elegant Enigma." It is, I think, a magnificently scholarly job, and eventually you'll all have a chance to read it. Mary has degrees in Comparative Philology and Classics from Radcliffe, and Structural Linguistics from the University of Pennsylvania. Her career has been with the Government since 1951. She is a linguist and cryptanalyst, but she thinks of herself mainly as a computer programmer, and it is this combination of talents that makes her so right for the enterprise that she and the other participants in the seminar will undertake today. There can't be, I think, anyone better equipped anywhere to take on the job that she's about to do, which is to lead this enquiry into the search for solutions to the mystery of the Voynich manuscript.

I.B. Introductory Remarks. M. E. D'Imperio, Moderator.

Before I present our featured speakers, I would like to give a brief introduction, for the benefit of those who may be unfamiliar with the problem of the Voynich manuscript. My remarks will center around two main topics: first, I will try to sketch, very rapidly, something of the history and physical nature of the manuscript. Then, I want to say something about the cryptanalytic problem posed by the manuscript, and some of the things that have made it so challenging and so interesting to so many people.

The Voynich manuscript itself has the shape of a small book, about nune inches long and six inches wide. Most pages contain colored pictures of plants and astronomical or astrological diagrams. Here are some slides showing some sample pages so you can get an idea of what they are like. (It was, unfortunately, not possible to reproduce the slides here. -Ed.) Some seem to be medical or pharmaceutical in nature, and feature naked human figures, mostly female. These figures have very plump and matronly shapes, and appear to be sitting, standing, or swinming amid a weird conglomeration of tubs, pipes, and other odd plumbing. No one, as far as I know, has gotten very far in figuring out what any of the pictures mean.

On almost every page, there is a lot of writing in brownish ink. It is very fluent, clear, and relatively neat, but it is in a writing system that nobody has, so far, been able to identify with any known language or culture.

The Voynich manuscript was discovered in 1912 by Wilfrid M. Voynich, a rare book dealer. He found it at the Villa Mondragone, near Rome, among other manuscripts which he was buying in a large lot. With it was a letter, dated 1665 or 1666, from a man named Joannus Marcus Marci to Athanasius Kircher, a well-known Jesuit scholar with a strong interest in cryptology. Marci was a scholar associated with the court of the Emperor Rudolph the Second in Prague. The letter said that Marci was giving the mysterious manuscript to Kircher, in the hope that he would be able to decipher it. The letter also said that the manuscript was thought to be by Roger Bacon, a philosopher of the thirteenth century in whose work there was great interest at Rudolph's court at the time.

Several people have claimed that they could read the cipher in modern times. The most famous solution was that of Professor William R. Newbold in 1921, which was completely demolished by Professor John Manly of the University of Chicago in 1931. Mr. and Mrs. William F. Friedman also had a part in the research which resulted in the disproof of Newbold's claim.

Since that time, although there have been several other claims to a solution, none has succeeded in convincing cryptologists or any other scholars that the mystery has really been solved. The elegant puzzle is still there today, waiting for all of us to try our hand.

The manuscript itself remained in the possession of Mr. Voynich, and after his death, in his wife's estate. It was purchased in 1961

by H. P. Kraus, another antiquarian bookseller, for the sum of \$24,500 in cash. He tried to sell it, reputedly for as much as \$100,000 and later \$160,000, but apparently couldn't find a buyer. In 1969, he presented it to the Beinecke Rare Book Library of Yale University, where it now is.

Now, I would like to say a few words about the analytic problems presented by the Voynich manuscript. Why is it such a persistent and fascinating problem? Why has no one succeeded in solving it in the nearly fifty-five years since its discovery?

First, nearly everything about the problem is an unknown. We don't know what country or even what part of the world the manuscript came from. We don't know what language underlies the text, or even if it is a natural language at all. We have no sure knowledge of the date of its origin, although most students agree it cannot be much earlier that 1450 or much later than 1550. As far as we can find out, no scientific study has ever been made of the vellum or the inks, and no paleographic studies have been made of the writing. We have no clue about who the author or authors could have been, or why they wrote it.

Attempts to discover other manuscripts with similar writing or drawings have been completely unsuccessful. The Voynich manuscript seems to be a unique document. We have had little or no success in figuring out what the pictures mean, or using them to break into the text. There is, in short, nothing that can serve as a crib or Rosetta Stone.

The scribe or scribes of the manuscript have been fanatically careful to leave nothing in the clear to give us a break-in point to the text. While there are a few scribbled phrases in other writings on some pages, they are so crabbed and faint that nobody has been able to make much out of them. They have never, so far as we can tell, been examined under special lighting or otherwise studied scientifically as they should be to see what, if anything, they do say.

On top of these very general difficulties, there are some basic analytic problems that hamper us in attacking the Voynich text. First there is the writing system or alphabet; we simply don't understand how it works. The symbols seem to be built up from smaller units in some way, but we can't come up with a convincing analysis into basic elements. So we don't really know how many letters there are in the alphabet; some students see as few as 17, while others see as many as 39. Each researcher has his own theory about the alphabet and his own transcription. Then there is the question of what the cipher units are and what plaintext units they represent. Are we dealing with words as wholes, syllables, mixed-length strings, or single letters? Finally, there are very few patterned repeats in the text that can give us a clue to the workings of the system. While many single word-like elements are copicusly repeated throughout the text, we have had little success in finding any parallel elements in the context surrounding occurrences of similar groups.

There are approximately 250,000 characters of text in the manuscript. No one has apparently ever succeeded in completing a machine index or concordance of the entire corpus. For the most part, only small samples of 5,000 to 20,000 characters have been studied in any detail. A hand concordance was made by Father Petersen of Catholic University; unfortunately, this is with the Friedman collection in Lexington, Virginia, where it is not readily accessible to many students.

These are some of the reasons why the Voynich manuscript has been rightly called a Mount Everest for cryptographers by some, and a work of the Devil by others who have struggled in vain with its puzzles within puzzles.

Theories that have been held by various researchers concerning the nature of the Voynich text fall into the following five general categories:

First, some think the text is in a natural language, not enciphered or concealed deliberately in any way, but simply written in an unfamiliar script. Mr. Child's theory, which we will soon hear him describe, is based on this assumption.

Second, some maintain that the text is a form of natural language, but enciphered in some variety of simple substitution with various complicating factors. The theory of Dr. Robert S. Brumbaugh of Yale University, announced in 1974, is of this nature.

Third, some think the text is not in a natural language at all, but rather in a code or synthetic language like Esperanto, using an invented alphabet for further concealment. William F. Friedman

was a proponent of this theory, and Brigadier Tiltman has also favored it.

Fourth, some believe the Voynich manuscript is an artificial fabrication, and much of the text is randomly-generated, meaningless padding. Within it there is some quantity of decipherable text. Dr. Brumbaugh also holds this view; he feels that the manuscript was manufactured in the sixteenth century by an opportunist for the specific purpose of peddling it to the Emperor Rudolph in Prague. According to this theory, while most of the text is meaningless and will never be read, some portions can be deciphered if we know how.

Fifth, there are some who believe that the text is all completely meaningless doodling, produced by a mentally-disturbed or eccentric person. According to this view, we will never make any sense out of it, no matter what we do. Doris Miller, a recently retired colleague who has returned to be with us today, has presented an eloquent case for this theory.

With this introductory sketch to set the stage, I will now introduce our first speaker.

James Child

Mr. Child received his A.B. in Germanic Languages and Literatures from Princeton University, and an M.A. in Baltic and Slavic Philology from the University of Pennsylvania. He has had a long and distinguished career as a linguist, both in the practical and theoretical aspects of the field. He has worked as a translator, has taught many basic language courses in a wide range of languages, and has been active in the design of language proficiency tests for job placement and career development. His interest in the Voynich manuscript was aroused by Brigadier Tiltman's lecture in November of last year. He has published two brief articles on his theory concerning the manuscript in periodicals circulated within his organization. We are happy to have Mr. Child here today to tell us of his approach to solving the mystery.

I.C. A Linguistic Approach to the Voynich Text. James Child.

I sincerely hope that my work doesn't go the way of poor Newbold, or Manly, who demolished Newbold's theory but didn't do any better himself. This seems to have been the case for anyone who has had the gall to get anything out of the manuscript: nobody comes out looking very good, but then nobody is put down permanently either. It is still an open case.

There is still a lot of work to be done, but I do believe I have an opening wedge into the manuscript. I feel that I know at least a few things about the nature of the underlying language, which I believe to be human language, plaintext, an Indo-European language, and a language in the Germanic family. Beyond that I would be rash in going.

Assuming this is a natural language, what kind of distribution would you get? First, you would expect words and characters to fall in certain positions. Finding a sequence of four or five letters, all of which you had assumed were vowels, occurring in a row would argue against a simple cipher. But if you find reasonable sequences of vowels interspersed among consonants, there would have to be a very sophisticated enciphering mechanism to produce such text if it were not in fact plaintext. In the Voynich text, I believe we have a complex situation: vowel letters, consonant letters, and digraphs. The digraphs occur especially at the ends of words, tending to obscure the grammatical relationships. I will elucidate further later on.

First, I'd like to give you a notion of the procedure I've followed in trying to break this text. A few definitions are in order: they are linguistic in nature, and I'll try to make them as painless as possible for those having an aversion to linguistics.

1. <u>Morphemes.</u> All languages have sound combinations that represent meaning units. At a lower level, a sound sequence is just a syllable, but at some level you begin to have potentially meaningful units. Nevertheless, meaning is always <u>in context</u>. I have tended to approach the Voynich in this way: what are the bases and affixes (prefixes, suffixes, and infixes), and do they seem sound and reasonable in terms of the particular sort of language I assume underlies the system? These units are morphemes: values lexically and semantically possible.

2. <u>Lexemes</u>. Lexemes are the same values, but in context. Scholars cannot immediately zero in on meanings of words when they are studying a new language. They try to find what the parts of speech are, how they relate to one another, the alignments of nouns and verbs, and so forth. For example, short words or morphemes occurring in front of noun-like things give you prepositions; words linking noun and verb combinations can be conjunctions; and so forth. Once you have nailed down some of these, you try to specify certain kinds of nouns (for example, the declensions in Indo-European languages). You try to refine the nouns and relate them to the things you are calling verbs, to establish, for example, a noun plural going with a third person plural verb form, etc. These are going to be lexemes: meanings of morphemes in particular contexts.

3. <u>Semanes</u>. Our final definition, that of the sememe, stands for the concept that the writer is trying to express and get across to the reader; the idea behind the forms (morphemes) and the forms in context (lexemes).

This is the theoretical approach I've used to attack this problem. I tried early on to establish, first of all, the letter patterns: the morphemes. I came to the conclusion that the morphemes I found were valid for a human language in the Indo-European family and in the Germanic family in particular, and that they seemed to play the proper role as lexemes.

(Could I have the first slide please? By the way, I want to thank Mary D'Imperio for doing these; my handwriting is absolutely abysmal in my native script, so far be it from me to take on the Voynich!) (See Fig 1.a.) One of the first things I noticed was this place at the top of the slide, from folio 114, which has "OOR." If that could be considered a way of lengthening the "O," the word would be a good preposition in the North Germanic language family. The next group after that would have to be a noun by definition; what kind of a noun, Heaven only knows. But I could add the information that the preposition "OOR" would require the dative case. The final letter of that next word is a consonant in my reconstruction: either "D" or (the sound at the beginning of the English word "the" -Ed.), so it's not a dative ending. It could be a feminine noun with a zero ending - possible for some North Germanic languages. The next group, which I read as "OG," is still the conjunction "and" in most Scandinavian languages. It

appears in other Germanic languages as "auch," "also," (although the word for "and" in West Germanic is either "and" or "und"). This suggested tentatively establishing the language as North Germanic. Here you have a preposition, a noun, and a conjunction, so you need another noun, to give you something like "From ---- and ----."

This approach gives the whole thing an extremely algebraic appearance. In English, if you did the same thing, and left out all the content words, keeping only the function words (like "the," "of," "and," etc.) and the inflexions (the "-ing's" and "-s's" and "-ed's"), you would get something like this: "Somebody or something) is doing, will do, or did do (something) to (someone) at or in (some place)." You, the listener, may regard this as absolutely idiotic, and in terms of a message, of course it is. But in terms of the informational process it is not at all meaningless, and is in fact quite instructive. You have, in fact, to reconstruct something or this sort when you are working with an unknown language, to prove, or at least to suggest strongly, that you've got a real language. Taking words out of context, by themselves, obviously won't do.

Now on the second part of this slide (Fig 1.b.) we see a repeat of the conjunction "OG." In front of it we have a word I assume to be "THOR" or "TOR." That letter at the beginning could stand for "TH" or "T"; this sort of thing was most common in German manuscripts. Old High German is a living horror; in ways much worse than the Voynich: you can have eight or nine different spellings for words or names. So the fact that the first letter of

"THOR" may be "T" as well as "TH" doesn't bother me very much.

After the "OG," it looks as if we might have a parallel noun; perhaps another god, or simply another man's name, depending on who "Thor" actually was. I thought this might be "THRUTHER." Thruther is, in some legends, the daughter of Thor, in others simply the hammer of Thor. It would seem a good guess to try to reconstruct morphemes and put more lesenes in, so I went on that assumption. Incidentally, the first word here, "FRHA," looks very reasonable to me; the "R" doesn't look like a final "R," because it's apparently a digraph: "R" plus short vowel "E" or "I." That equation holds up pretty well through the pages I've studied. I've given some consideration to nine or ten different pages; I haven't just stuck with one, which would be foolish.

Obviously, I wanted to look beyond simple noun collocations. I wanted to see if I could find some parallel syntax. In slide two (See Fig 1.c.), we have what appears to be a repeat of "THOR," and the second word I regard as "LIOFA," which would mean "beloved." We have a possible genitive plural with long "A" for the third word - a correct Scandinavian genitive plural. A repeat of "OG," "and," run together with "THOR"; more often than not the conjunction is run together with the following word. The first word in the second line may be read as "ALIA," "nourisher, he who nourishes." We would have to assume a Norse participial form for that. But that's rather shaky, and I'm quite dubious about it.

Down in line 18 (Fig 1.d.), I've tried to extend my procedure a little further. Those underlinings are adjective and noun. Incidentally, the noun plural forms (and I think I've isolated four different noun plural forms in this language) match Swedish very closely, better than any of the other Scandinavian languages (although my original assumption had been Danish). It seems indeed to be closer to a form of Swedish, but it's not pure Swedish either. I have conjectured - and this is a simple conjecture, nothing more - that what we may have here is a residue of Gothic; not the language of the Goths of Bishop Ulfilas' time in the fourth century, but the latter-day Goths, those people who settled Southern Sweden and parts of Northern Denmark. This may, perhaps, be their dialect. I don't know for sure - I just want to make a suggestion.

In slide three, at the top (Fig 1.e.), we have another nominative plural noun, then we have a plural third person form. The third person plural ending is usually " $-\bar{A}$," so this, I'm assuming, may be " $-N\bar{A}$." That final digraph " $-N\bar{A}$ " holds up pretty well in many places. So we have something like this: "These people or things, whatever they are, do something, whatever it is they do." Again, this is admittedly algebraic, but nevertheless, this is the procedure I followed. The bottom example on this slide (Fig 1.f.), has another nominative plural of a noun, then our conjunction "OG," then "THA," which is a good Norse demonstrative, and goodness knows what that last word is.

We'll go on to the last slide, and I'll try to wind up here. (Fig l.g.) We have the first two words in this line repeated over and over again on some of the pages I've studied. I'm reading them "GOTTAR REIDA." "Gottar" would be "the Goths." That, incidentally, would be the Swedish nominative plural today. "Reida," again a

third person plural form, is perfectly correct: the digraph for "RE," then long "I," the "8" letter again, which is our "9"; "REIDA," like German "reden," "to say or promounce." "And the Goths say..." I'm not sure what the next word - "GOTTEDA" - is, but the last two words could be "OF LATAIN," "in Latin," and then "RES AINA." "Res alma" is not a very good co-occurrence in Latin; it's perfectly good grammatically. I don't know about it as a phrase; it might mean a "tharitable thing," or a "good thing."

All of these examples are intended to be primarily an illustration of the method. A lot of these findings are obviously still going to be in doubt for some time, but I'm having a lot of fun with it! I think if you don't have fun doing something like this, a lot of the purpose is lost. I certainly appreciate everybody here coming to listen to my ramblings, and I guess we'll see a great number of you this afternoon at the later session.

Thank you very much.

a. Folio 114r, line 2:	Saud	007 C	ecco og og
		FROM	A~9
b. Folio 40v, lines 9, 10:		THOR OG	Mar Maud THRUTHAIR NO THRUTHER
c. Folio 58r, line L:	THOR LI	SFA	129 01108 98029 020108 OTHOR ALIA PL) AND THEA NOURSHER(?)
d. Folio 58r, line 18:	0801		Barat and (N. PL.) Noun
e. Folio 58r, line 21:	900	की संद	aid offeg
	(N. P	L.) (3 P	L.) 8
f. Folio 58r, line 22:	400	ut rage	orthy day
	(N.)	PL·)	AND (DEMONET)
g. Folio 107v, lines 10,			
4098ar 2089 4	Ac 84 at	Folland	2 1 9 12 K 9
GOTTAR REIDA G	•	•	
			"RES ALMA".

Fig. 1. Sample Readings (Mr. Child)

Voynich Symbol	Equivalent		
0	ö		
00	0		
4	a		
9	ā		
41	ĕ (ai)		
C	Ĩ		
<i>c</i> c	u or m		
a.	ī		
4	g		
4 8	gh (as consonant)		
	(after vowels, lengthens vowel)		
99, H	t, th (as in thing)		
8	d, d (as in the)		
や や や ひ ひ ひ ひ ひ ひ ひ ひ ひ ひ	p		
1 ² , 1 ²	f		
2	r		
5	8		
-	n, or n + short vowel		
2	11		
CT.	ri		
ヷ	is		
\mathbf{x}	? (possibly a Greek sound χ)		

Fig. 2. Symbol Correspondences (Mr. Child)

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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.

I.D. Some Important New Statistical Findings. Capt. Prescott Currier.

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 this 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.

<u>Two Hands and Two "Languages" in the Herbal Section</u>. 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.

<u>"Languages" A and B Statistically Distinct</u>. 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 <u>astrological section</u>, there seemed to be no real differences that I could detect. The <u>biological section</u>* is all in one "language" (B) and one hand. The next section in which I noted a difference was the <u>pharmaceutical</u> <u>section</u>. 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

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

section, two "languages" and two hands. The <u>recipe section</u> 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 notionably to the eye from that on 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 <u>four</u> or <u>six</u> hands all together at this point. The final section of the manuscrupt 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 <u>is</u> 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).

<u>A New Slant on the Problem</u>. 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 writing is consistent throughout, and is obviously the work of one man. Well, it obviously <u>isn't</u>, 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

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

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 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."*

<u>These Findings Should be Considered by Anyone Who Studies</u> <u>the Manuscript</u>. 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 <u>damn</u> significant. Anyone who attempts to work on the text without considering these, ignores them at his own peril. They are <u>there</u>, and they are very definite. No matter which one of the forms that Mary originally mentioned** 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

25 *See Appendix A for details. -Ed. **See pp. 7-8 above. -Ed. own minds any questions or discussions that you'd like to go into this afternoon. Thanks very much.

II.A. Introduction to Afternoon Session. M. E. D'Imperio, Moderator.

Dr. Sydney Fairbanks will probably need no introduction for many, if not most, of those present, but for the sake of those few who may not know him, I will say a few words of introduction. After some early years in England, Dr. Fairbanks entered Harvard at the age of fifteen. He somehow managed to combine with his Harvard studies adventures as an ambulance driver in France, Italy, and Palestine during World War I, for which he was awarded the Croix de Guerre for courage under fire. He also served as an interpreter between French and Italian troops, and accompanied Ambassador Johnson to Rome as his private secretary.

Dr. Fairbanks next went to Harvard Law School and distinguished humself as a law student. He was an editor of the Harvard Law Review, and later practiced law with a Cleveland firm. He decided, however, that law was not the field for him in the long run; instead, he went back to Harvard and got a Doctorate in Middle English; he was elected to membership in the Frisian Academy in recognition of the excellence of his doctoral research. He then entered on a highly successful teaching career, culminating at St. Johns College in Annapolis.

At the outbreak of the Korean war, Dr. Fairbanks entered the cryptologic service and has performed many distinguished services to his country in that capacity. We are indeed privileged to have Dr. Fairbanks with us today to tell us of his research on the Voynich manuscript.

II.B. Suggestions Toward a Decipherment of the "Key." Dr. Sydney Fairbanks.

The research I am presenting today has been directed at the last three lines of the manuscript, on Folio 116 verso. Fig 3.a. shows these lines as they appear on the original.

The first line, cmitting the final character, scans as a hexameter, which makes it sound impressive, but it is hardly informative. If the "-ton ola-" is cmitted, it reads approximately "michi ... dabas multas de carcere portas," or "Thou gavest me ... many gates from prison." There are, however, so many inaccuracies and oddly-formed symbols that it seems reasonable to suppose that we are dealing with a cover message, with the anomalies dictated by the necessities of the covered message.

Looking at the first two lines, "abi" in the lower line, followed by "cere" in the upper, followed in turn by "a" in the lower, suggest a sort of "desultory rail-fence cipher," taking varying numbers of letters first from one line then from the other, but of course moving steadily from left to right. Since such a process is capable of producing many permutations, of which more than one may read intelligibly, the one I am about to select can only be defended if it is measurably superior to others, and critics are urged to present, using the same system, as many rival decipherments as possible.

Following this scheme, I found myself forced to the conclusion that the alternation started with the final 8 of "michiton oladaba8." The message, however, if I am correct, starts with or in the course of these two groups, though the system of encipherment must be

different. One result of this scheme is to reduce the likelihood of noticing the rail fence.

We have then the arrangement shown in the next illustration (Fig 3.b.). Before making my rail-fence division, I shall make one or two adjustments, which must depend for justification on the results.

(1) The "mu" of "multy" " starts, with apparent carelessness, with a short stroke above the preceding cross. (These crosses, by the way, seem designed only to muslead; as for carelessness, I belueve that everything in these lines - even the smallest blot or stroke of the pen - is intentional and cannot be disregarded.) The result is that one can read equally well "imi" or "mu," and I shall choose the former.

(2) The s's written like 8's, and the obviously peculiar next-to-last symbol in "mult@" I shall assume to be symbols standing, in the covered message, for letters other than the "s" and "o" they superficially resemble and stand for in the cover message.

(3) The third letter in "movix" I shall assume to be a "v" although the peculiar way in which it is formed - apparently a distortion of the symbol rac, must be designed to give some other information that I have not fathered.

(4) The V that follows, occurring in "V $\langle x,$ " looks, compared with the others, like a capital letter, and supports the assumption that "Vitare" begins a second sentence.

(5) The symbol " < " in "v < x" and "ab < a" represents "ii."

(6) The "m" of "ma + ria" could equally well be "in," just as "mu" can be "imi."

(7) The first "e" in line 1 could equally well be "c."

(8) The final symbol "
(8) The final symbol "
(7) " on the first line is an over-lapping of of and "
(8) "a" and "n."

Now, for our rail fence, we obtain the arrangement shown in the fourth illustration (Fig 3.d.). Since "removet" requires both a subject and an object, and "similem," being an accusative, modifies the object, I have extended (legitimately) my rail fence to the "8" of "oladaba8." The sentence may then read: "8 similem a $t\chi\sigma$ " removet e (or c). Vitare ablicere a in a, portat χ r i a an." This may be translated: "C (or E) removes (i.e., distinguishes) the similarlywritten 8 from the "tu" 8. To avoid casting off 'a' from 'in,' 'i' is carried by 'an'." The argument for " χ " equal to "u" runs in three steps: (1) The first sentence says that unless "8" is removed it stands for "t χ "; (2) the "8" of "porta8," having no "c" or "e" to remove it, stands for "t χ "; (3) the only value for " χ " that fits into "portat-r" is "u."

The digraph "ix," as we have seen, has to stand for "e" if the message is to be readable. The writer of the key gives the meanings of several symbols, but always indirectly, using a strange character resembling the cipher symbol in a position where it has to be replaced by the meaning of the symbol. In the case of "e" however he used a digraph that does not resemble a cipher symbol. Why? And he selects, though any digraph would serve, the only one that has the value of 9. Why? To my mind, the only adequate explanation is that he wishes to tell us - indirectly - that 9 = ix = e.

The way "tar" is written resembles very strongly the way the four symbols H, η , f, f, f are inserted as infixes in the symbol " τ ," and I assume (as did the deviser of one of the alphabets for computer transcription I have seen) that "~ " stands for "t." " < ," I assume, represents " < " and stands for "ii." I shall later give tentatively some evidence that " 7 " as part of a different character stands for "1," which raises a strong probability that " \ " also stands for "i." The statement that "To avoid casting off 'a' from 'in,' 'i' is carried by 'an'," must mean in cipher terms either that to avoid casting off " <\ " from " M," " is carried by "an," giving us " A " = "a," " M = "n," "4 " = "4," or that "c" = "a" and to avoid casting off "c" from "1 1 or "1," " " is carried by " Δ ." This looks as if " \ , \\ , and \\\ " were respectively equal to "i, n, and m." But we are still adrift as to the meaning and effect of "casting off." Similarly the first sentence does not tell us what 8 means when it has been "removed" by c or e.

This brings me to the third line of the key, which begins with a series of cipher symbols $\alpha \langle o \rangle \langle \tau \langle c \rangle \rangle$ followed by the words "valson ubren so nim gas much o." Before I go further I want to draw a hard line between what I have said hitherto, presenting a method of decipherment, a reading of the first two lines, excluding the first two groups, and a series of derivable equivalents for λ ,

), $\langle , \chi , g \rangle$, $\langle \tau , \Lambda \rangle$, C; these constitute, so to speak, my "thesis," and are supposed to hang together. What follows is a list of observations, made by me in endeavoring unsuccessfully

to decipher "michitan aladaiaa." Some of them seem to me quite simple and probable, and others quite the reverse in both respects, but I am not asserting the consistency of any of them with the others. So, considering each one to be preceded with the word "perhaps," here they are.

1. Line 3 is concerned solely with "Michiton oladaba."

2. The cipher systels may represent letters in these two groups.

3.
3.
4. any represent the first two characters of line 1,
and stand for "a (not cast off) ni."

4. at may stand for "on."

5. A / 2 may, consistently with the thesis, stand for "it."

6. The two c's may stand for two "i's," two "o's," or two "a's."

7. They may, consistently with the thesis, stand for two "a's."

8. " 9 " may, consistently with the thesis, stand for "e."

9. The final letter of "oladaba" may be an "e" cut short to make it look like "a" in the cover message.

10. If "michiton" is written above "oladaba" the result could be read (by rail fence) "o (a not cast off) nichil dat on ba."

11. "Nichil dat" may be more likely than "michi dabas, or dabat, or dat," since from the standpoint of information both "michi" and the second person singular are otiose.

12. Assuming "nichil dat," our unsolved message may have to

be formed from the pieces "o, a, a, nichil, dat, on, ba, e," which does not afford much scope.

13. On the analogy of "multo8" read "imiltu8" the apparent word "valsch" may be read "valscn."

14. The facts that in "michiton" and "mich" the "h" has a loop, that the "n" has no loop and that a convenient blot obscures the junction between "a," "c," and "n" may tend to confirm this.

15. () may stand for "m."

16. The letters "mubren" can be transformed, by a regular process of moving each consonant clockwise into the place of the next, into the word "number," and this may be intentional.

17. The words "vals number" may mean "are in the wrong order."

18. If "o dat nuchil," the final "o" of lune 3 may be read "nuchil."

19. The preceding word "mich" may be inserted to encourage the cover reading "michi," while the "o" conceals "nichil."

20. g^{A} , written so that it is almost "gaf," may be a compromise between "dabas" of the cover message and "dat" of the covered message.

This ends my list of possible but not necessarily probable building blocks.

I should say a few words in defense of the practicality of the "desultory rail-fence system." Anagrams, as Friedman pointed out, are not suitable for communication, and it may be objected that the rail-fence cipher suffers from the same defect of producing far too many choices to be practical. Further reflection on the matter

will show, however, that the rail fence confronts us with a number of choices smaller by an order of magnitude: whereas an anagram of, e.g., seven letters provides 7!, or 5040 different choices, a rail-fence cipher of seven letters on two lines provides less than 2⁷ or 128; I say "less than 128" because once all of one line has been used there is no choice about the remainder of the other line. To give you a chance to convince yourselves of this, I have provided you with two enciphemments on one of the handouts (see Fig 4). The first is drawn from Bertrand Russell's History of Western Philosophy, and begins "He was somewhat ... " It contains a proper name, "Hanover," and is, I hope, mildly amusing. The second encipherment is a part of a long sentence chosen at random from ten pages of Bacon's Opus Majus. It begins with the letters "ae," and breaks off in the middle of a list of words. It is not amusing. My intention is to demonstrate that different people will independently get the same result from deciphering them.

I hope these remarks will be of some use to you. My reason, as you might surmise, for not keeping them to myself is that I hope someone will get the answer while I am still around to read it. It might even be one of us!

(Editorial Note: The above is a written version of his presentation which was kindly provided to me by Dr. Fairbanks for inclusion in these proceedings.)

mon chasting timbers + to tet covet powerd hang to listanariat menset vix + allert man et lot eeurg variet wirey le min gal mirit o 3.4. "Key" Sentences, Folio 116v (Photocopy)

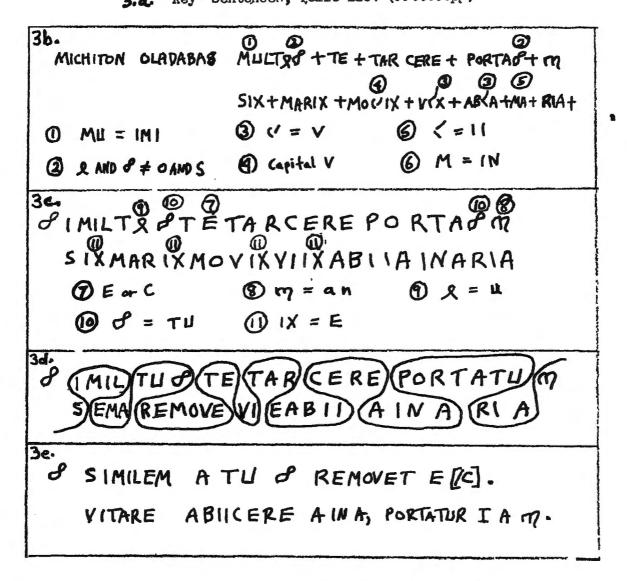


Fig. 3. Steps in Analyzing the Voynich Key (Dr. Fairbanks)

HEW ME NA MEW NEW NGLAY HE TO NORRIE ASSO HATA BOU TMOY HENA YOU DO FTCO THELE WEDI PRENTCO DHEDTO VER STIFU URFHA VERMA USEGI HEWHA CALDA DNGSENSI FULMA NDI NTHE DVINO GIVW NGNOW HACU RE NGOSE XIMSE GWITHA CETTO EUPASHI THA SBANDHI DENTRE COTHE RHE DESW ETEFU TSHEDSE DAHU STORY OSO RDWHE TBRI REGRAL A STMMSGITINUNTM LIAIDCTOIBUECMPOAEPAASS EIAUIURLGAMOSRATNSIINSESSOSITMTUCEEVA CCUALUMLNURUU₄MMNACUNIERMUSTELNUAIEIUM BLAIARIGAMCTA EQUECMMTUTIRINDIGISLNSTD USSYHSERCSAUSBOUSAASEOPTEAT.... OCPUCLIULICDIALSTHNAGARMER.....

-

Fig. 4. Encipherments (Dr. Fairbanks)

II.C. The Solution Claim of Dr. Robert S. Brumbaugh. M. E. D'Imperio.

I feel that, for the sake of completeness, this serinar should include a brief summary of another recent decipherment claim. Robert S. Brumbaugh, a professor of medieval philosophy at Yale University, became interested in the Voynich manuscript during the thirties. When it was donated to Yale in 1969, he began to attack it in earnest. He was also struck by botanist Hugh O'Neill's identification of American plants in the drawings. Brumbaugh published an article in the journal Speculum in 1974, announcing that he had solved the mystery. In 1975, he published a second article in the Yale University Library Gazette giving some further details. He claims to have read some labels on plant pictures and some other words on various pages of the manuscript. He also states that he has deciphered the name of Roger Bacon in the "key" sentences on the last page. He regards the manuscript as a deliberate forgery for the purpose of fooling Emperor Rudolph the Second of Bohemia into parting with the large sum of money he paid for it.

Brumbaugh makes considerable use of the "key"-like sentences others have noted on folios 1 recto, 17 recto, 49 verso, 66 recto, 57 verso and 116 verso. He says that these sequences were primarily intended to mislead the would-be decipherer, but they still provided aid to him, Brumbaugh, in penetrating the cipher, because the forger outsmarted himself and gave too much away. His explanations of these clues are, unfortunately, very incomplete. They are convincing at first glance, but when I tried to look more closely at them and

retrace the steps Brumbaugh claimed to have followed, they fell apart. To make matters worse, Brumbaugh offers no documentation or scholarly evidence of his sources other than a few off-hand, very vague words in passing.

For example, consider the sentences on folio 116 verso, which Dr. Fairbanks has studied so carefully from an entirely different point of view. Brumbaugh finds some phrases there to be enciphered in what he calls a "standard thirteenth-century cipher." He offers no evidence in the literature of just which cipher he means. He claims to find confirmation for his idea about this standard cipher in the paired alphabetical sequences which are very faintly and fragmentarily visible in the right and left margins of folio 1 recto. These are not visible at all in our photocopy, but may be seen in Father Petersen's remarkable hand transcript, a photocopy of which is here for anyone to examine during our break periods. Brumbaugh claims to find in these sequences a monoalphabetic substitution of two normal alphabets, with "a" of one set against "d" of the other. I can see no evidence that the alphabets are offset at all in Petersen's transcript, which was carefully matched and corrected by him against the original.

But using this cipher and some rearrangement of other syllables, Brumbaugh obtains the name RODG BACON from the phrases he singles out on folio 116 verso. This is the planted reference to Bacon that he claims was intended to attract Rudolph's cipher experts into advising the Emperor to buy the manuscript.

On folio 66 recto, Brumbaugh sees a set of "formulae" in the words and letters scattered down the right margin. These formulae, he claims, serve to equate symbols to other symbols in the Voynich script by a sort of "cryptarithmetic." He gives some examples of this in his 1975 paper. The only evidence he gives for his idea is the following rather airy sentence: "Since I had seen a number of these characters in another cipher in Milan, where they represented numerals, I suspected an arithmetical game." He provides no further support or explanation of his sources. Unfortunately, as I soon discovered while researching my monograph on the Voynich manuscript, there are literally hundreds, perhaps thousands, of early Italian ciphers which use numeral forms as cipher characters, many of them very similar to some Voynich script characters. None of these ciphers, however, seem to include any such cryptarithmetic as Brumbaugh claims to see on folio 66 recto. Without a better reference, we cannot track down the source upon which he bases his idea. While I will admit that the small number of formulae he explains in full are plausible enough as they stand, I have been unable to extend the same principles to all the other examples on that page which he does not explain, and in fact some actually seem to contradict the method he suggests.

Using these "equations" and some recoveries of labels for plants, Brumbaugh set up a nine-by-four matrix. The plant labels, all on folio 100 recto, he got by cribbing and by using word patterns with repeated letters like the p and e in "pepper," and guesses at what plants the pictures showed. Again, Brumbaugh claims that the

nine-by-four matrix is similar to "a standard alchemist's or astrologer's cipher, well known in the trade," and as usual, he provides no further reference or explanation to back up this claim.

All the Voynich symbols, according to Brumbaugh's theory, stand for forms of the numerals one through nine. The encipherment is a two-step operation. First, letters of plaintext are replaced by numerals using the nine-by-four box, collapsing the letters of the alphabet onto the nine numerals. This slide (Fig 5.) shows the matrix as Brumbaugh published it in his paper. For instance, the letters B, K, and R were all replaced by the number 2. Then, as a second step, a choice was made among several different fanciful designs for each numeral to conceal them, producing the Voynich cipher text as we see it. According to Brumbaugh, the symbols were chosen from "modern and archaic numeral forms, Greek and Latin letters, and several cursive compendia." Again, he gives no evidence or detailed explanation of the origin of any particular symbol, so we have nothing to go on.

The next slide shows a matrix with some of the Voynich symbol variants for numeral forms (Fig 6.). This is my own tentative reconstruction of the cipher correspondence from Brumbaugh's articles, since he does not explicitly provide them anywhere. We see here, for example, that there are four Voynich symbols all standing for the numeral 7. There are some uncertainties, for reasons to be discussed in a moment.

Decipherment involves three steps; first, recognizing the numbers underlying the multiple variants in the Voynich script. Second,

writing, under each numeral the two, three, or four possible choices for plaintext equivalents. Third, selecting a pronounceable and plausible Latin-like word out of the resulting rows of letters. The plaintext produced is described by Brumbaugh himself as follows: "An artificial language, based on Latin, but not very firmly based there; its spelling is phonetically impressionistic; some sample passages seem solely repetitive padding." Also, the "upper cipher key" (whatever that may be) changes slightly every eight pages.

This slide (Fig 7.) shows two of Brumbaugh's sample decipherments to illustrate his method, and some of the problems I encountered in reconstructing it. The top example is from folio 116 verso. He reads this as ARABYCCUS, supposedly referring to the Arabic numerals of the cipher. Even granting his identification of the Voynich characters and his matrix, it could as easily be read ARAKYLLUS, ABARYLLUS, UBARYCCI, or any number of other more or less pronounceable, Latin-like things.

The bottom one is from folio 100 recto. This is a page containing rows of small plant pictures each labelled with a Voynich script sequence. Brumbaugh reads this as a garbled word for PAPAVERUS or "poppy." But then he seems to have gotten in a hurry or mixed up in his interpretation of the Voynich characters; he apparently sees the fourth letter as an O-like symbol, corresponding to the numeral 1 and plaintext A, whereas I see it clearly as an A-like symbol. I cannot account at all for his interpretation of the fifth letter as a plaintext V. In almost all of his other sample decipherments, there is at least one such letter that is puzzling, or can be inter-

preted differently from his choice. There is a "messiness" about the whole affair that is not satisfying. Attempts to extend the recoveries to labels on other pages result in many meaningless sequences that bear no relation to Latin or anything else, with one or two slightly more promising instances now and then, to keep us "hooked" and keep us trying.

A new paper by Brumbaugh is scheduled to appear in the 1976 issue of the <u>Journal of the Warburg and Courtauld Institutes</u> of the University of London, probably to be published in early 1977. We can hope that he will provide a more carefully worked-out and documented exposition of his theory there so that we may subject it to independent verification.

	INTE	RMEI	TATE	CIPH	ER				
	1	2	3	4	5	6	7	8	9
P	A	В	C	D	E	F	G	н	1
L A	J	к	L	M	N	0	P	Q	
N		R		S		T		Ц	ЦS
	V		W	(X)	X		Y		Z

Fig. 5. Brunbaugh's Matrix

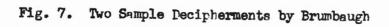
しーやモモル	<	R 2 2	いたまれる	x 4	۵ 5	D 6	2222	8	9 9
P	A	B	С	D	E	F	G	н	1
LAIN	Ъ	K R	L	M S	∧ ×	0 T	P	С D	US Z
			W	(X)	^		ι		2

Fig. 6. Reconstructed Cirber Correspondences

43 *

Folio loor

C	0	2	1 Q	?	0	~	o	8
7	I	7	5	2	1	7	1	4
6	A	6	E	B	A	G	A	D
P	J	P	N	ĸ	σ	Ρ	T	M
Ŷ	V	Y	X	R	۷	Y	۷	5
"PAPAVAYJS" ER								



44 '

II.D. Further Details of New Statistical Findings. Capt. Prescott Currier.

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: 0, 0, 9, 2 all seem to start with a "c"-curve, which was made first, in this direction:

(c, so we have: o = 42, d = 41, g = 41, 2 = 4. These forms all have counterparts starting with L : f = (f, f = 1), l = 1, etc. we also have q = 41. 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., cdg, ccdg. On the other hand, the letters g and l (which have very high frequencies) can <u>never</u> be preceded by c, <u>ever</u>; they are instead preceded by G.

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

		110	IIID
X	(12)		(11.2)
	11	(nr)	(1117)
H	17	(ung)	(1115)

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 of 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 ℓ and

c ; it doesn't mean anything, but it's interesting.

2. <u>Origin of the Symbols</u>. This symbol 9 is a common Latin abbreviation standing for CON, CUM or -US, so that it can come at both the beginnings and ends of words. For example, "continuus" might be written " 9 ± 100 ." Now 9 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 \P ,

series. It looks as if whoever designed the alphabet used 9 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 σ , it is a frequent letter in Etruscan, in Lydian, and in the Lemmos 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 \mathcal{H} , \mathcal{H} , \mathcal{H} , \mathcal{H} . The symbol \mathcal{H} 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 (\mathcal{H} , \mathcal{H}) 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 \mathfrak{H} , \mathfrak{H} , 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 \mathcal{F} , \mathcal{F} are not the same as those two \mathcal{N} , \mathcal{N} as the statistics show. The letters \mathcal{H} , \mathcal{H} are followed anywhere in a "word" by our little friend c about half the time (say 750 out of a total of 1500), including initially. These two, 7 , F are never, ever, anywhere in the manuscript, followed by C . These latter symbols are much less frequent than the first two, but their occurrence followed by C is zero. I don't have to calculate signages on that! Therefore, a , are not aberrant or variant forms of \mathfrak{R} , \mathfrak{R} , 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 \mathcal{H} and \mathcal{P} 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, $\subset \mathcal{I}$?

<u>Currier</u>: That may be a way of abbreviating two of those looped letters. It doesn't happen frequently enough to bother me.

Frog. (Example: 109

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 c_7 and c_7 . (This letter c_7 , by the way, is in fact made like this: c_7 .) 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"
ر ۳	118	20	3
reo91	212	38	26
ረት የ	24	4,5	0
~~091°	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 \circ . This applies only to "Language" A, by the way; words with this initial group are low in "Language" B ($\pi \circ \Re$, for example, occurs only 5 times in Herbal B, but 212 times in Herbal A).

4. The Nature of the Symbols \mathcal{H} , \mathcal{H} , \mathcal{H} , \mathcal{H} , \mathcal{H} . My next point concerns the so-called "ligatures" based, apparently, on the series \mathcal{H} , \mathcal{H} , \mathcal{H} , \mathcal{H} , \mathcal{H} . They are made like this, by the way: (\mathcal{T} , with \mathcal{H} , 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

symbol	all "word" initials	first "word" of line
Æ	326	3
Æ	67	1
र्तीत	82	0
at t	14	0

of identifying the two "languages." In Herbal A, the word-initial

r

occurrences are as follows:

Therefore, \mathcal{H} , \mathcal{H} , and the like are symbols in their own right, and are <u>not</u> equal to $\mathcal{H}_{\mathcal{H}}$ or $\mathcal{C}_{\mathcal{H}}\mathcal{H}$, 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:					
	40	842	trate			
> series	13	7	91			
? series	10	2	68			
) 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 40. 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 <u>consistent</u>, <u>statistically significant</u>, 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.)

II.E. Questions and Discussion.

Question (Speaker not identified): How do you account for the fullword repeats?

Currier: That's just the point - they're not words!

<u>Child</u>: 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.

<u>D'Imperio</u>: 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.

<u>Valaki</u>: 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.)

<u>Currier</u>: 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 the tape -Ed.) I have a whole notebook of statistical charts at home: things I wanted 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?

<u>Ourrier</u>: Not specifically. I've got it all at home...but it hasn't suggested anything to me.

<u>D'Imperio</u>: 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.

<u>Currier</u>: But there <u>are</u> a lot that are exactly two long. (Examples from "Herbal A" and "Herbal B," not auduble on tape -Ed.) Certain groups - a different one in A than in B material - are repeated four times in a row; they would <u>have</u> to be numbers, I can't think of anything else. If the one were "zero" in "Herbal A," the other might be the "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...

<u>Currier</u>: An annoying little circumstance: words beginning with " (**7** ' 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 " (**7** " 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?

Ourrier: Yes, it is basically true, but especially in "Biological B."

<u>D'Imperio</u>: 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."

<u>Currier</u>: 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.

<u>D'Imperio</u>: I have a copy of Captain Currier's alphabet and sorting sequence.

<u>Currier</u>: 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.

<u>Miller</u>: I'd like to bring up something relating 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 <u>purposeful but inarticulate</u> writing; doodles are simply to pass the time away...

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

<u>Miller</u>: 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?

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

<u>Miller</u>: 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.

<u>D'Imperio</u>: 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...?"

<u>Miller</u>: But with all these statistics that Captain Currier, Brigadier Tiltman, and Mr. Friedman have given - hasn't anyone...

<u>D'Imperio</u>: The trouble is, how <u>can you prove</u> 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 <u>does not</u> 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 <u>does</u> exhibit those criteria. This would constitute a sort of "uncomputability" or "undecidability" theory for the <u>semantics</u> of textual strings. Is this possible? At our present stage 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 <u>fabricated</u>, independently of any semantic or linguistic structure having a recoverable meaning; these tests and hypotheses ought certainly to be applied to the Voynich text.)

<u>Valaki</u>: 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!

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

<u>Valaki</u>: Maybe that's like the Voynich - it could turn out to be a good straight copying job.

<u>D'Imperio</u>: 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.

<u>D'Imperio</u>: 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.

<u>Currier</u>: Evidence that it can't be "doodles" is the minimum of six people involved in the 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 table <u>so</u>. 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 <u>his</u> thing with another in <u>his</u> "language." And they went through the second stack and interleaved them; one man did it one way and the other man 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 ...

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

<u>Currier</u>: 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 26 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).

<u>Buck</u>: 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!

<u>D'Imperio</u>: One of the missing folios was for the zodiac signs of Capricorn and Aquarus; maybe that was somebody's horoscope?

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

<u>Currier</u>: 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 <u>feel</u> that they are. In the Astrological Section, the signs of the zodiac are in the right order.

<u>D'Imperio</u>: There is some evidence in the folio gatherings - 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!

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APPENDIX A

The VOYNICH MANUSCRIPT Some notes and observations

Capt. P. H. Currier

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 <u>Astrological Section</u> (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 <u>Biological Section</u> (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 <u>Pharmaceutical Section</u> (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 ' OS_9 ,' 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 Pharma-

ceutical '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 <u>Recipe Section</u> (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'

It 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 principle differences between the two 'languages' in this Section are:

(a) Final ' **89**' is very high in Language 'B'; almost non-existent in Language 'A.'

(b) The symbol groups ' **(70)** ' and '**(70**' are very high in 'A' and often occur repeated; low in 'B.'

(c) The symbol groups '**ctaid** ' and '**ctaid** ' rarely occur in 'B'; medium frequency in 'A.'

(d) Initial ' all ' 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 <u>much</u> 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 principle reason for transcribing and indexing this material. It was hoped that by the 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 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 ' $4 \circ$ ' was restricted pretty largely to ') '; and that initial ' $(c/c^2 + was preceded much$ more frequently than expected by finals of the ' <math>m '-series and the ' χ '-series. Additionally, 'words' with initial ' cc/c^2 ' 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 a functional entity

As mentioned in para. 3. above, 'words' with initial ' α / α' ' 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 ' $8c\tau$,' ' $9c\tau$,' etc., which have the appearance of ' $c\tau$ '-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 medial ' \mathbf{H} ' and ' \mathbf{H} ' for all sections showing both total and line-initial frequencies.
Table 2.	Frequency of finals following ' $c c$ / $c c$ ' for all sections of the Manuscript.
Table 3.	Frequency of finals following medial ' 11' ' and ' # for Herb A, Herb B and Biol. B.

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Table 4.	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.'

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TABLE I

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OF FOLLOWING WORD' IN BIOL B Selected high trequency words with R, P, 112.9 tracis showing that (a) words triding in R, P and in are failowed most frequently by 'words' with initial cz/cz and only rarely or not at all by words' with initial x on P (b) worde' ending in g ore Followed Most frequently by "weeds" with initial 40; and includes most words with initial x or P.

TABLE 6

APPENDIX B

What Constitutes Proof?

Stuart H. Buck

November 1976

I don't have any answers to offer - only a few questions and some observations. It seems to me that the main problem confronting anyone wishing to evaluate claims of a solution of the Voynich Manuscript is how to test the bits and scraps of decrypted text offered as proof. If a crib seems to work in one or two places, how can anyone determine that the <u>same</u> Voynich symbols always mean the <u>same thing</u> throughout the entire manuscript? There exists no standard index of the whole corpus showing every occurrence of each "character" with preceding and following context. If someone were to undertake to make such an index, how are the Voynich characters to be represented in Roman letters or other symbols that can be printed out by the computer? Is anyone certain how many basic or distinctive elements are contained in the script? How do these elements combine with each other? How should their ligatures be represented?

Furthermore, if someone offers a partial decryption in a language as it was presumed to be used in some period before the sixteenth century, what means do we have of testing the validity of a decryption in any of the languages of that period? For example, who has access to a plain language study of medieval Latin? What statistical knowledge do we have of other languages that might have been used? How can one determine the relative frequency of vocabulary, common stereotypes, clichés, etc.? Who today is steeped in the

highly specialized vocabulary of alchemy, magic, astrology, cosmology, herbals, and other topics suggested by the drawings in the Voynich Manuscript? Or are these to be ignored? If so, why?

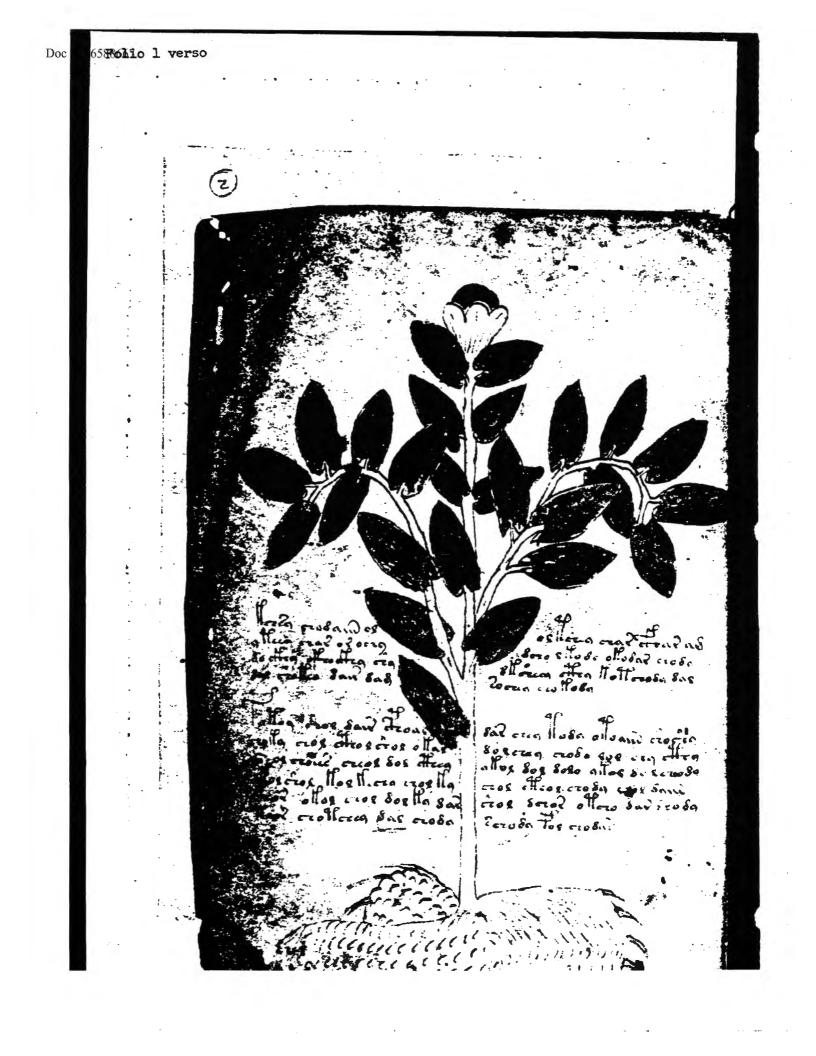
Perhaps the most serious problem confronting the student of the manuscript is lack of knowledge of its age and country of origin. The fact is, it cannot be traced beyond the court of Rudolph II of Bohemia - and how it got there is uncertain. And yet the identity of the author of the manuscript is all-important. One would not expect a man to devise such an elaborate scheme to hide a text in a language that he didn't know. It seems reasonable, then, to assume that the underlying language of the manuscript would be the one used by educated men in the country where the author resided. This does not have to be the case, but it is highly likely; if, indeed, a natural language is involved at all. Any hypothesis, then, that ignores any real knowledge of the age and place of origin of the manuscript is taking grave risks, and would require massive internal evidence in order to be acceptable.

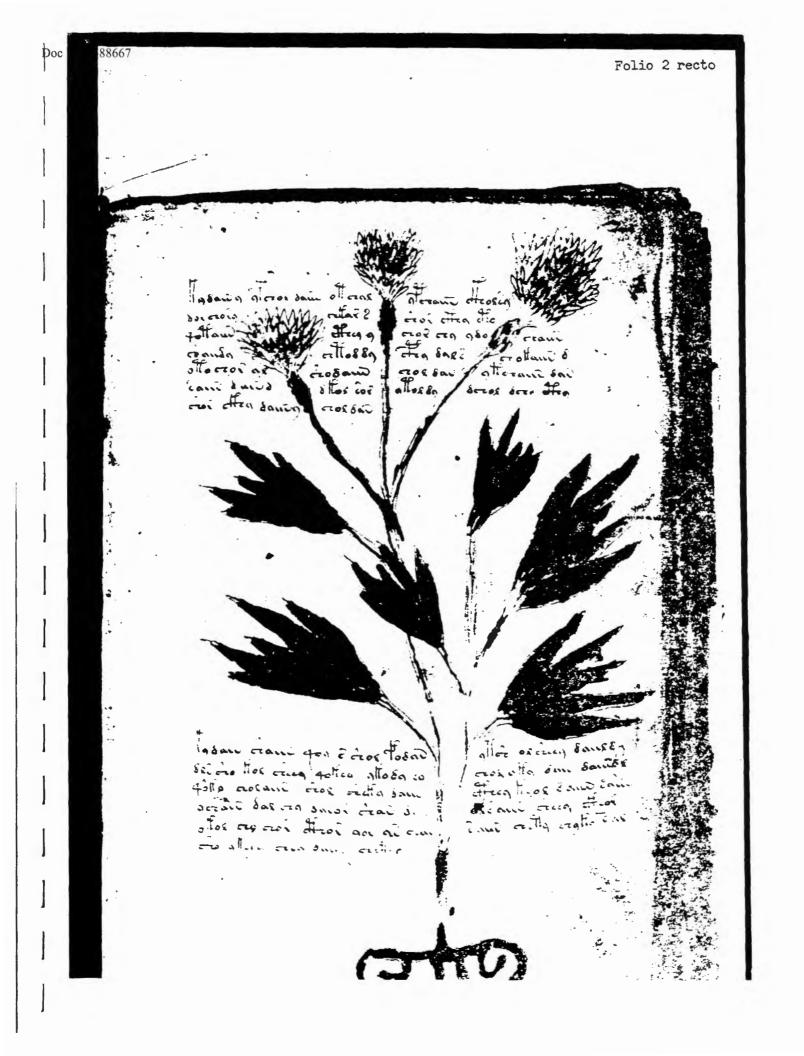
One last word: if you think that the Voynich Manuscript is nothing but an elaborate hoax, then that also is a hypothesis to be demonstrated or disproved. You can't just wave the whole thing aside because you don't understand it. The Voynich Manuscript does not deserve our attention merely because it is an intriguing enigma demanding an answer only because it is there. What makes it worth talking about is that it involves questions of methodology, tools, and validation that concern all analysts faced with the problem of deciphering secret writing, past and present.

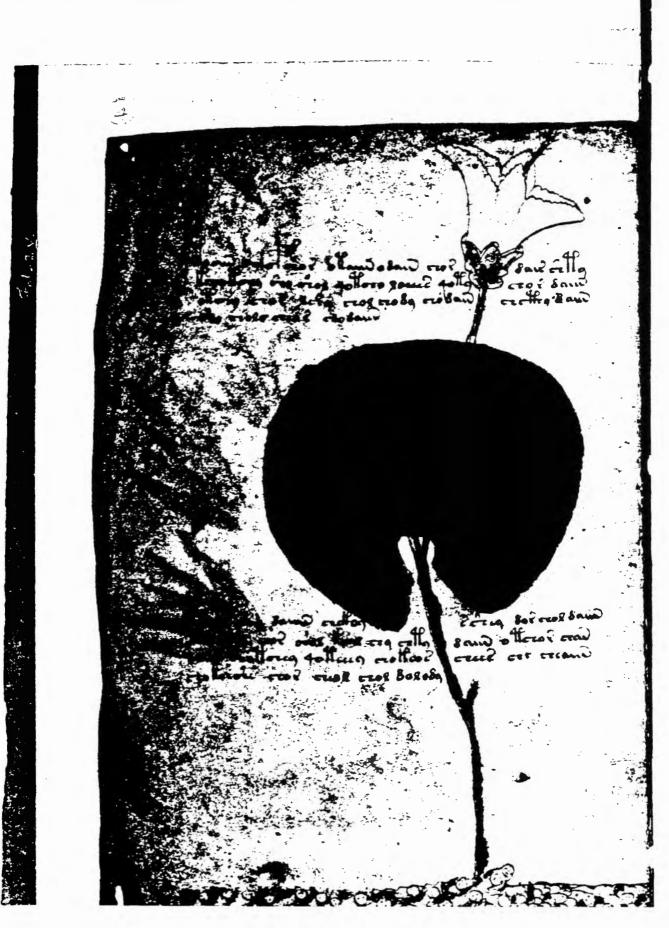
THE VOYNICH MANUSCRIPT (Photocopies)

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Approved for Release by NSA on 08-04-2017, FOIA Case # 101982







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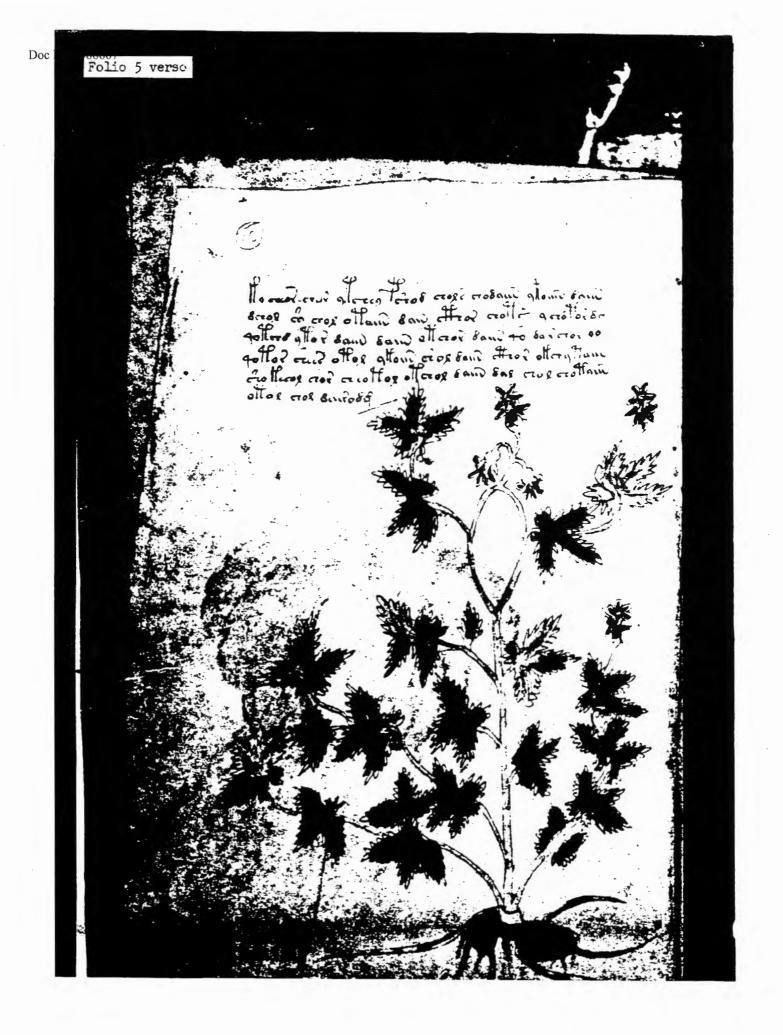
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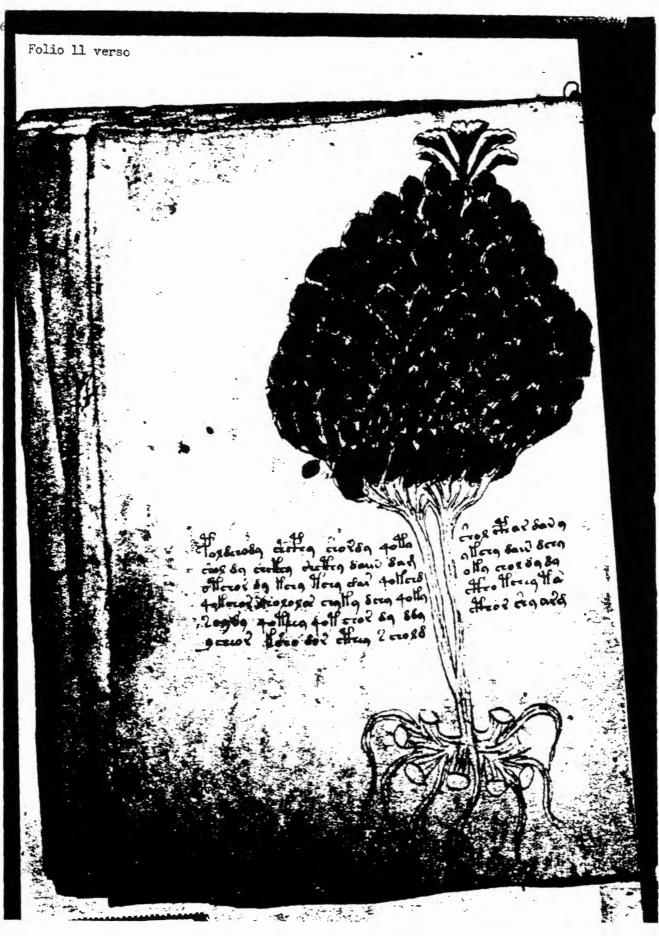


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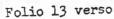
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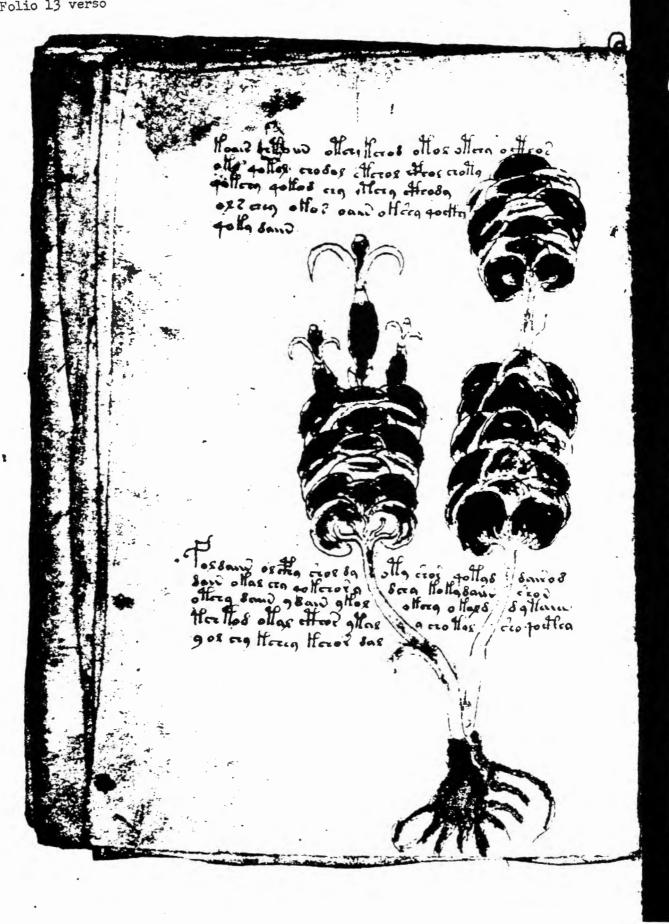
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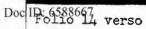
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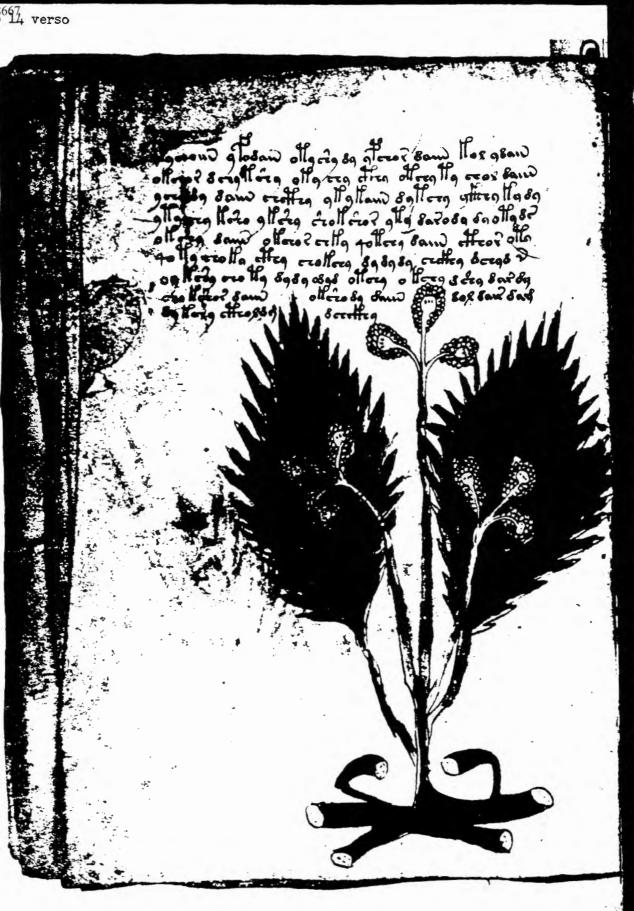
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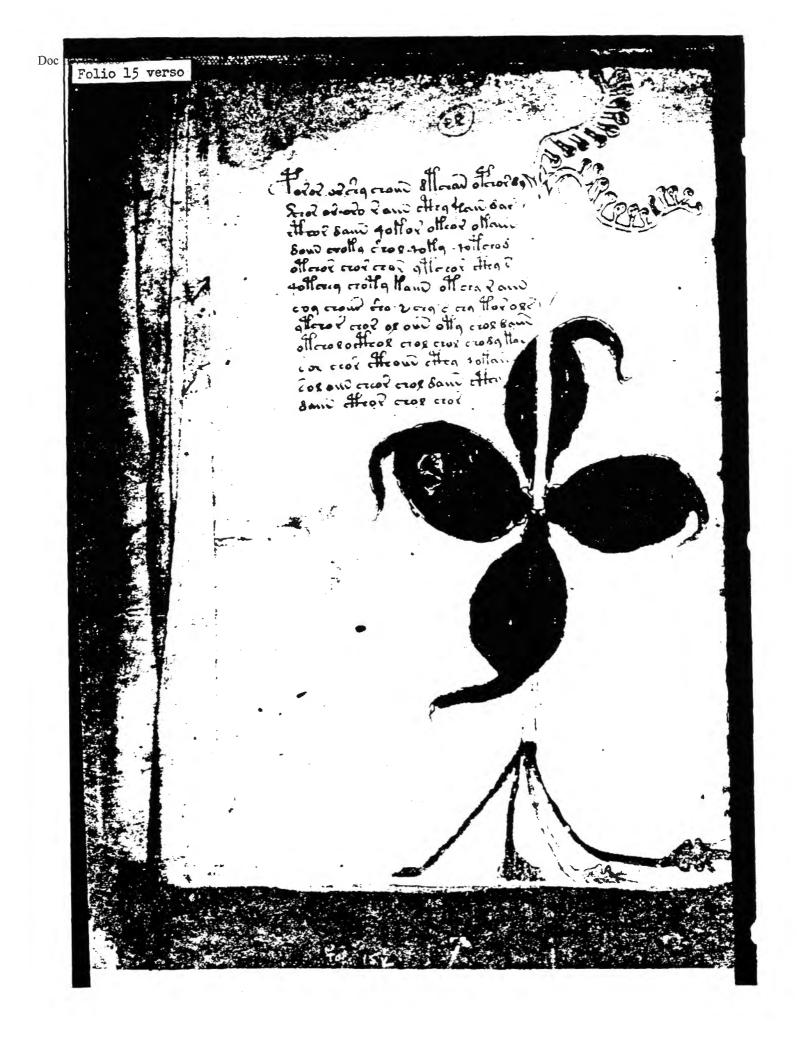
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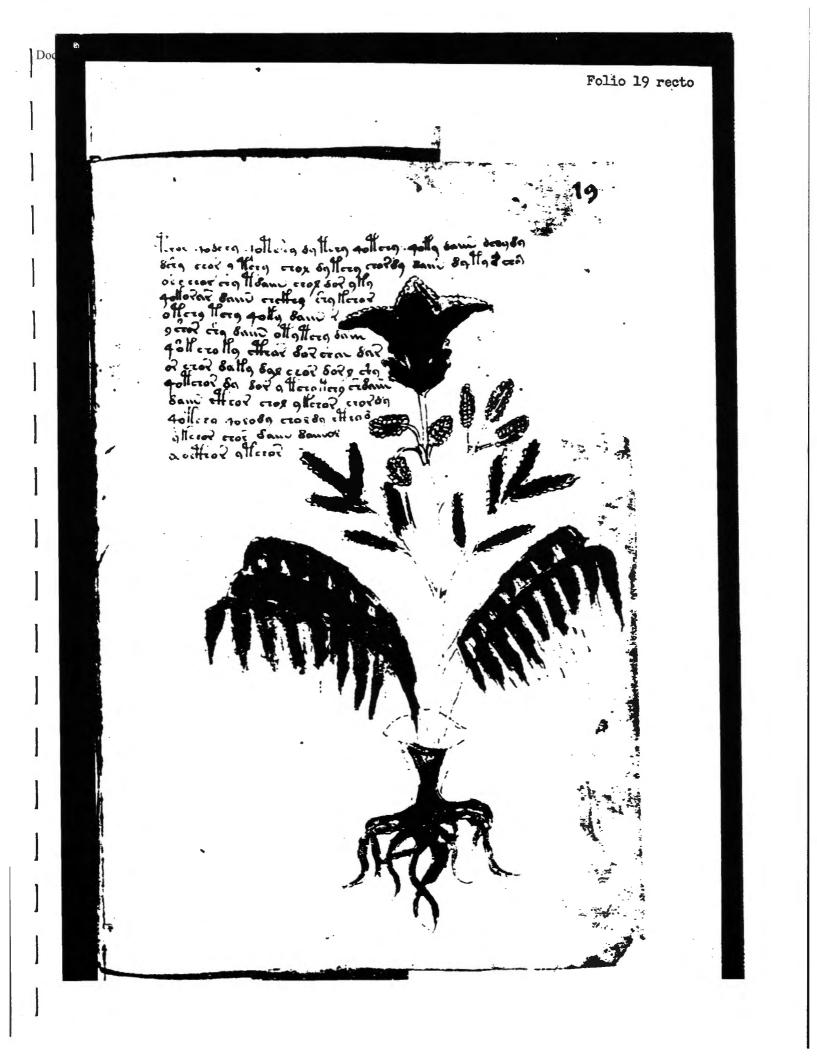
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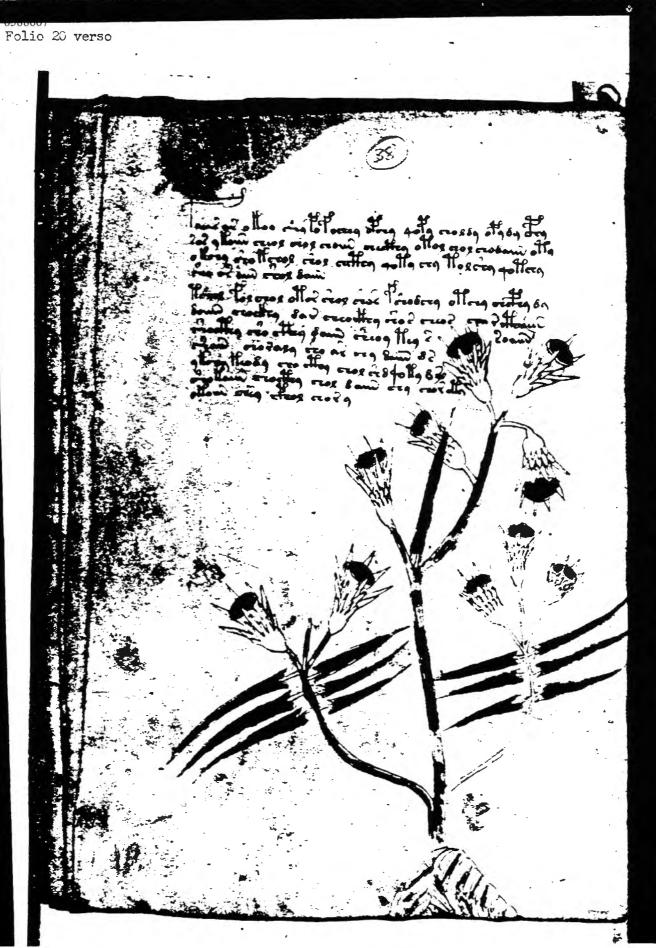


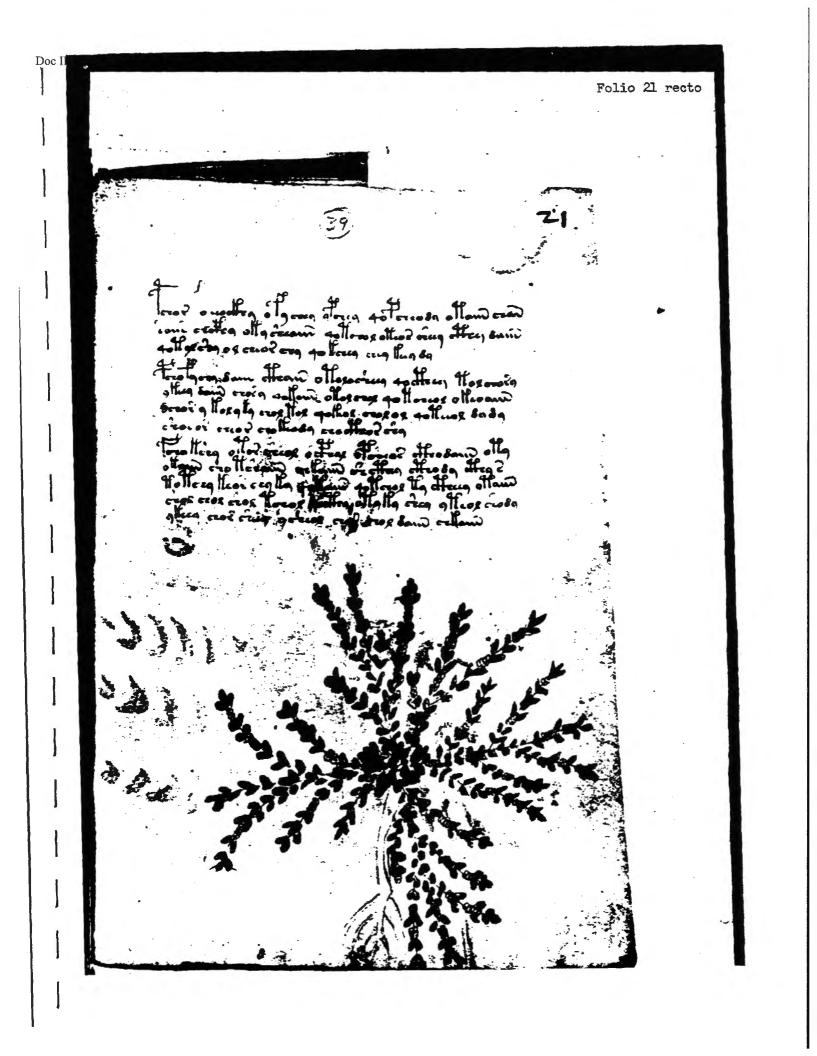
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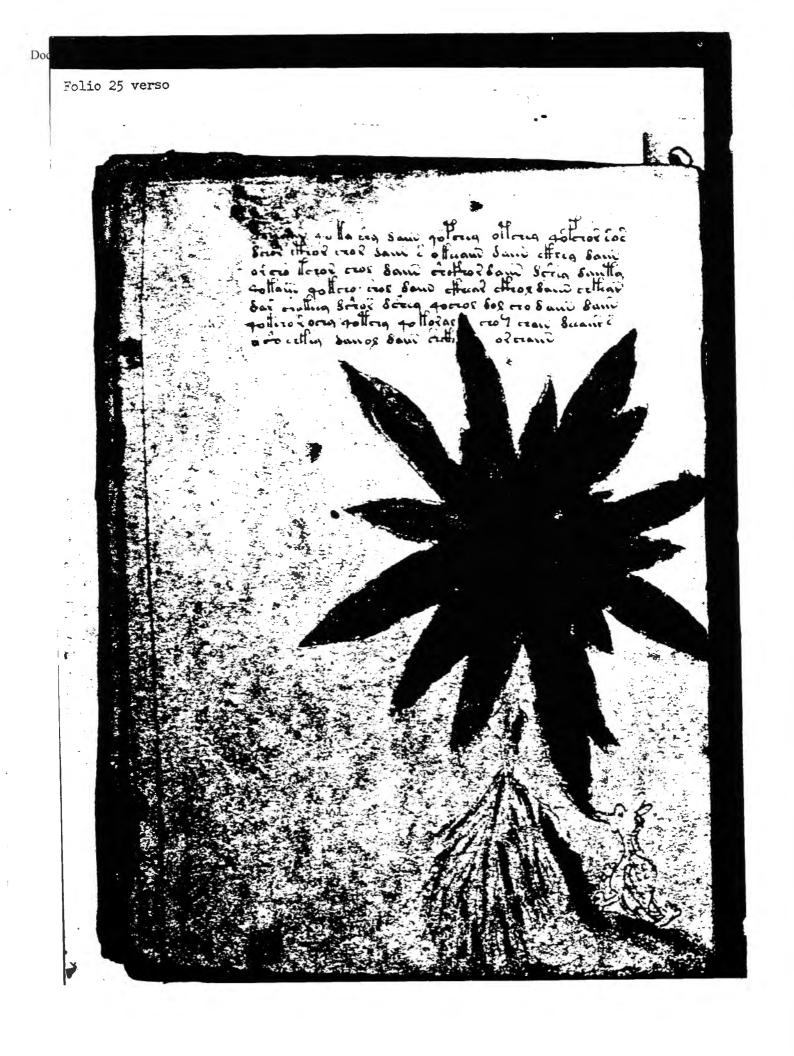


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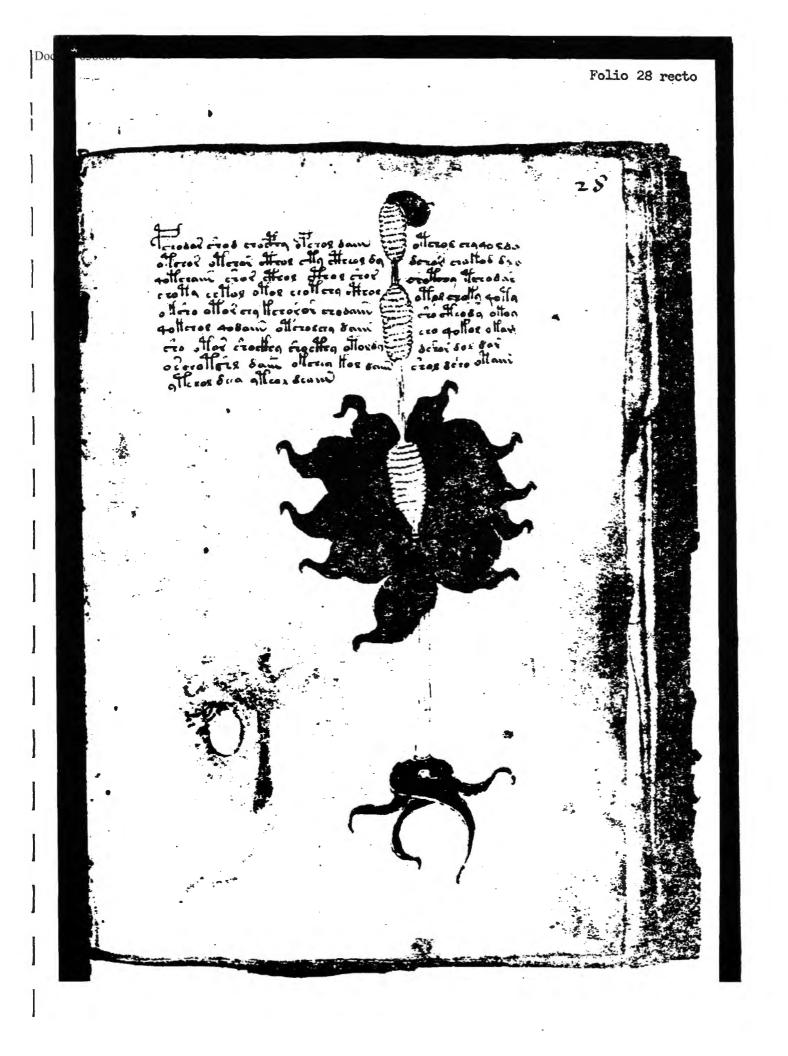


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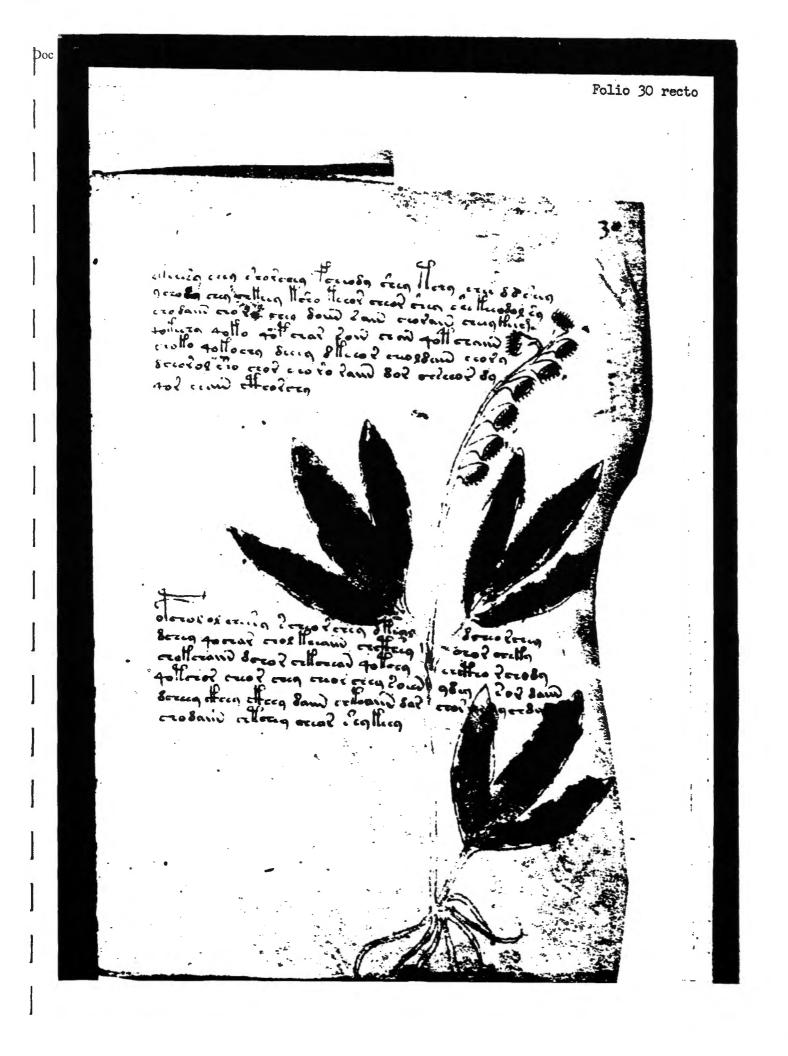
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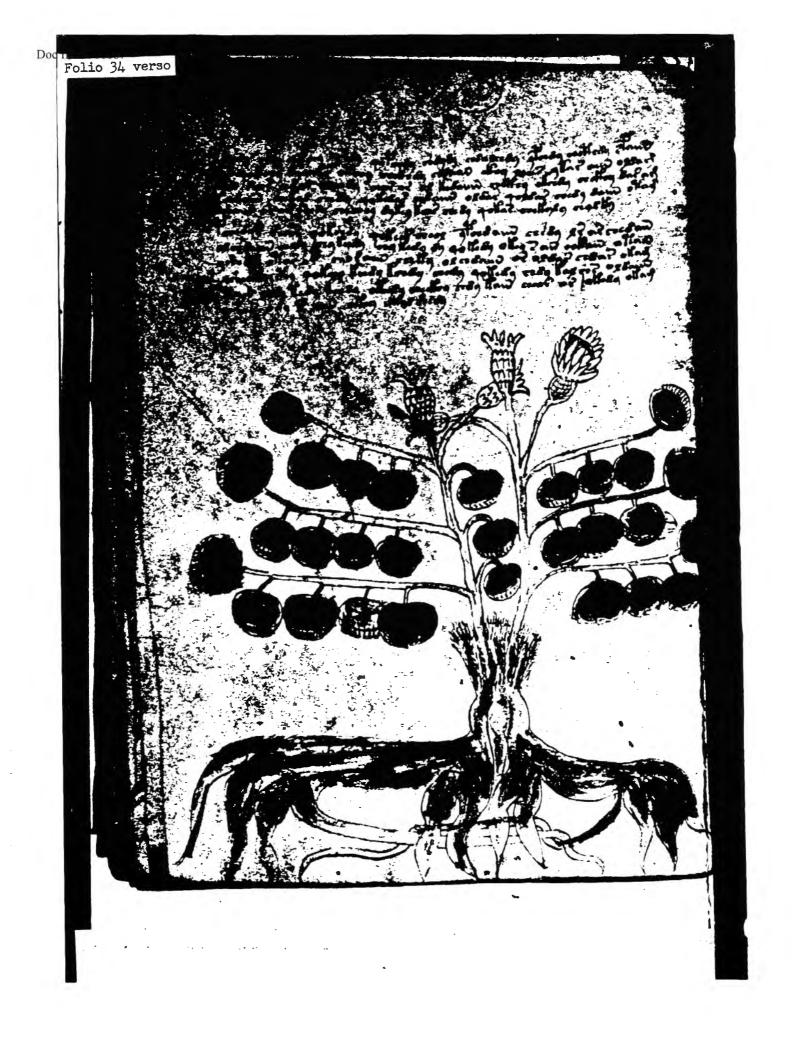
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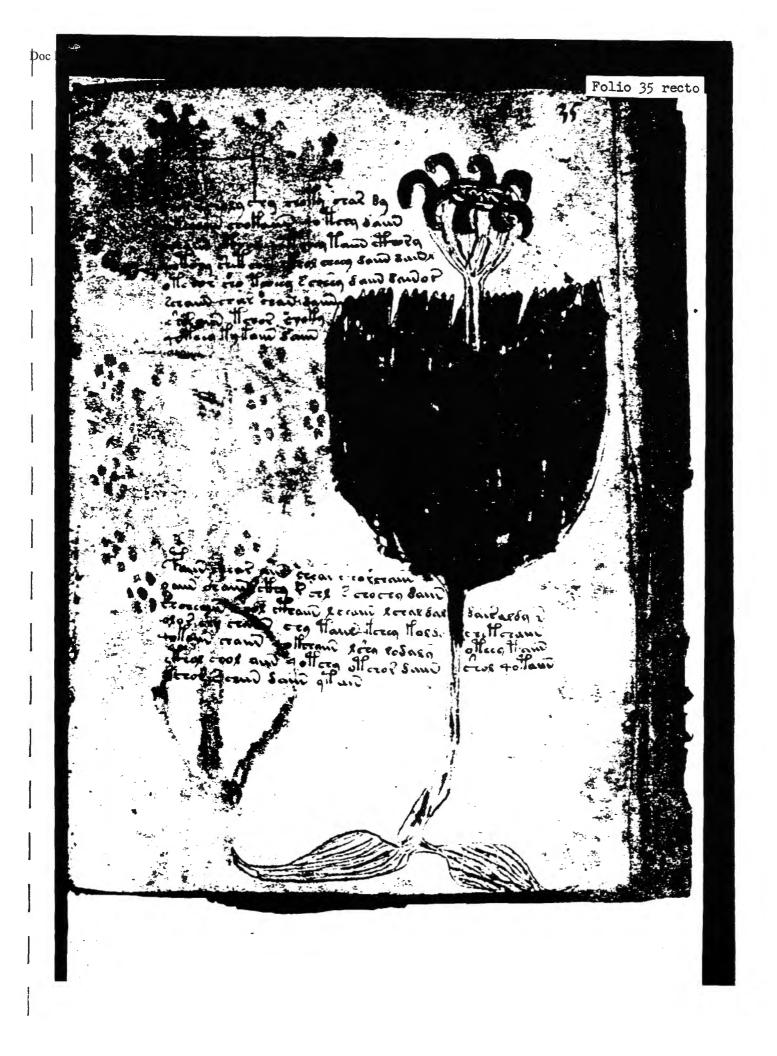
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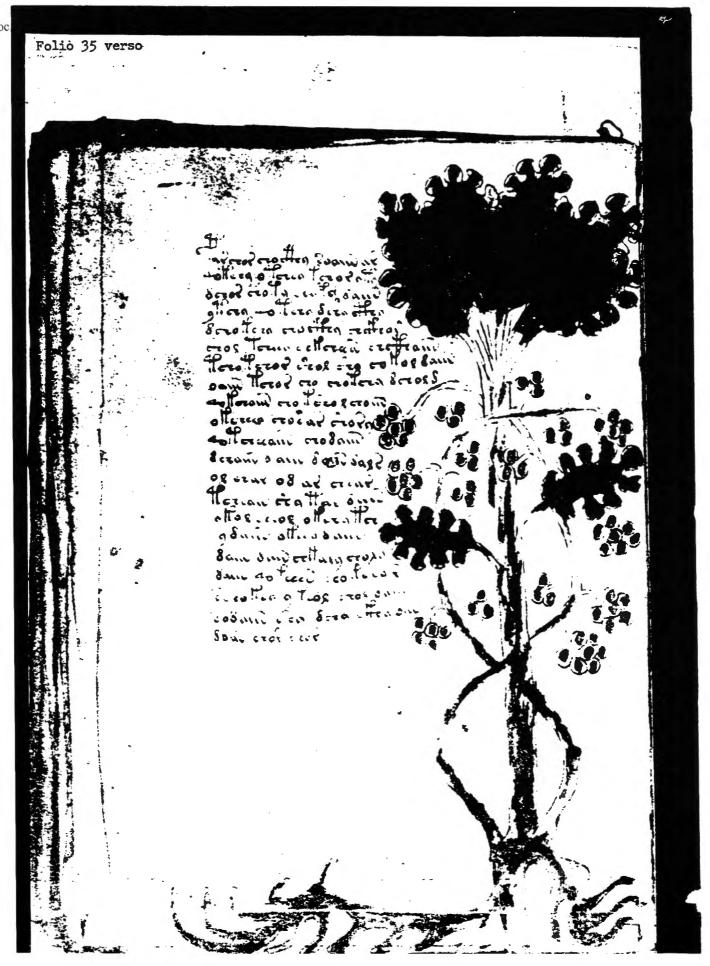
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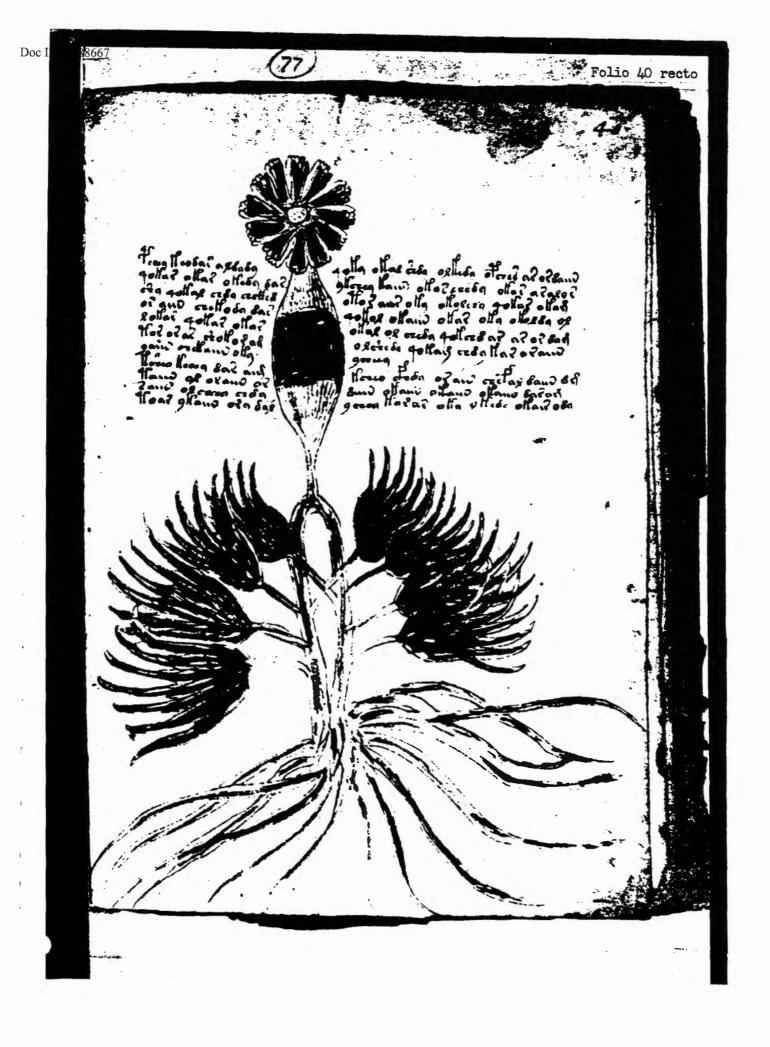


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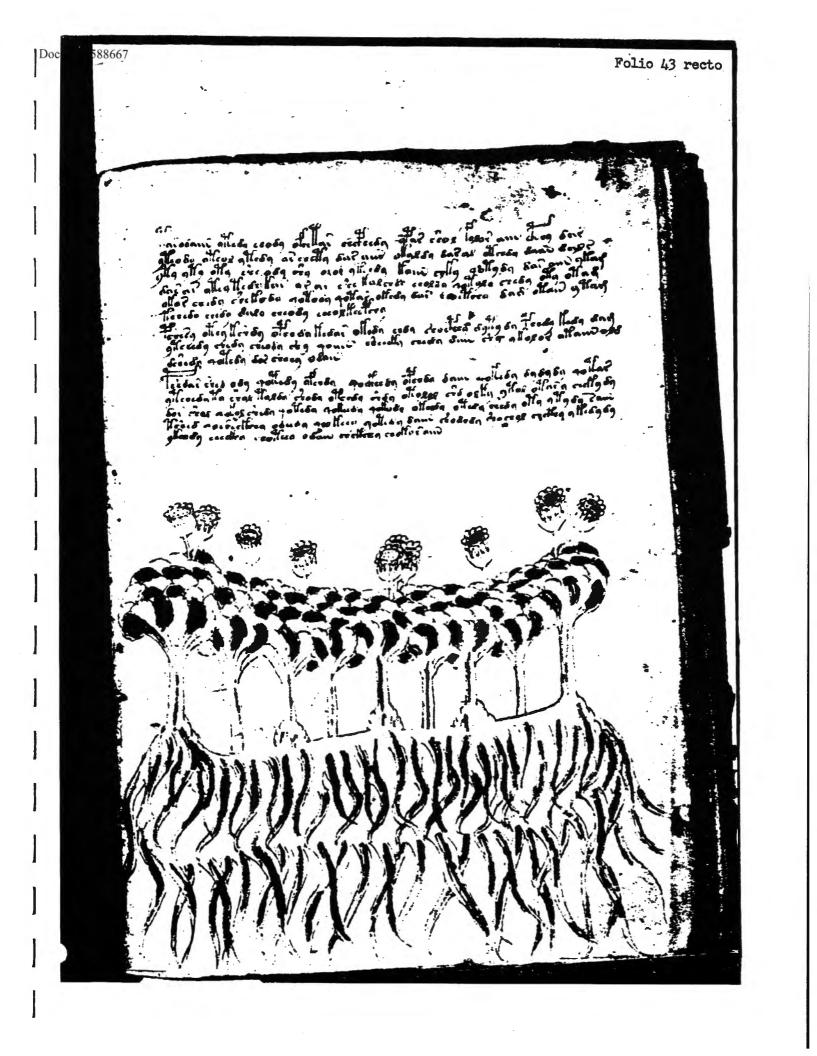
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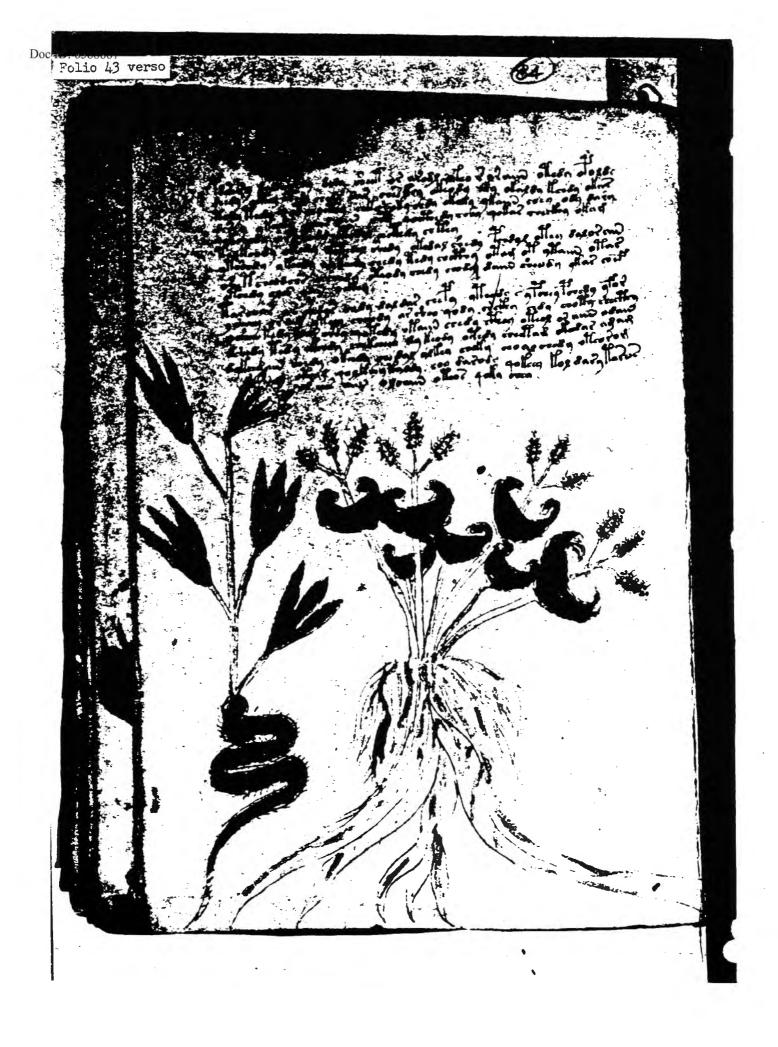




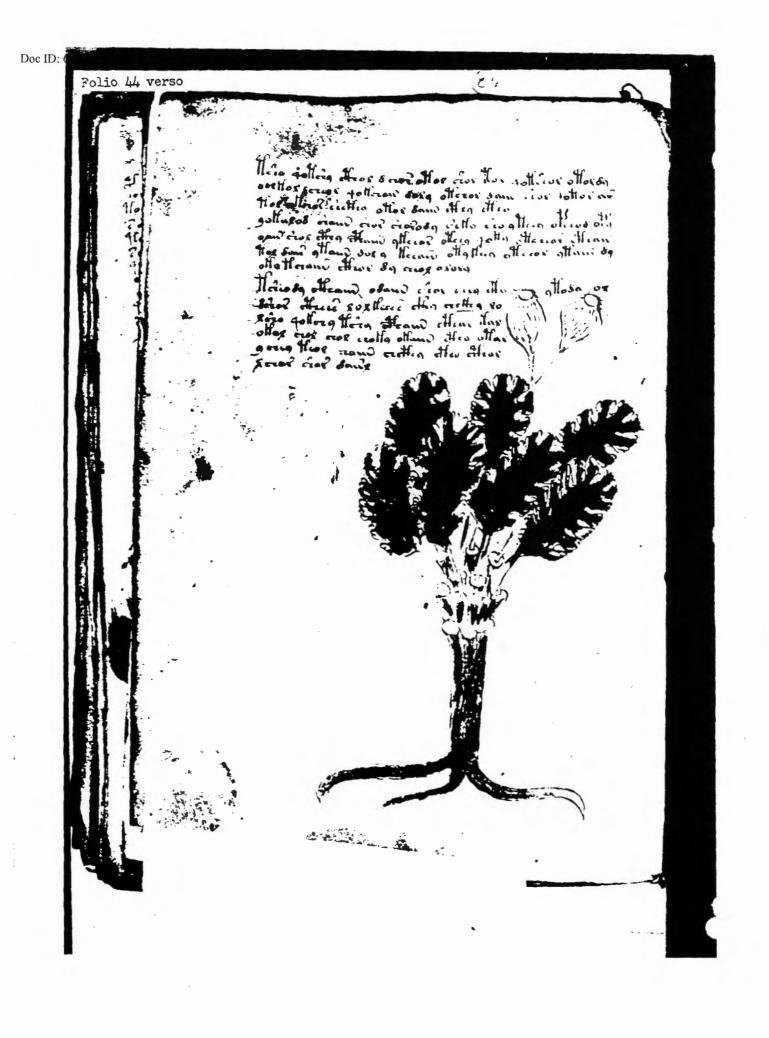
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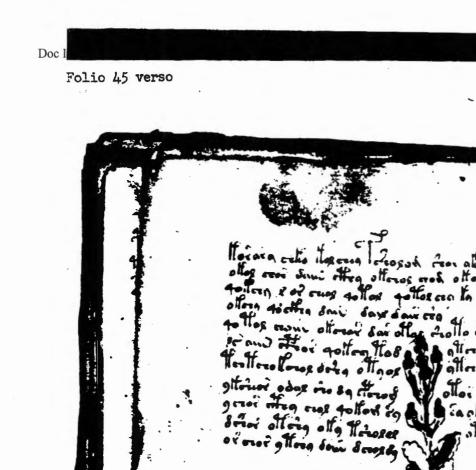




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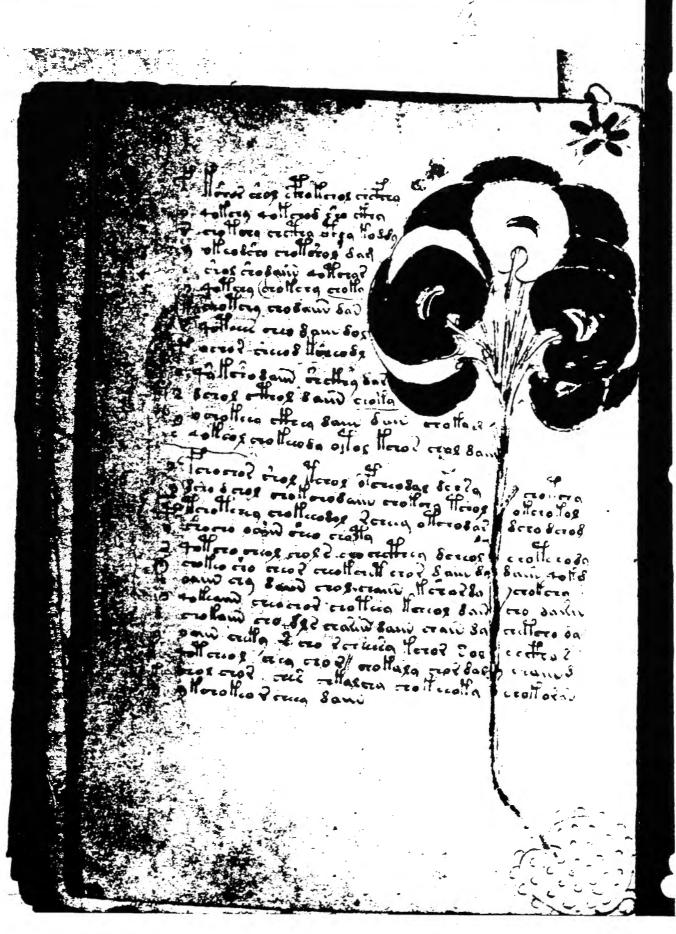
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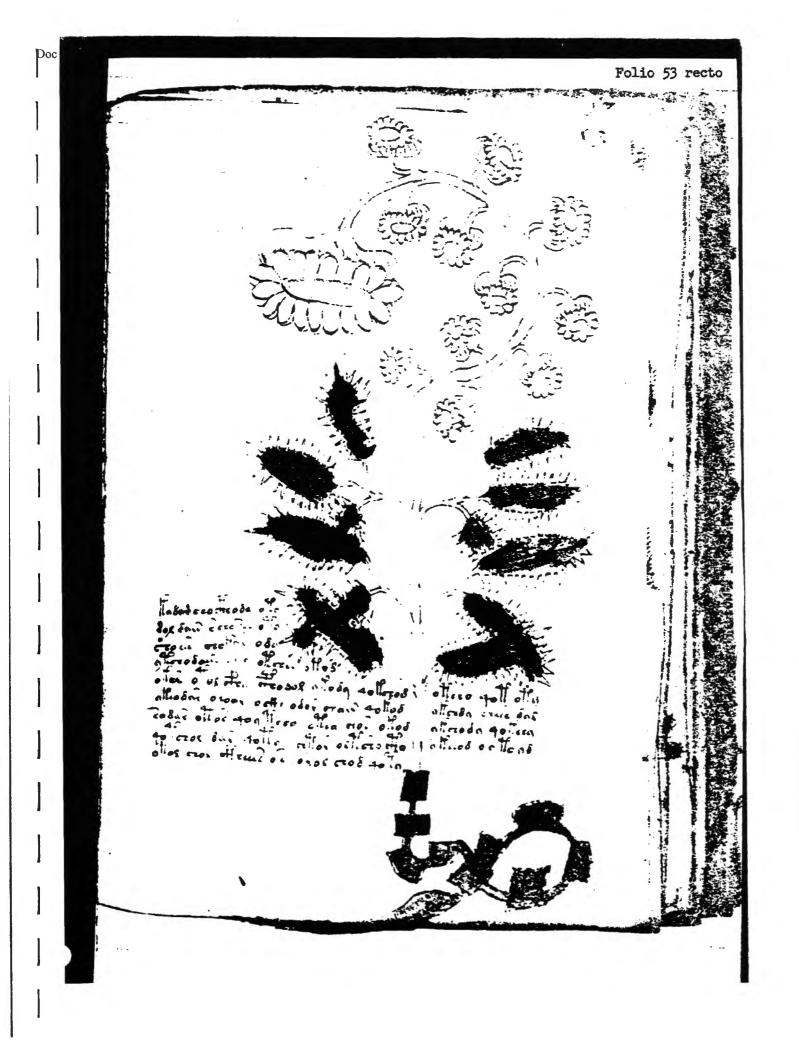
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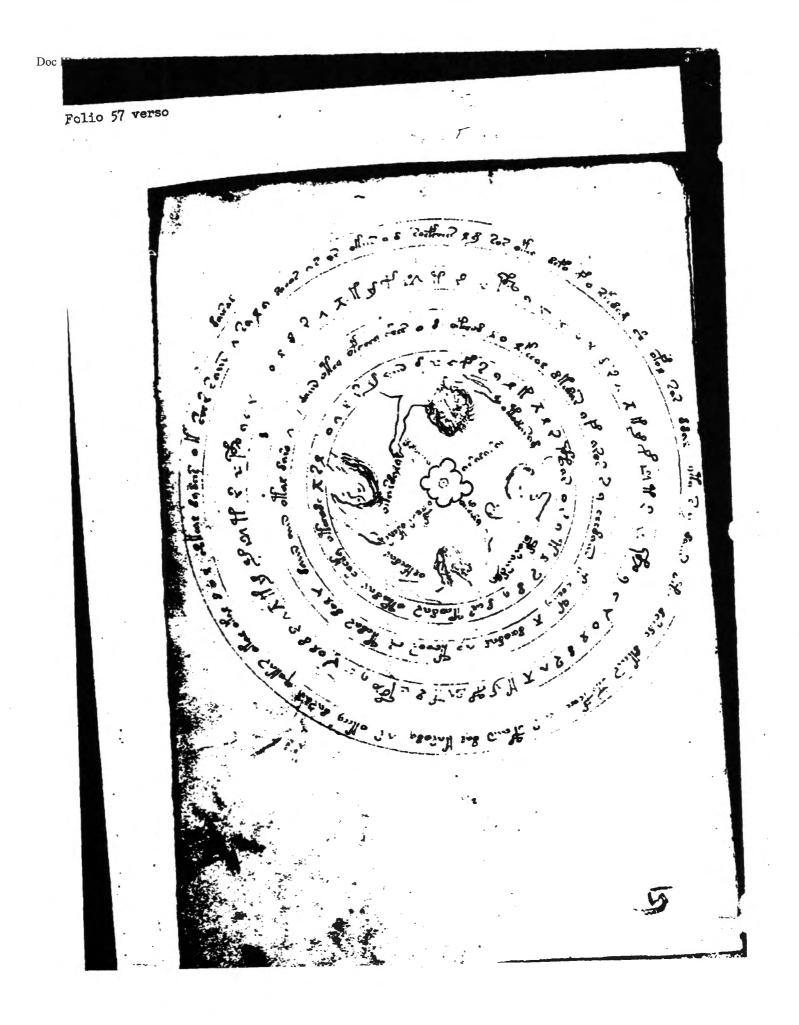
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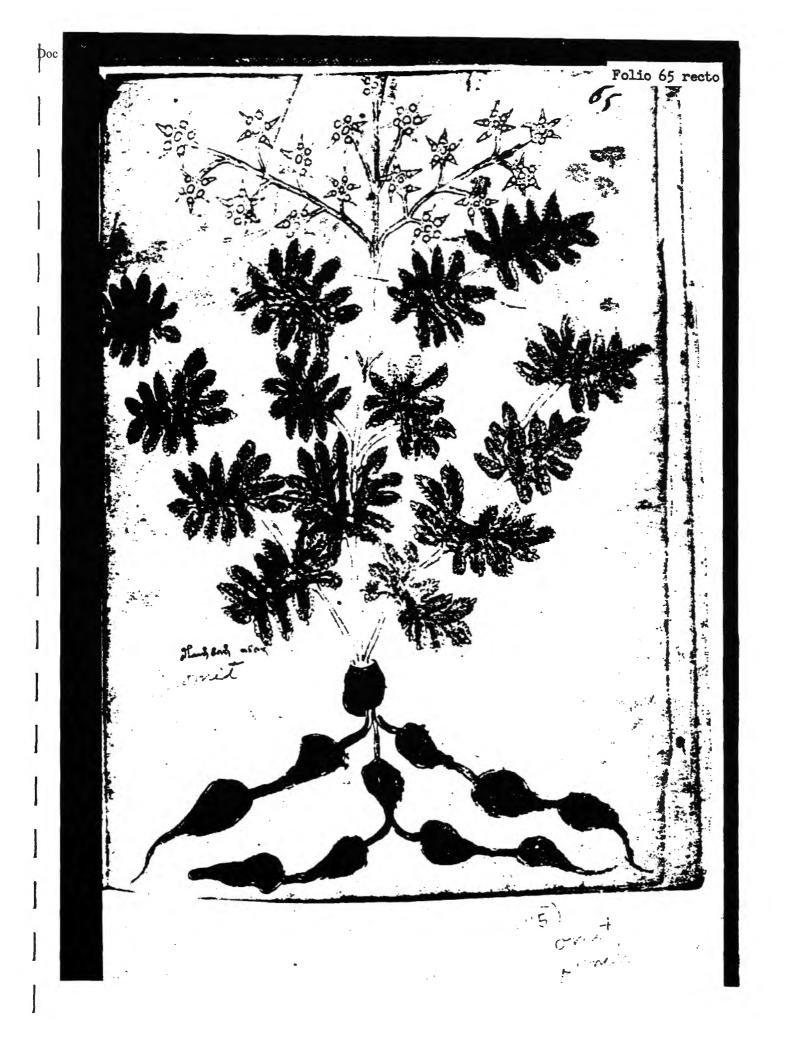
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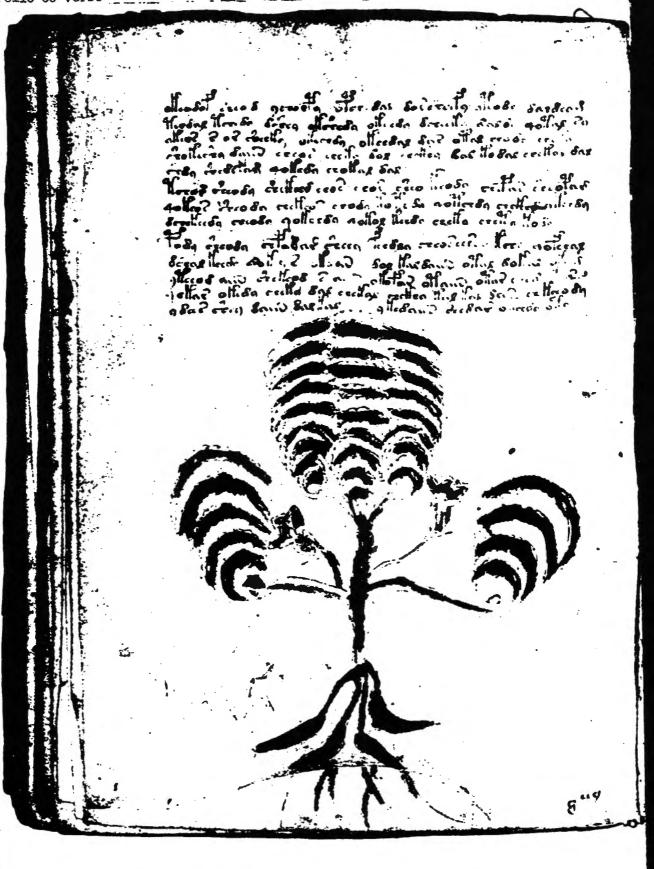




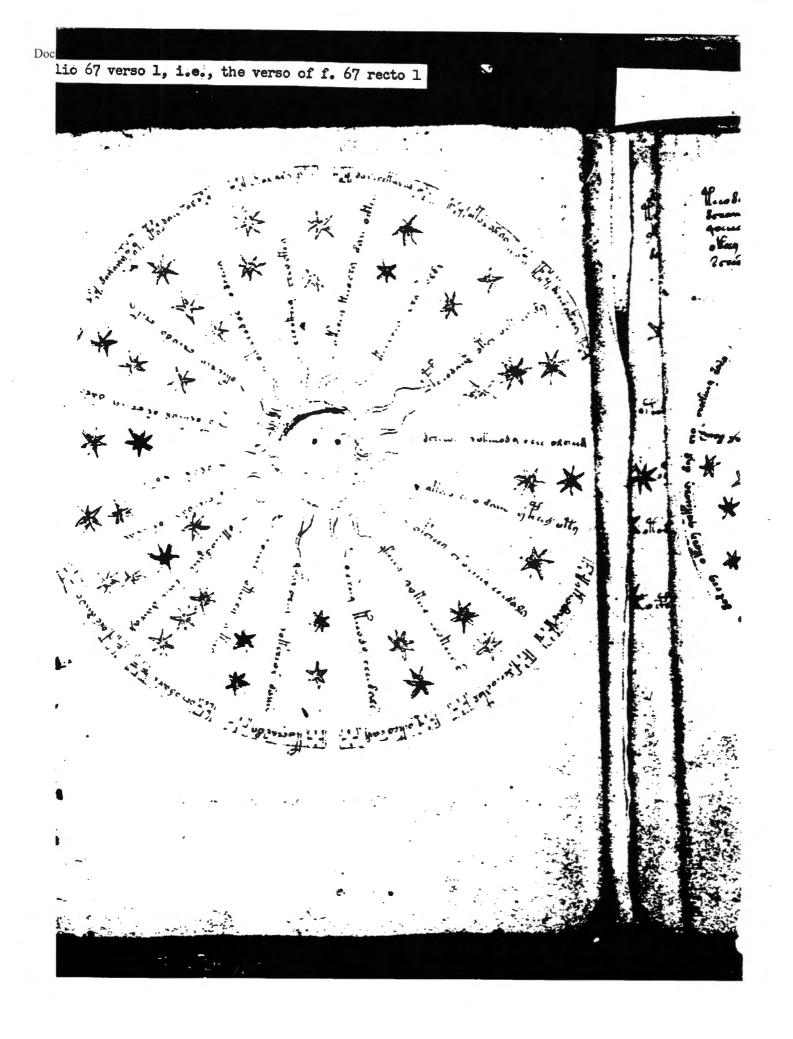
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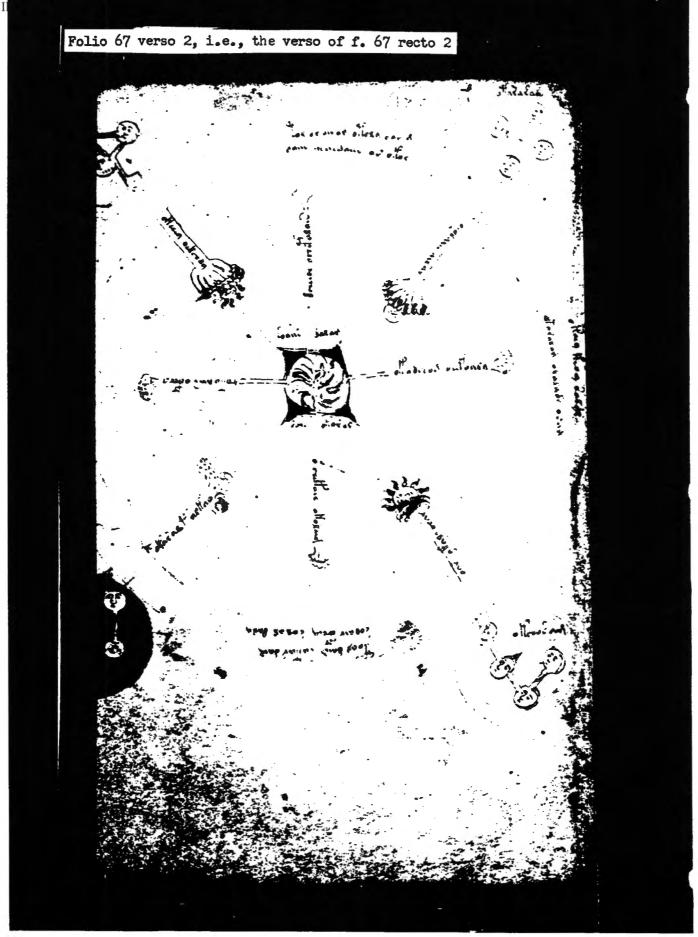
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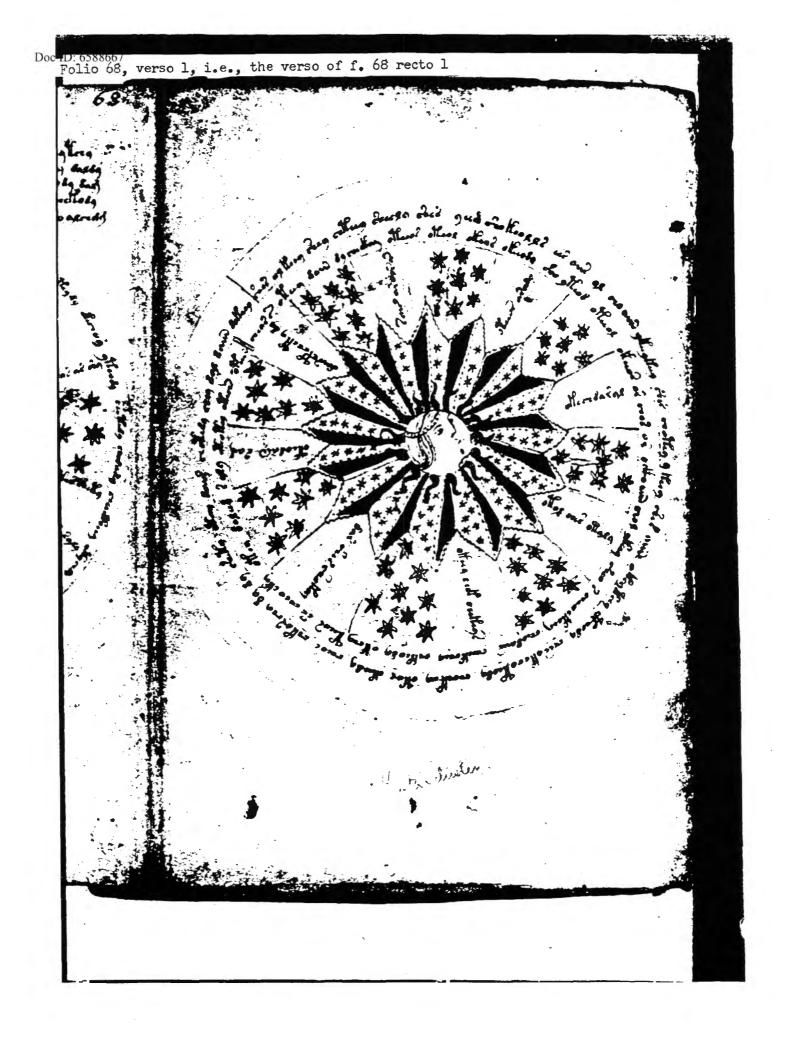


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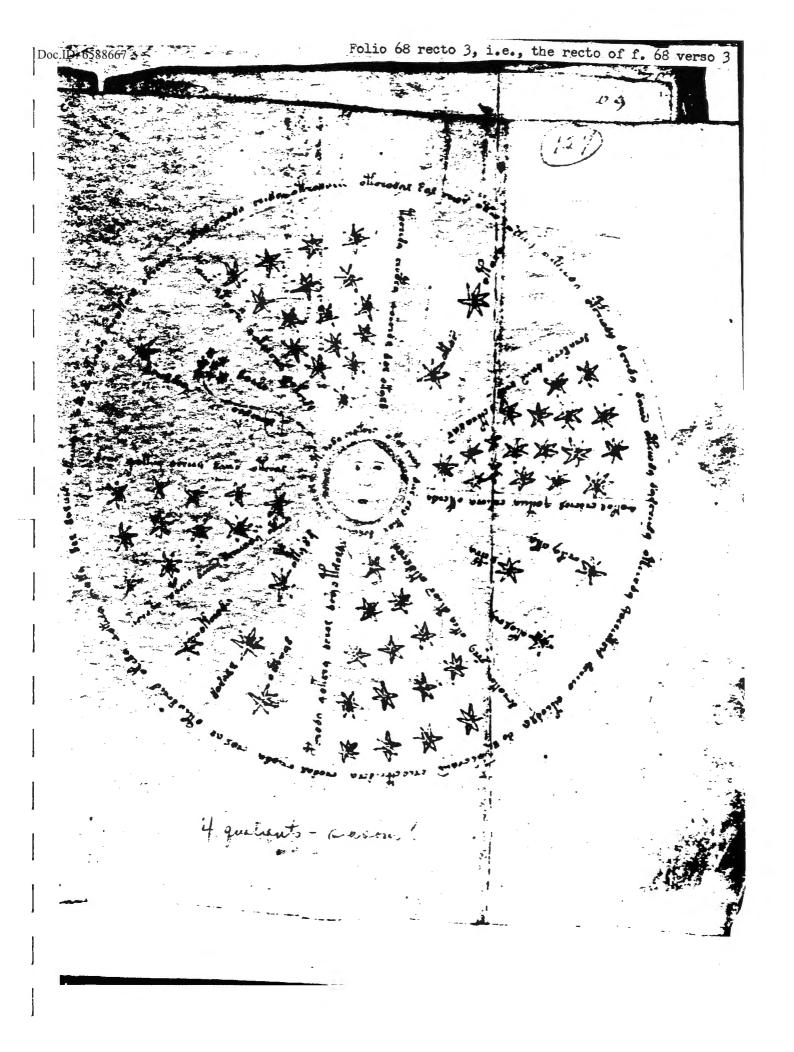
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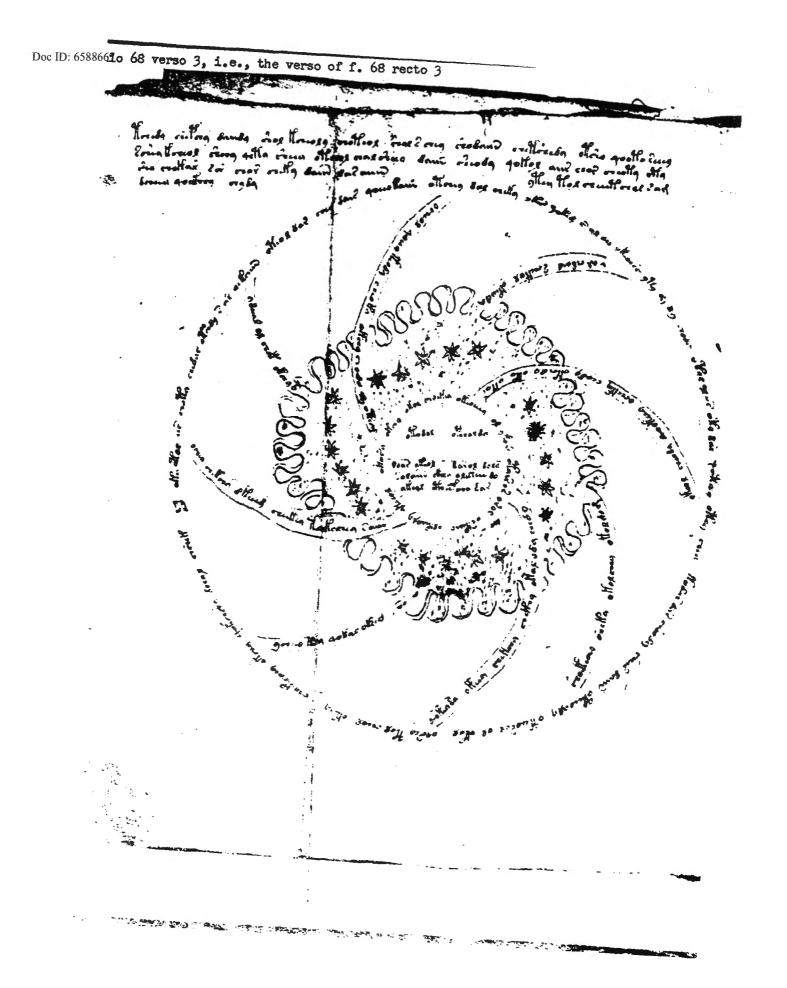
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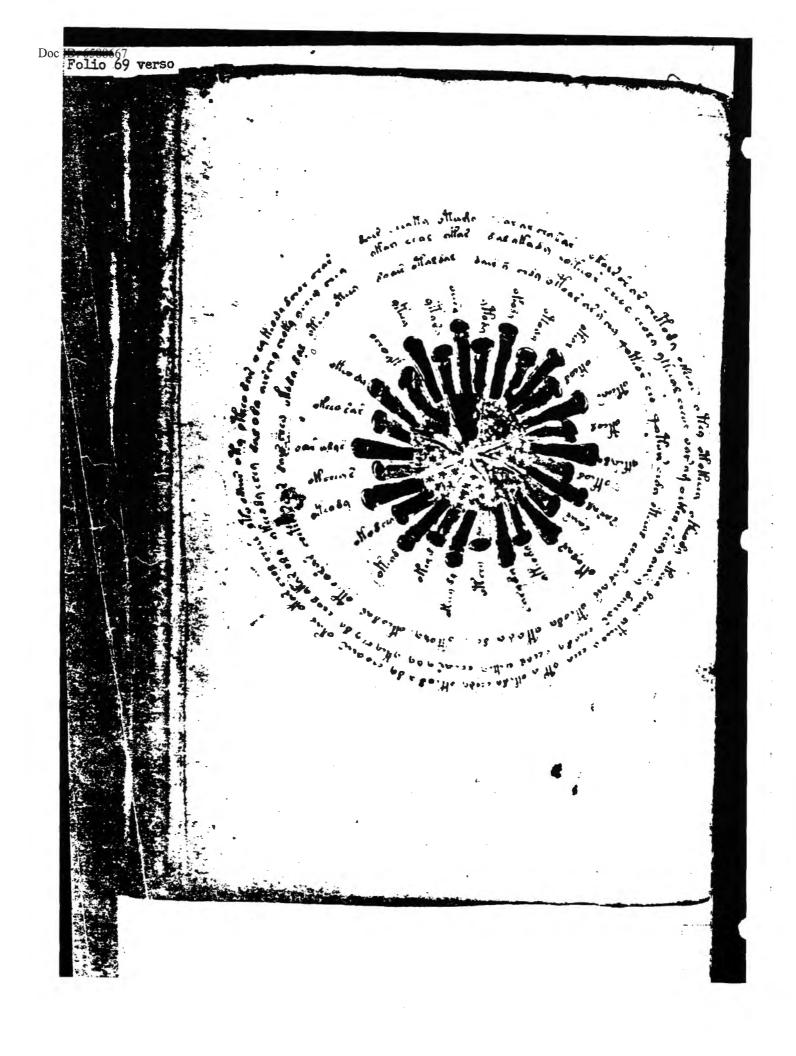
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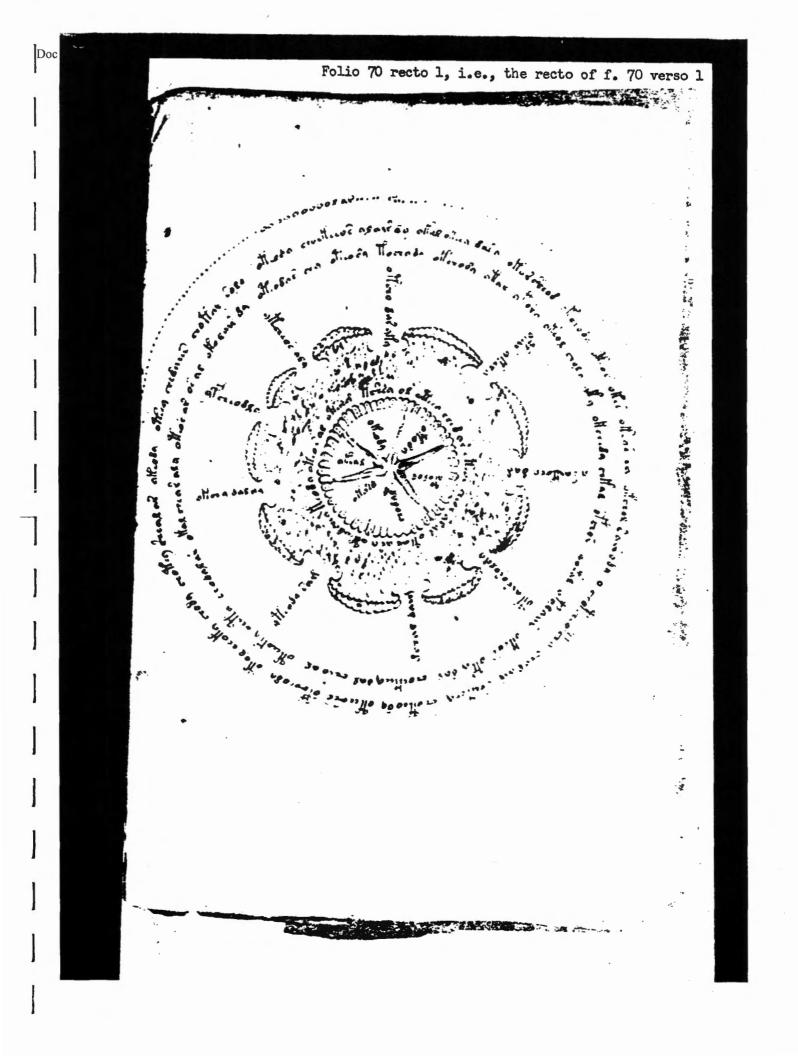
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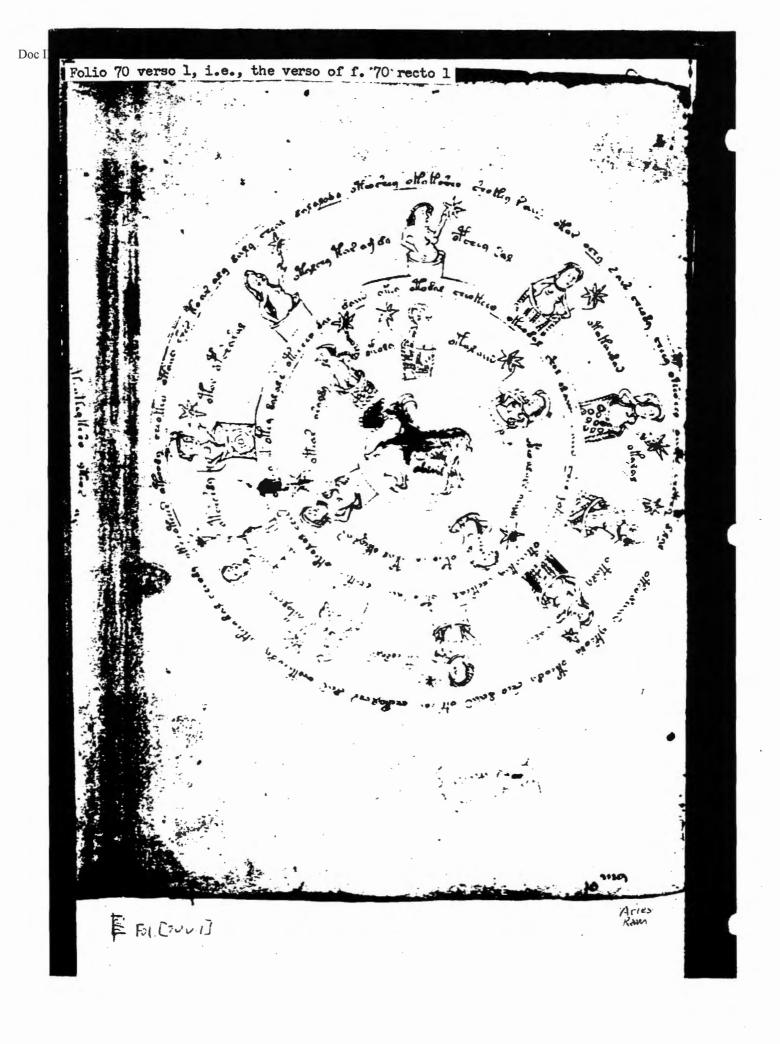


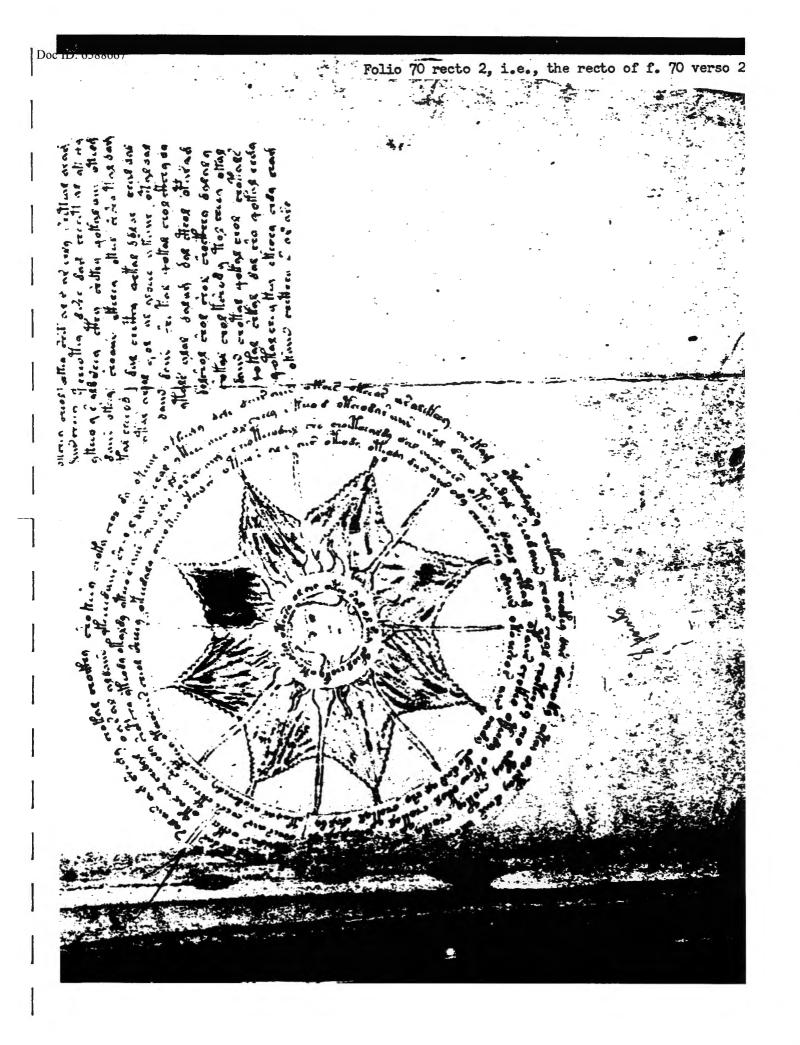


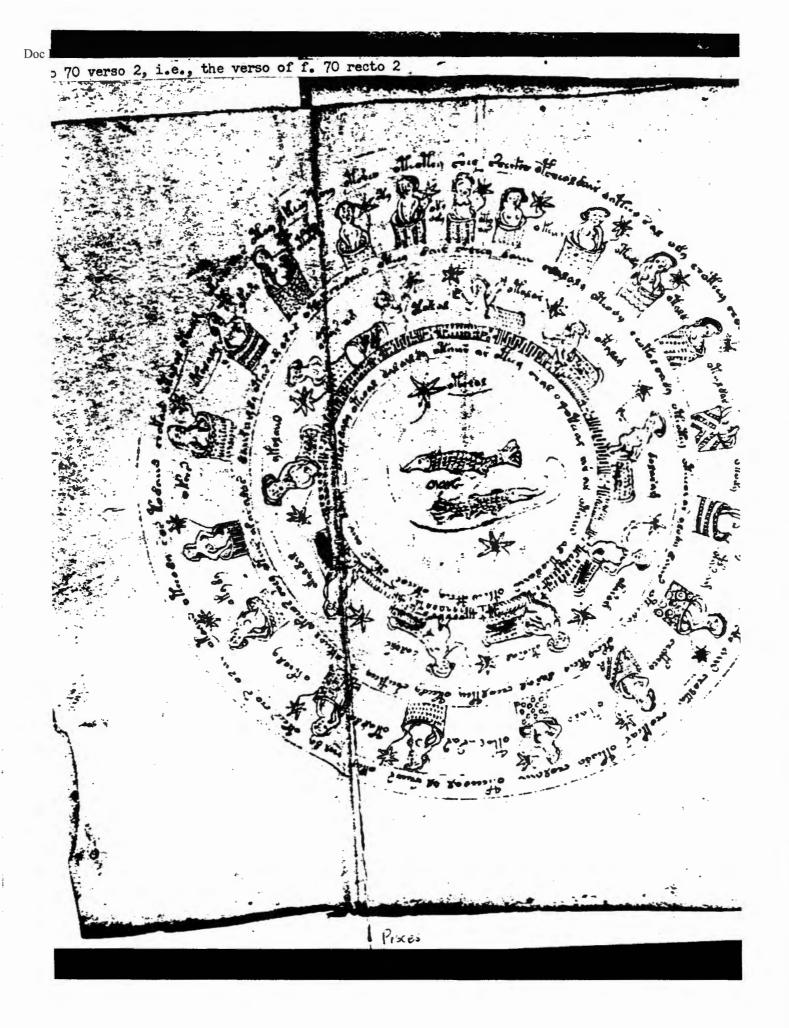
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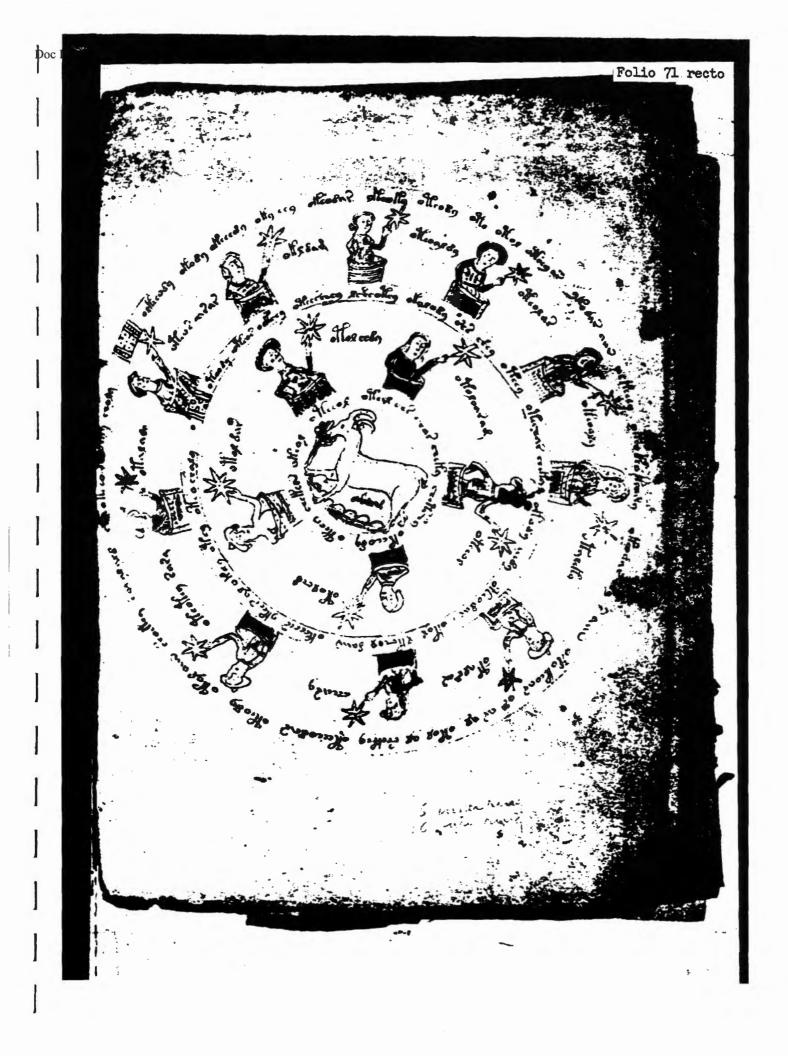




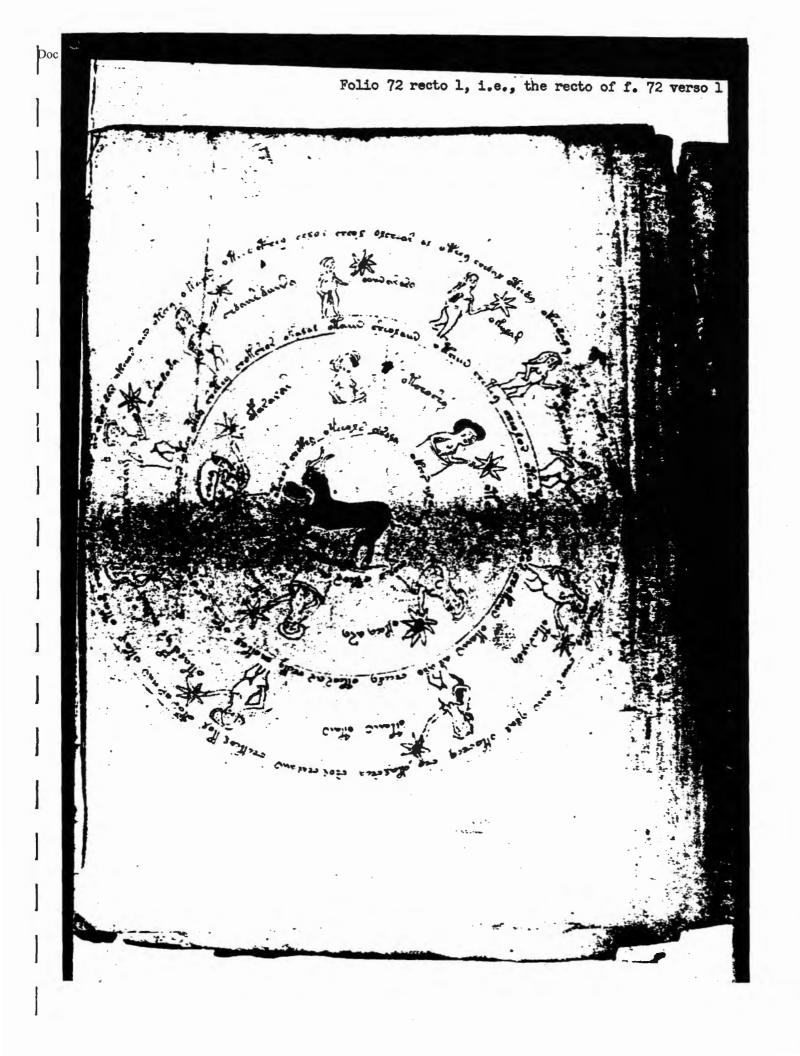


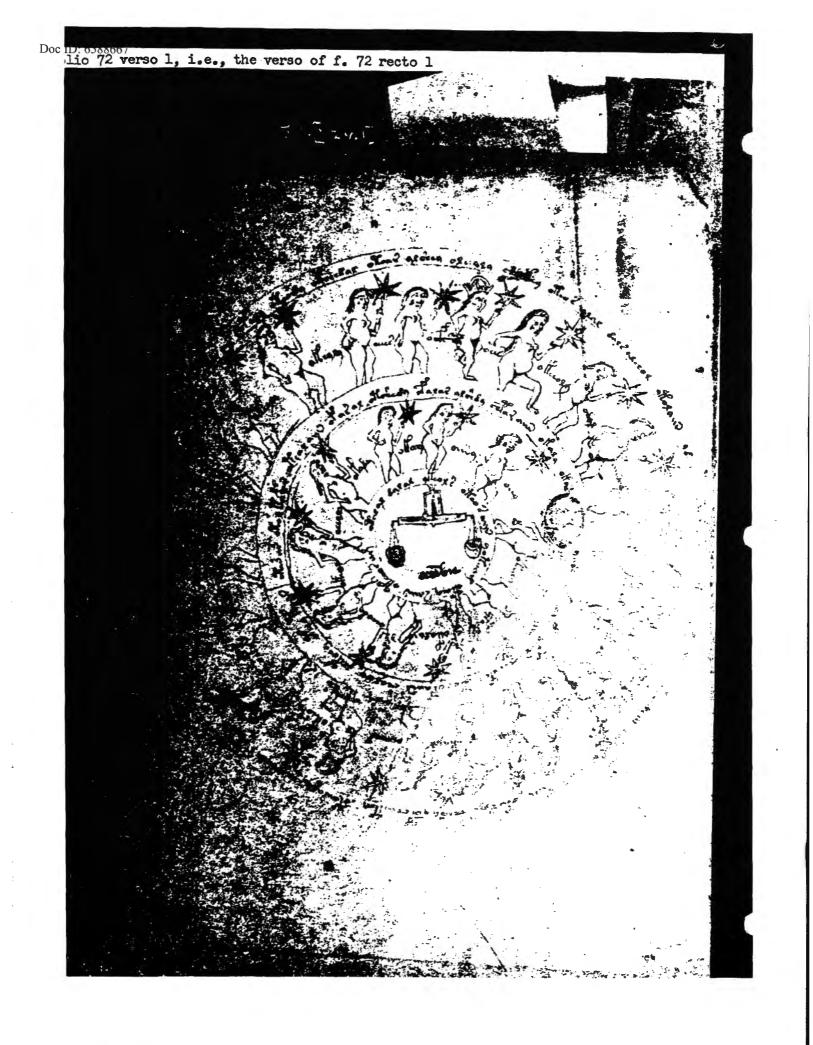


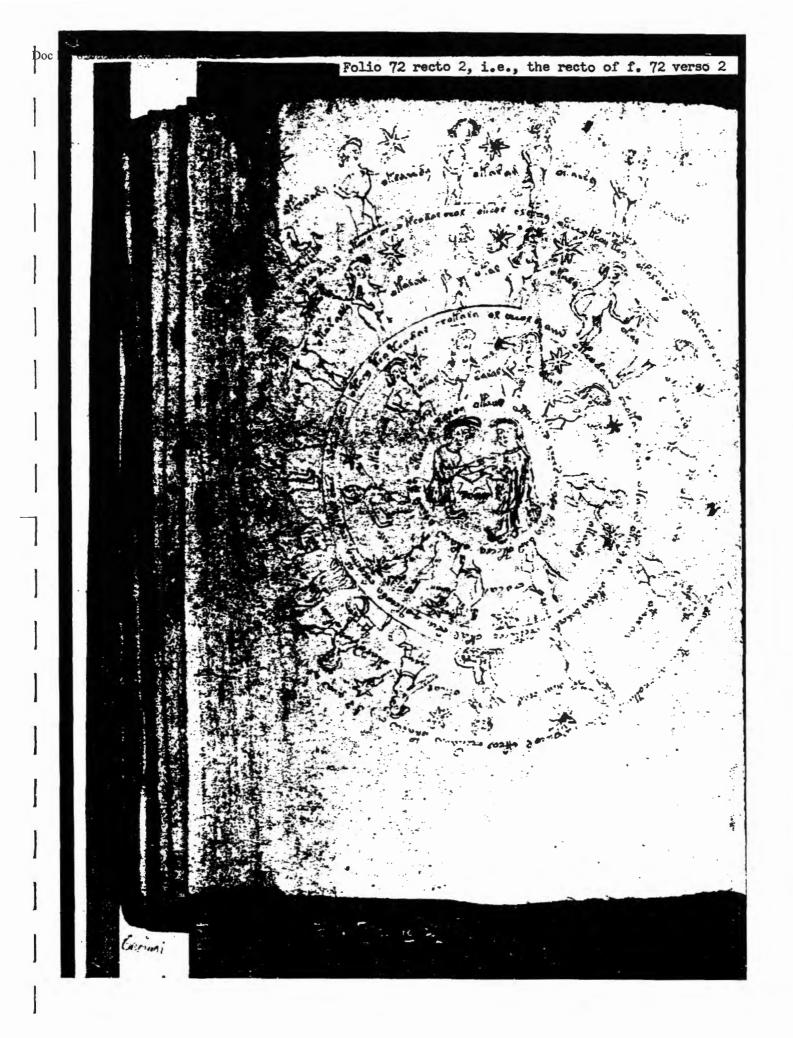


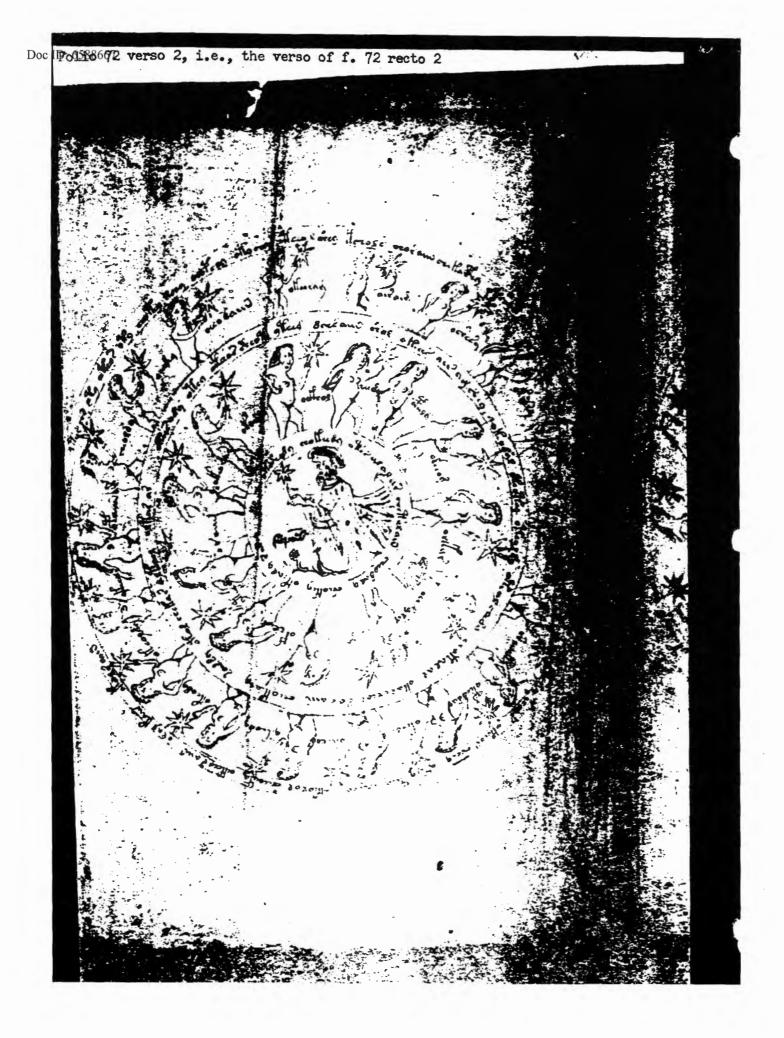


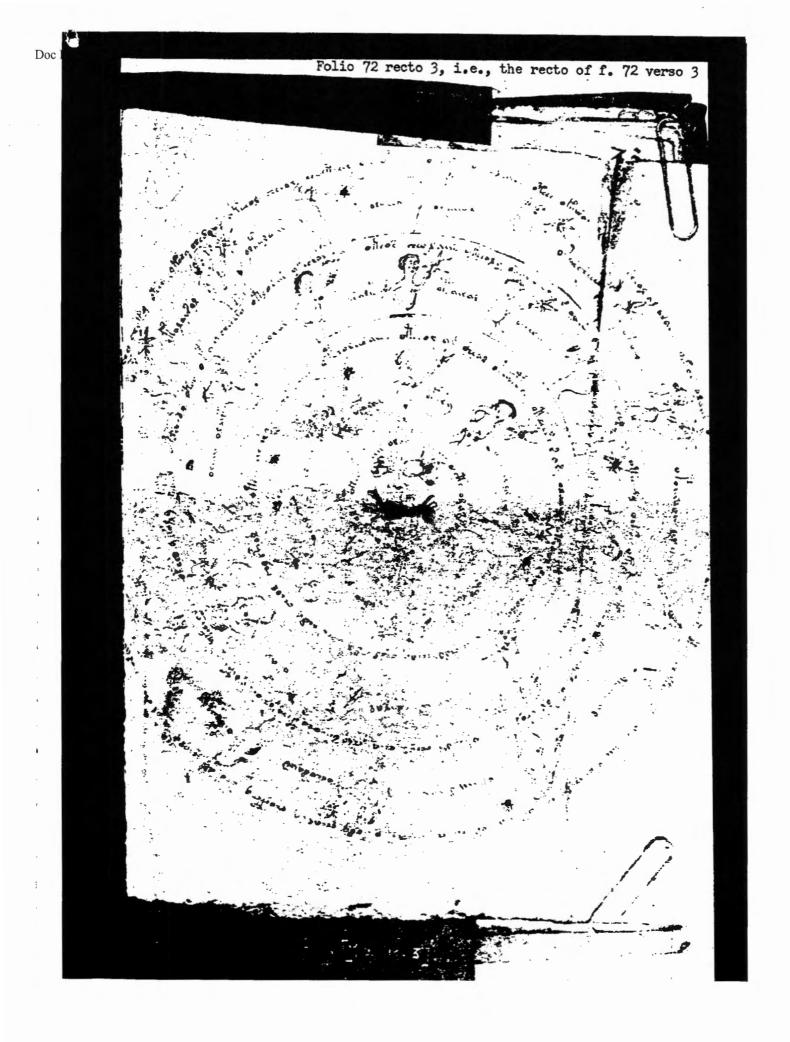
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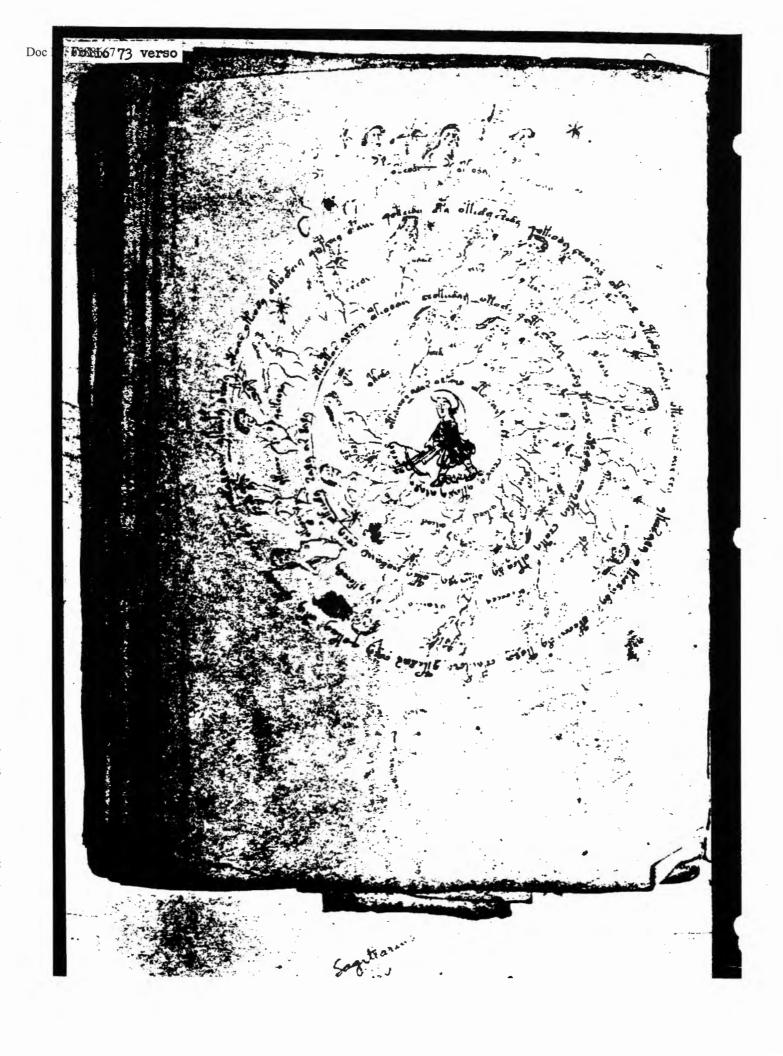








Doc 58866 Folio 73 recto . 3.



Folio 74

Doc

This folio was missing when the manuscript was discovered by Voynich in 1912.

Note folio 75 recto and the evidence that folio 74 was excised at some earlier time.

Folio 75 recto.

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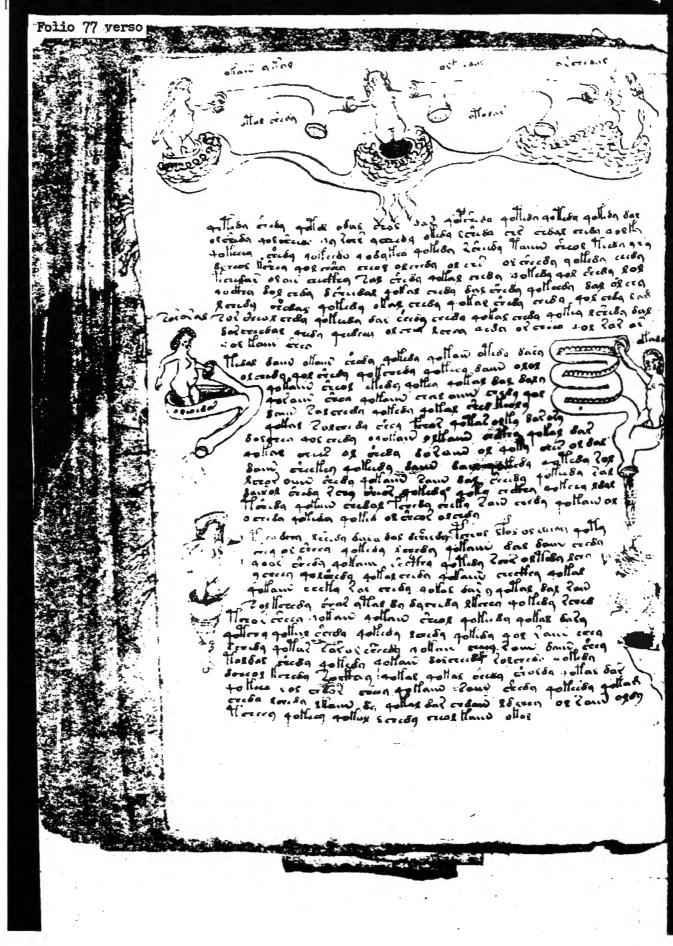
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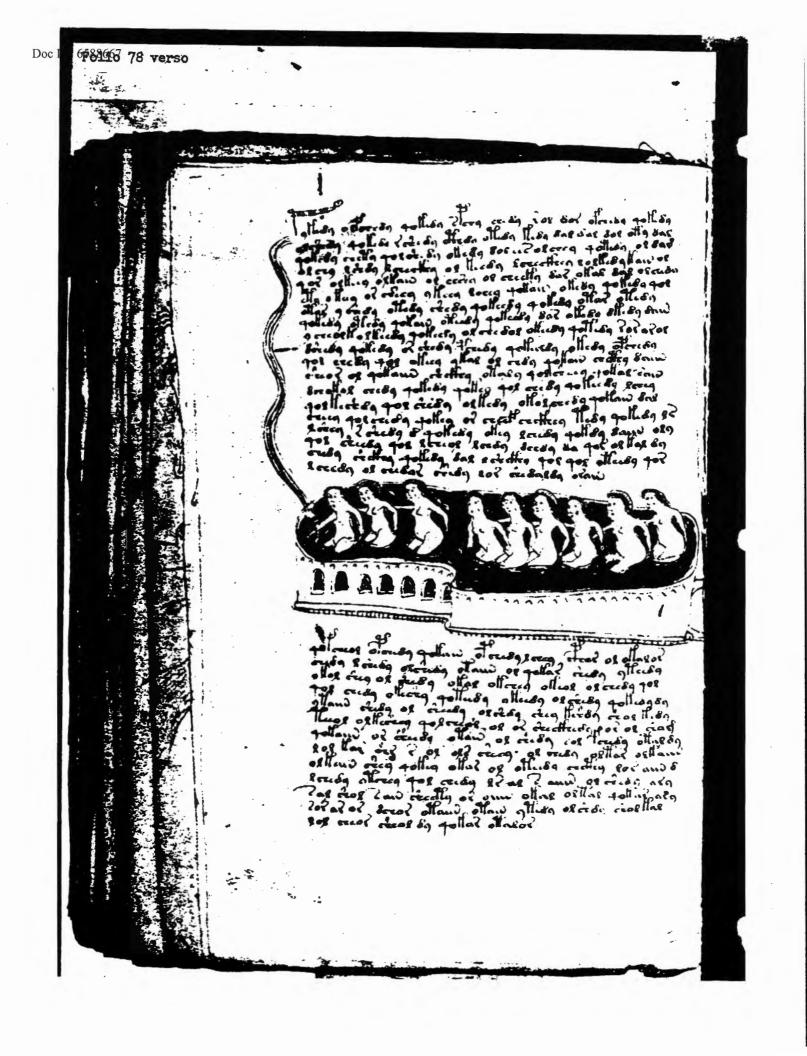
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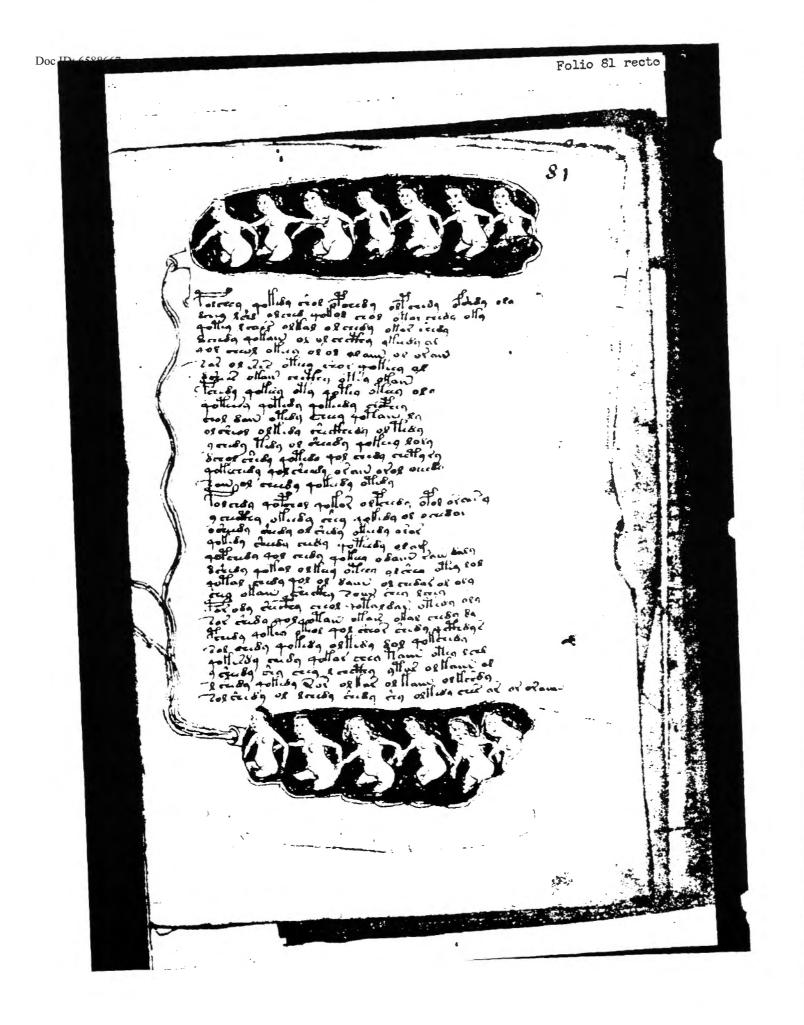
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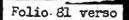
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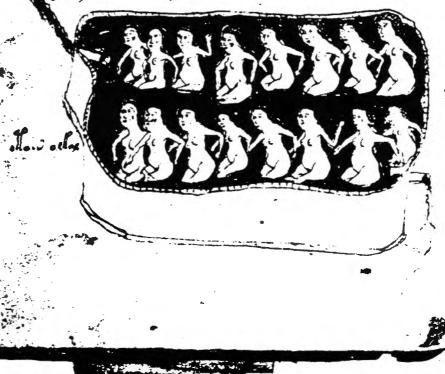




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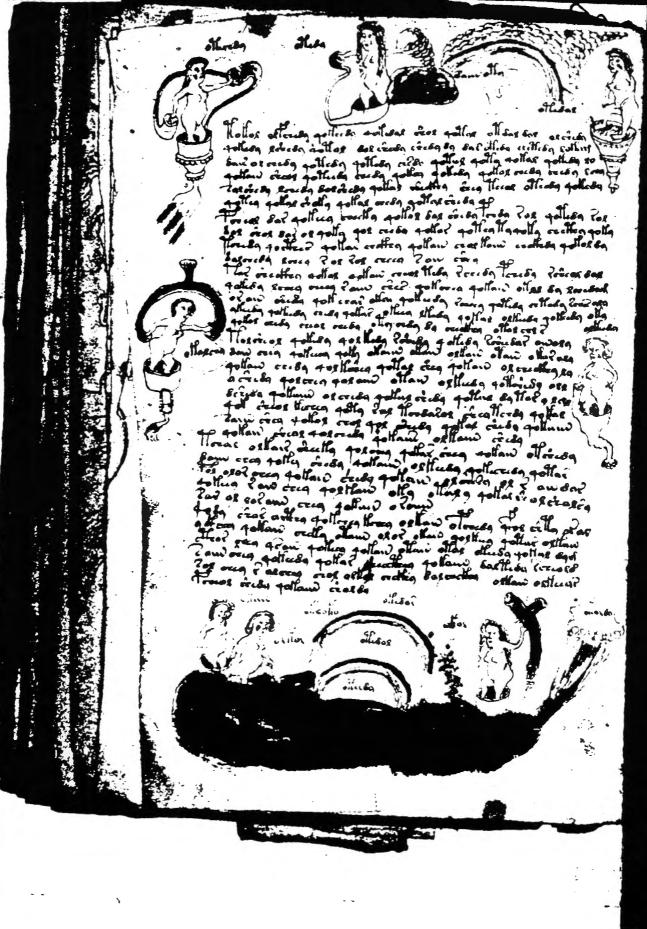
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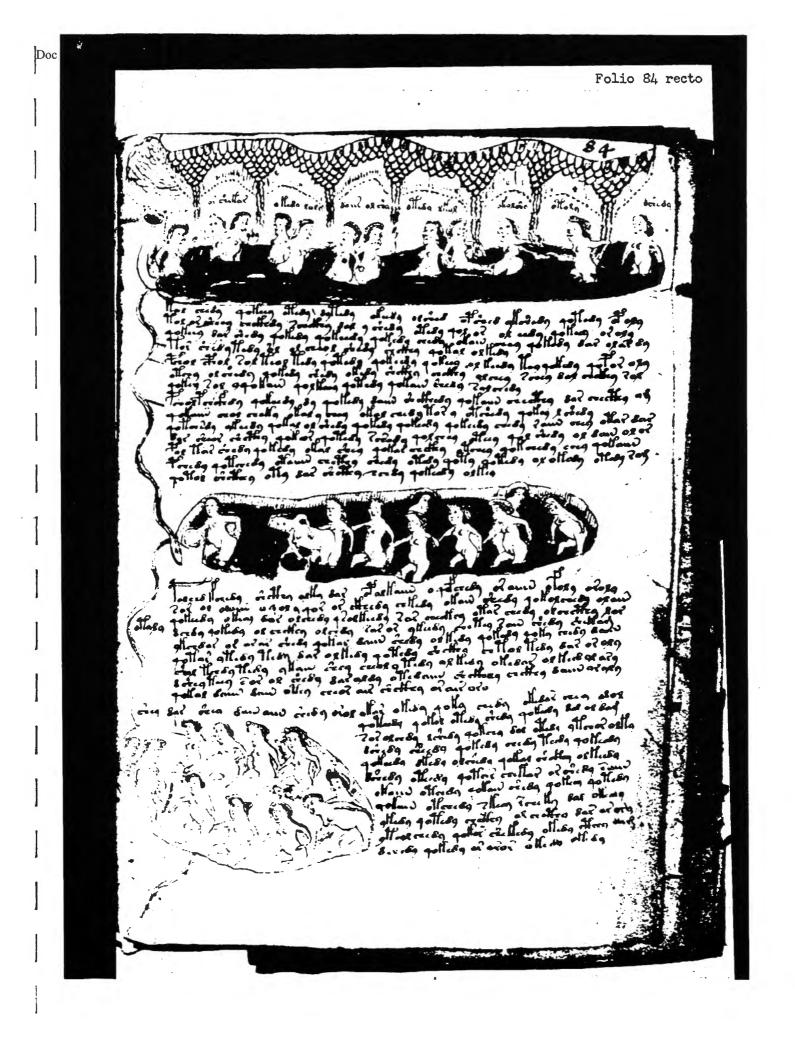
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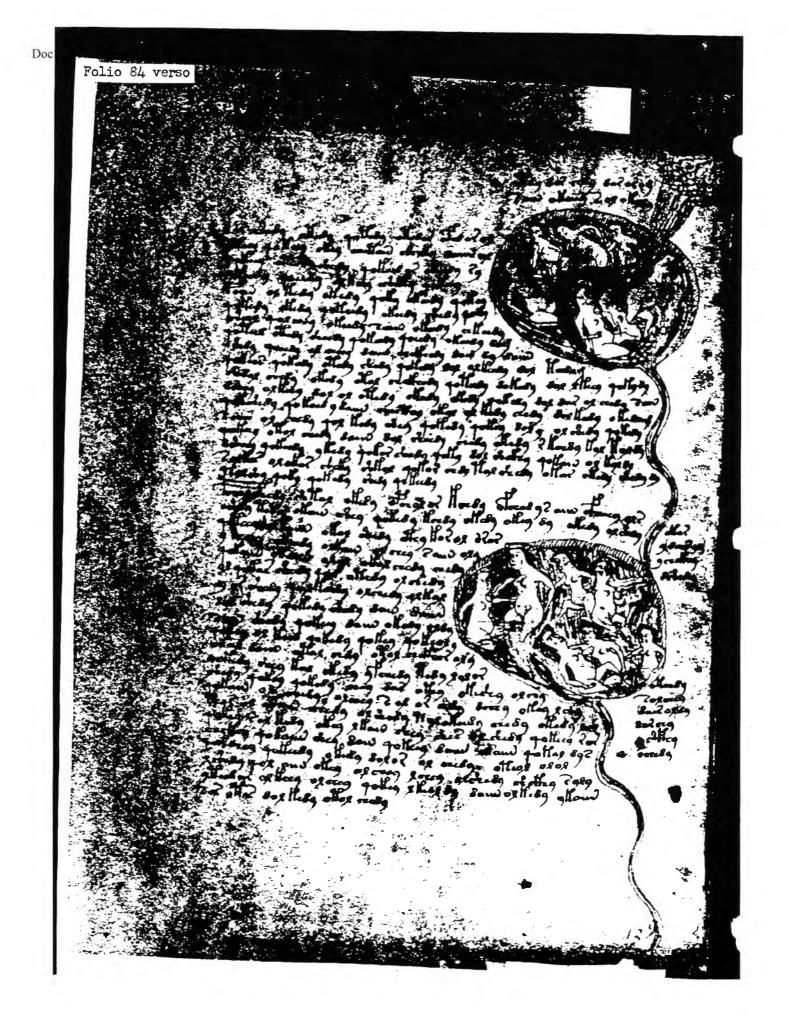


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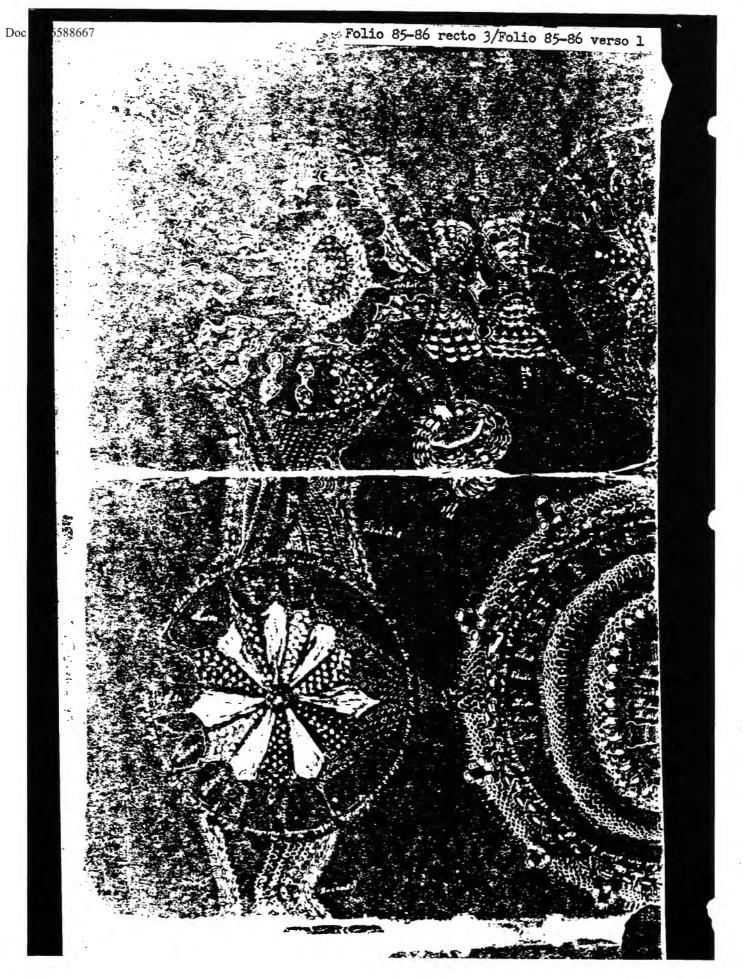




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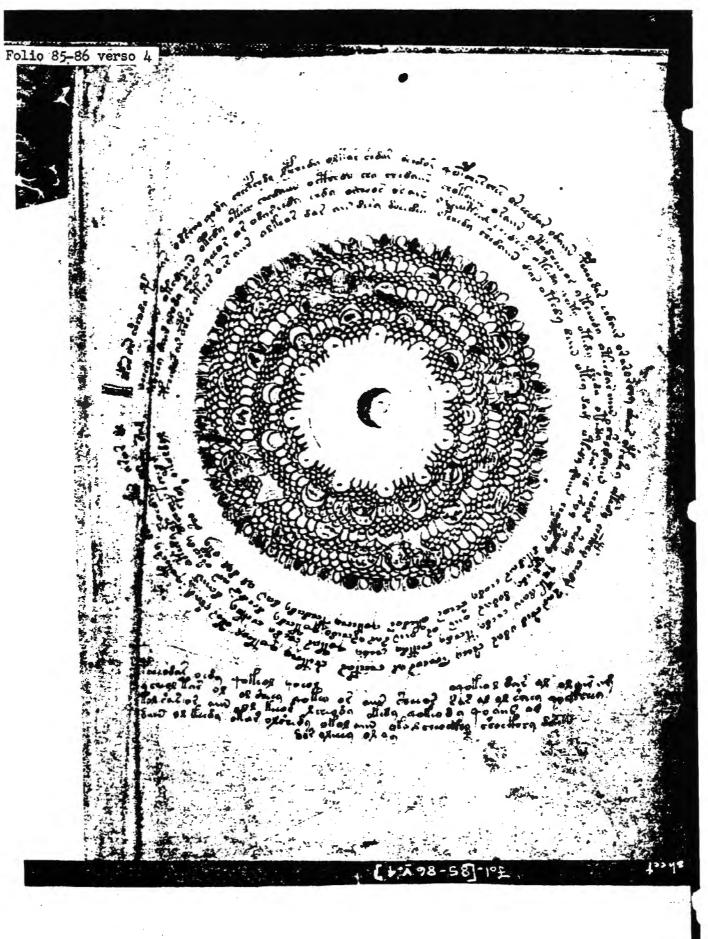
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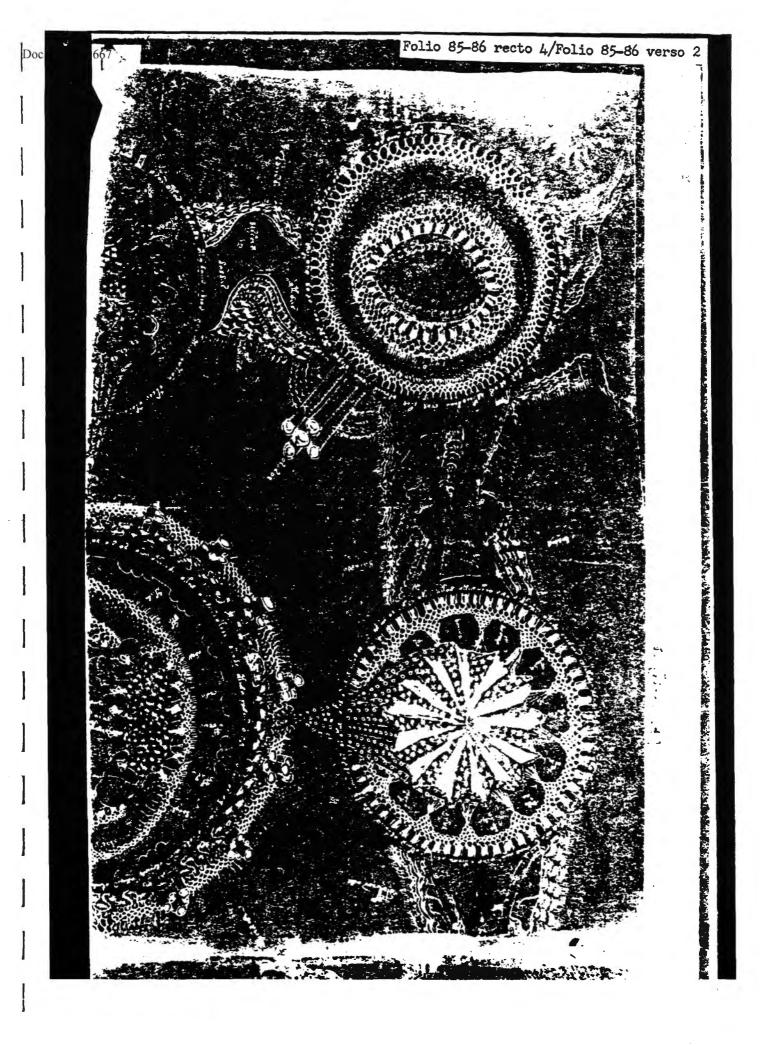
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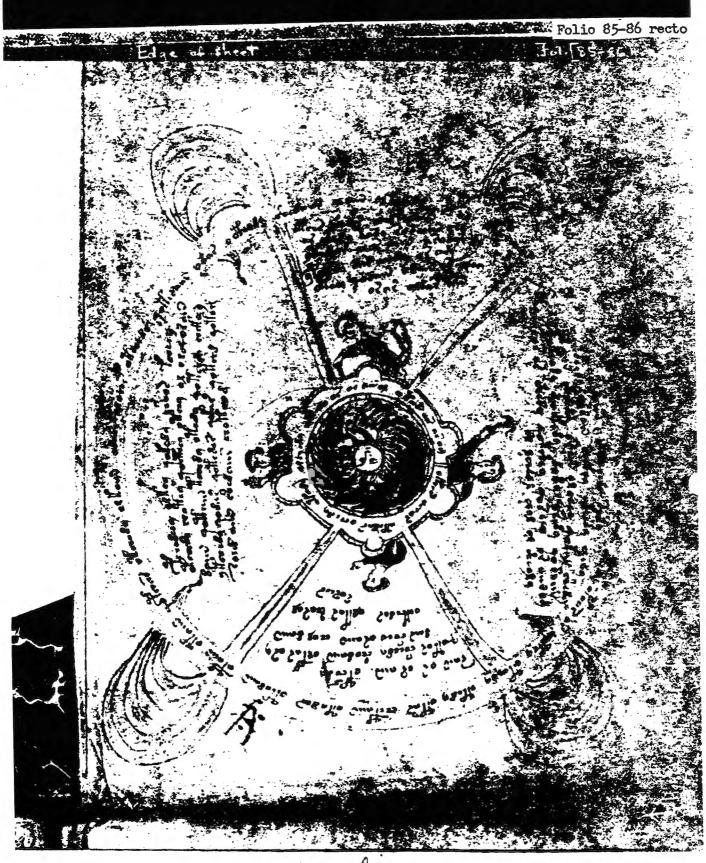
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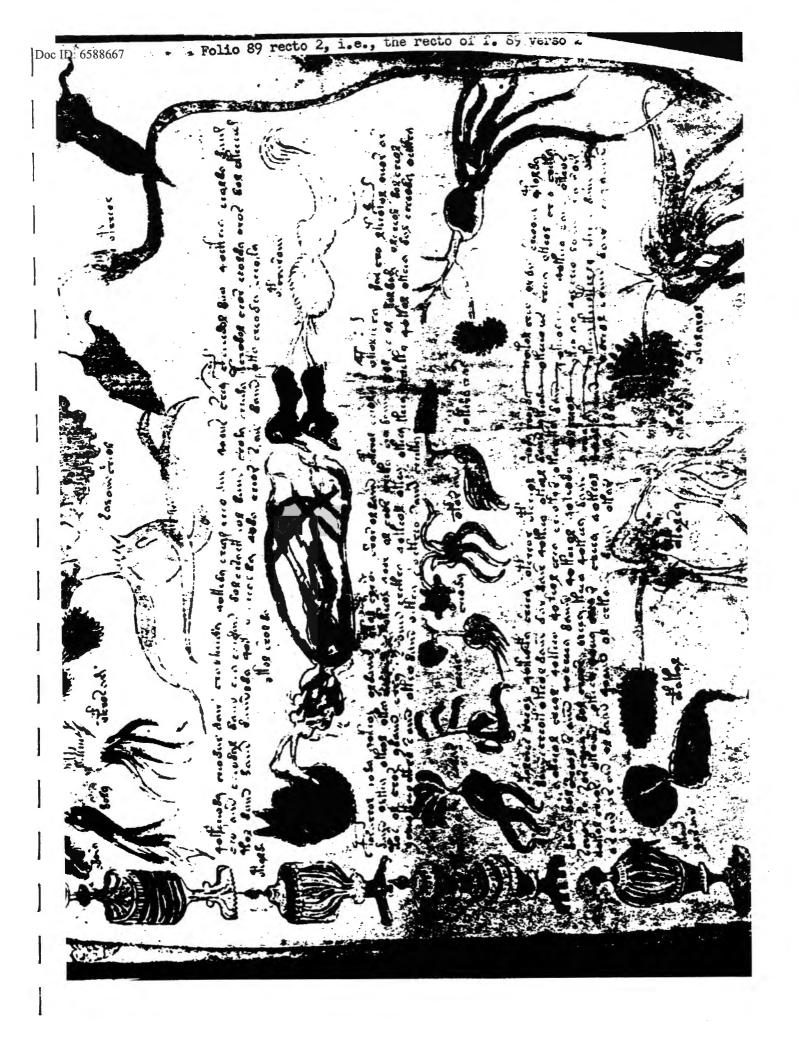


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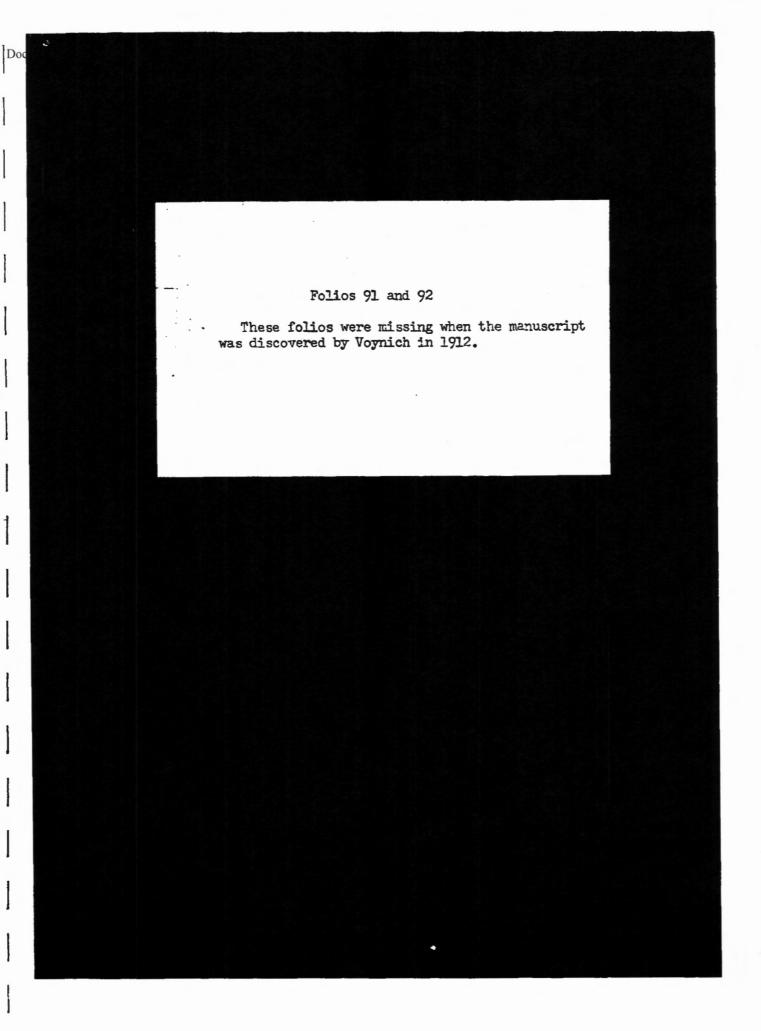


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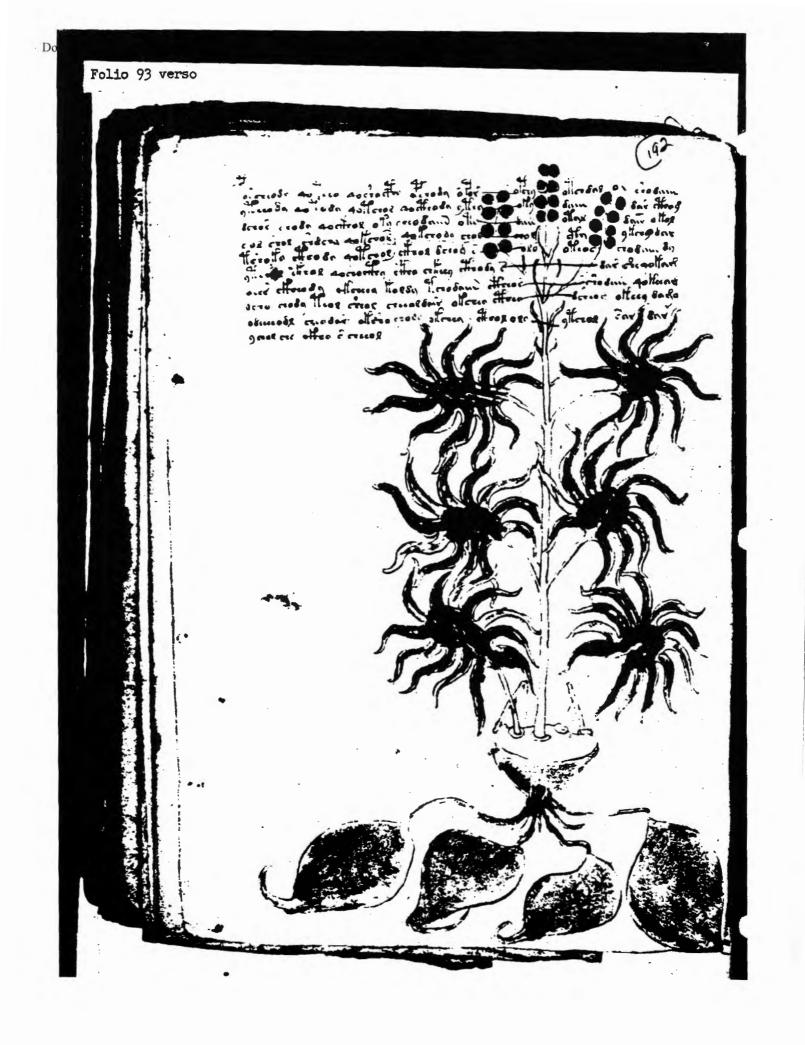
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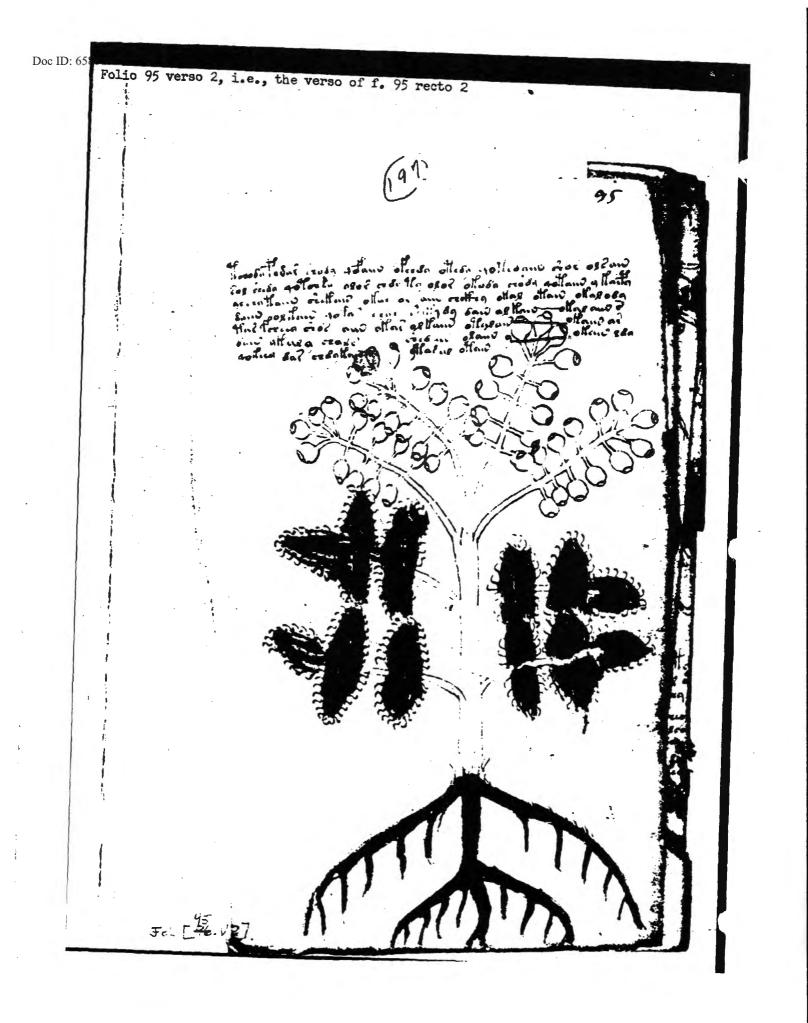
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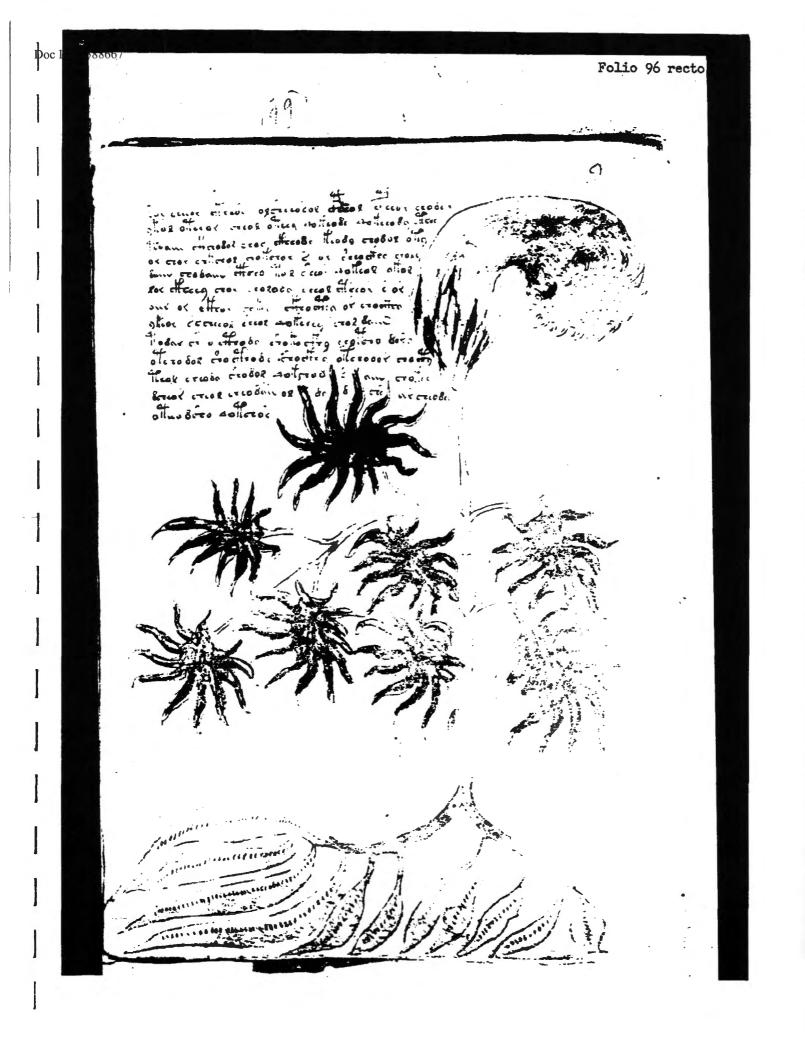
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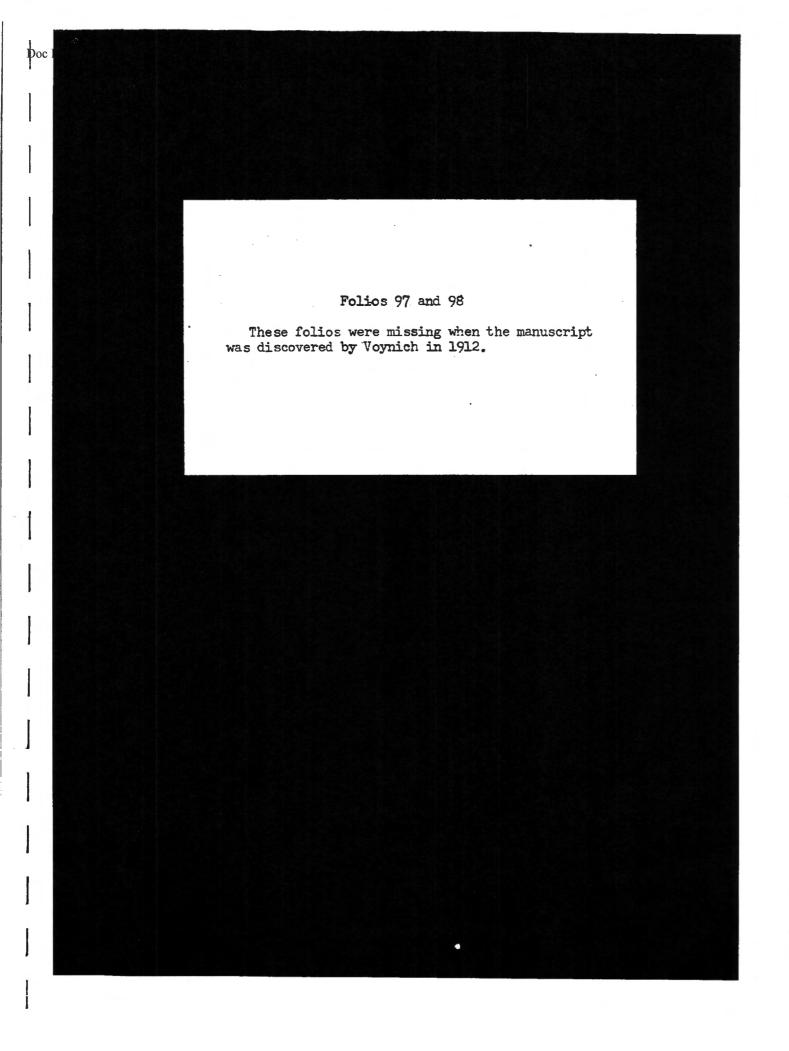
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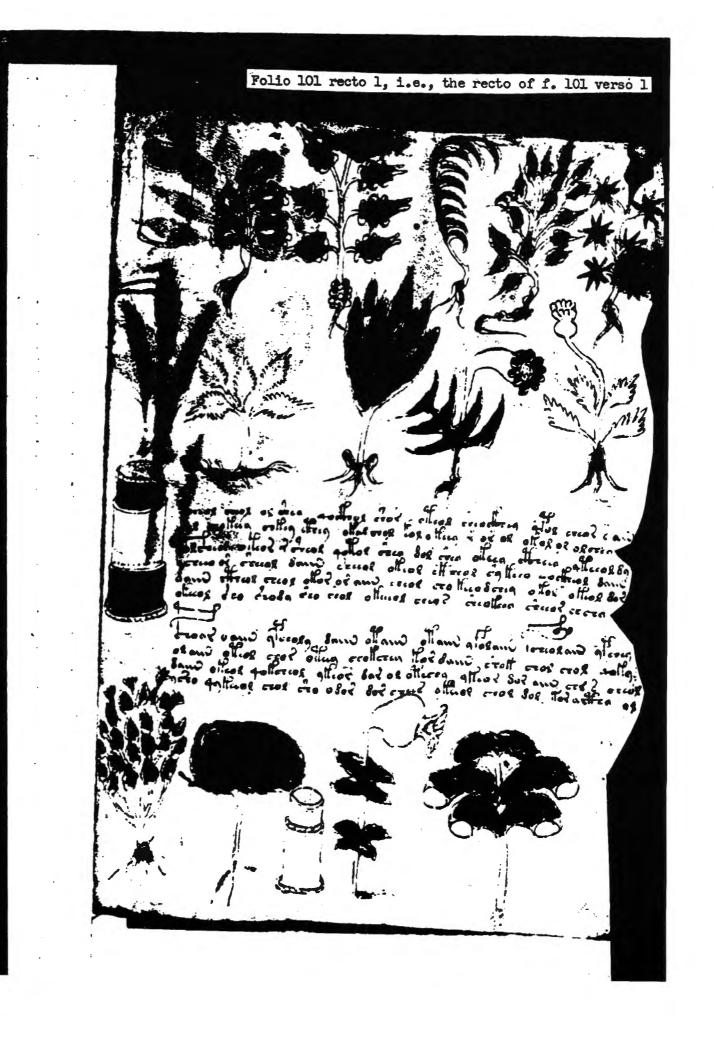
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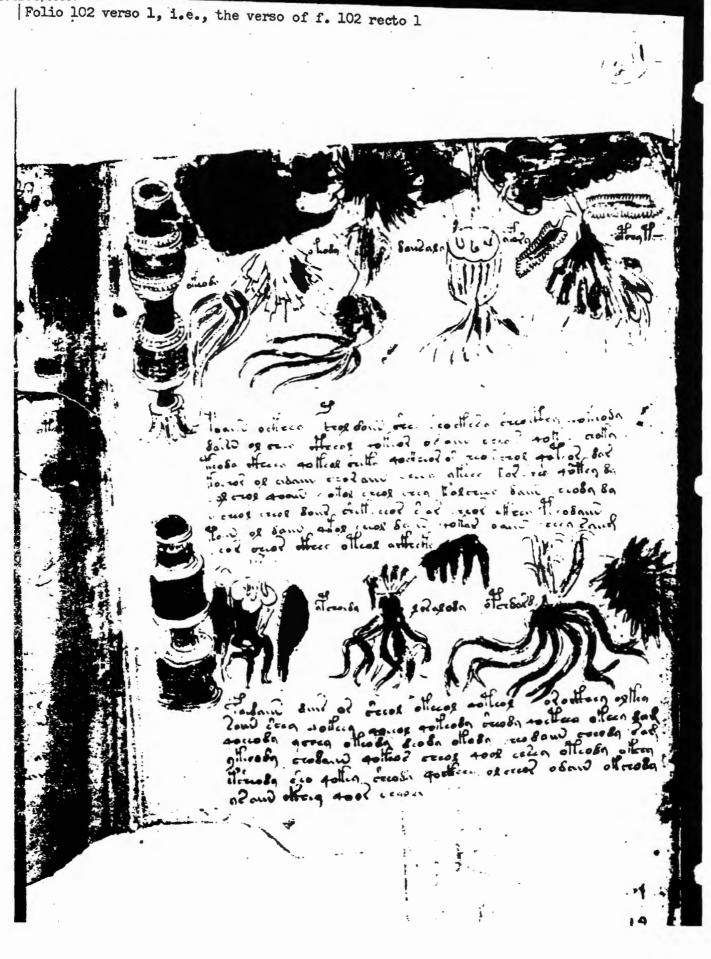
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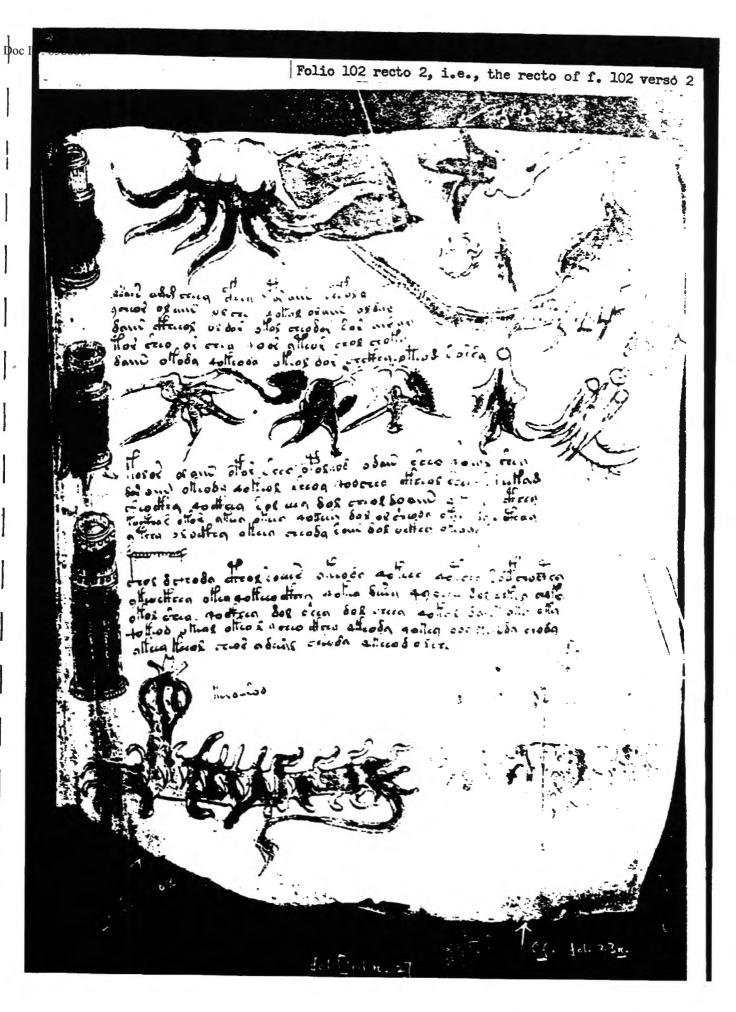




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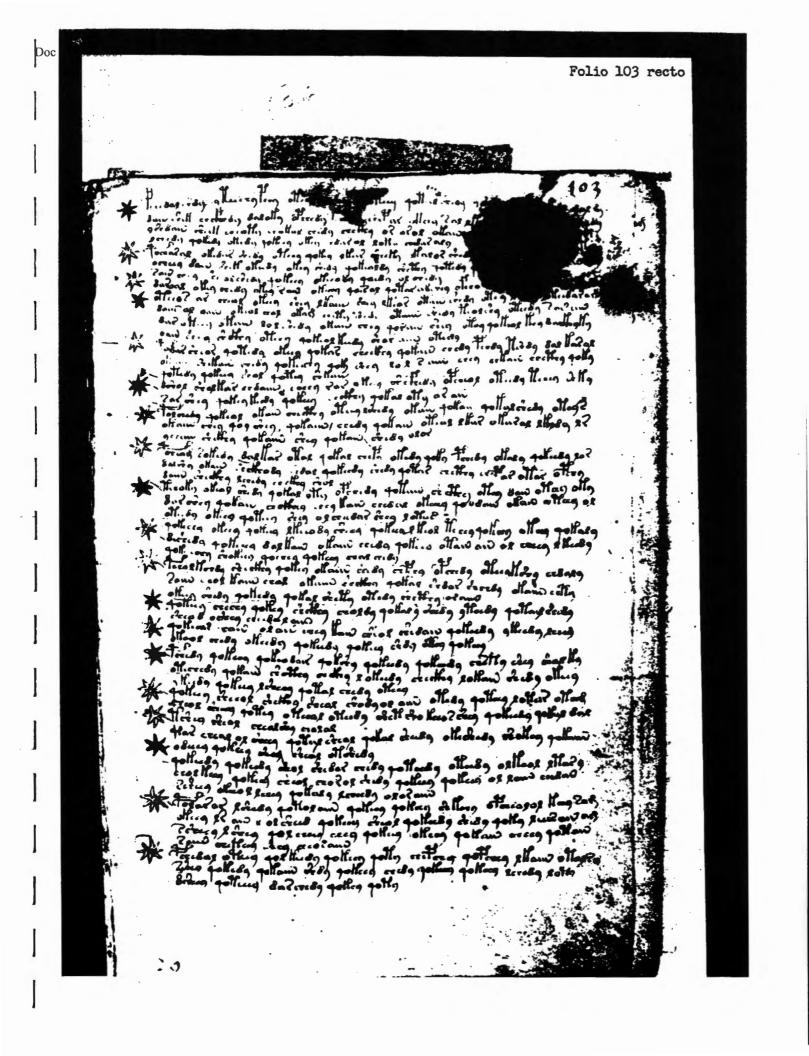
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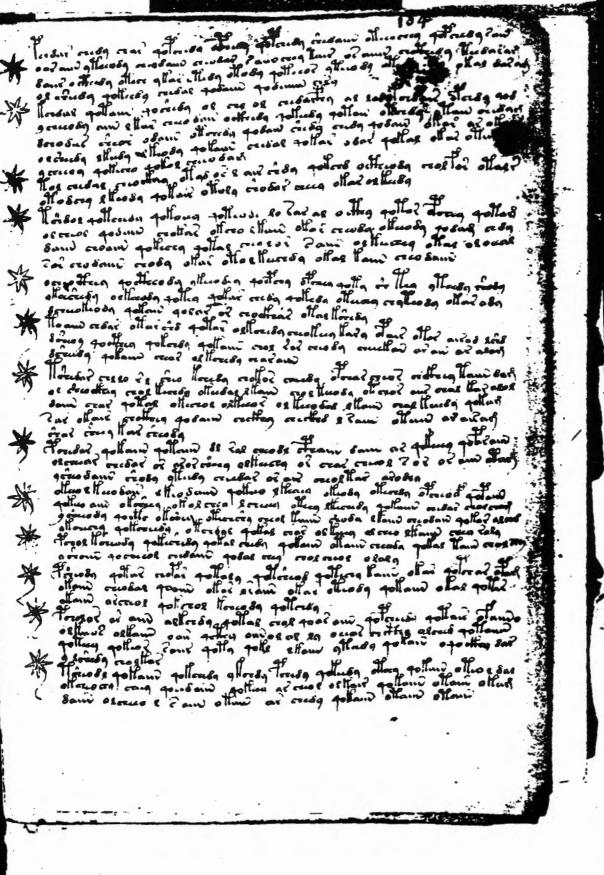
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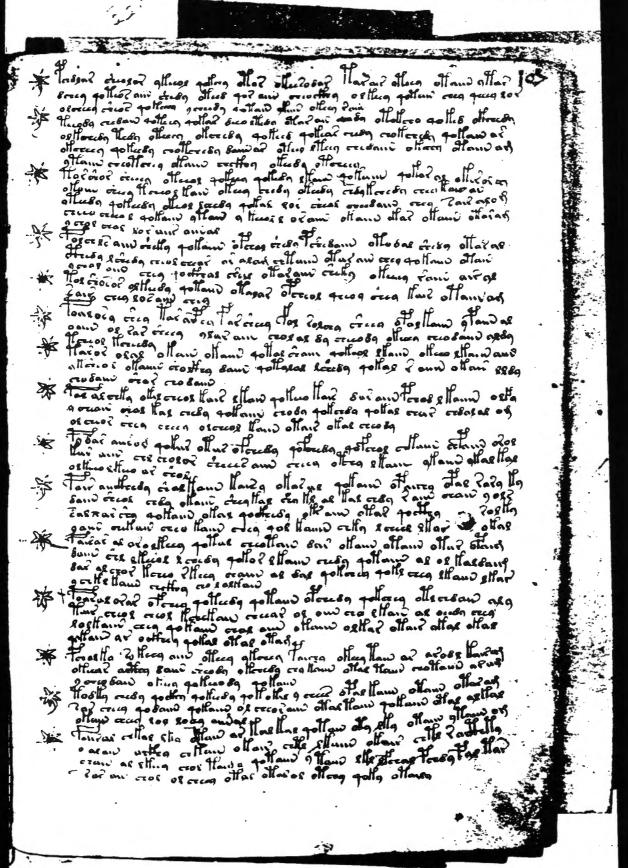
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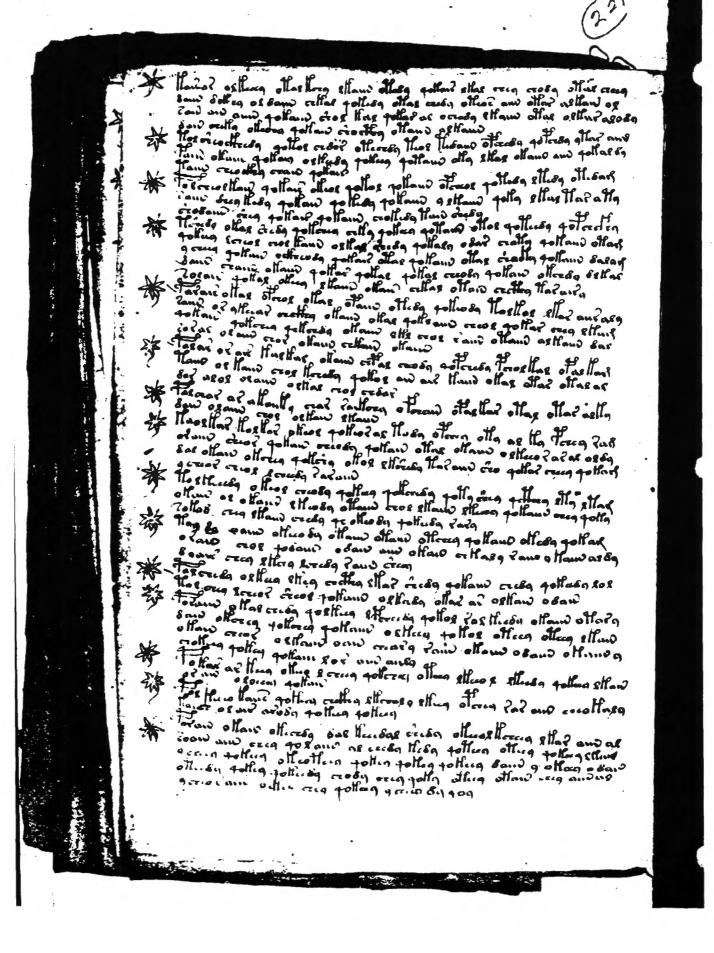
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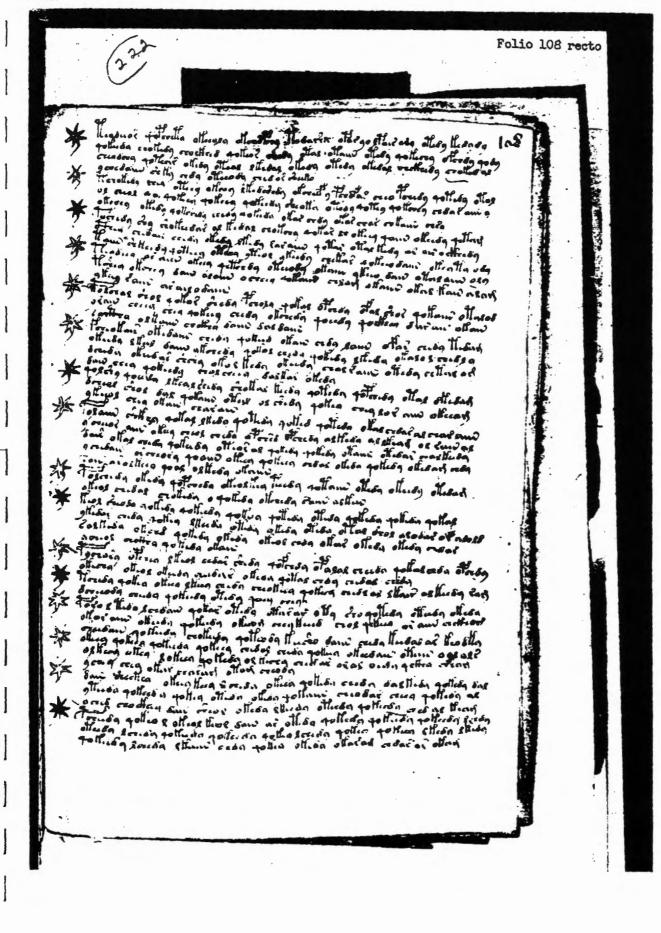
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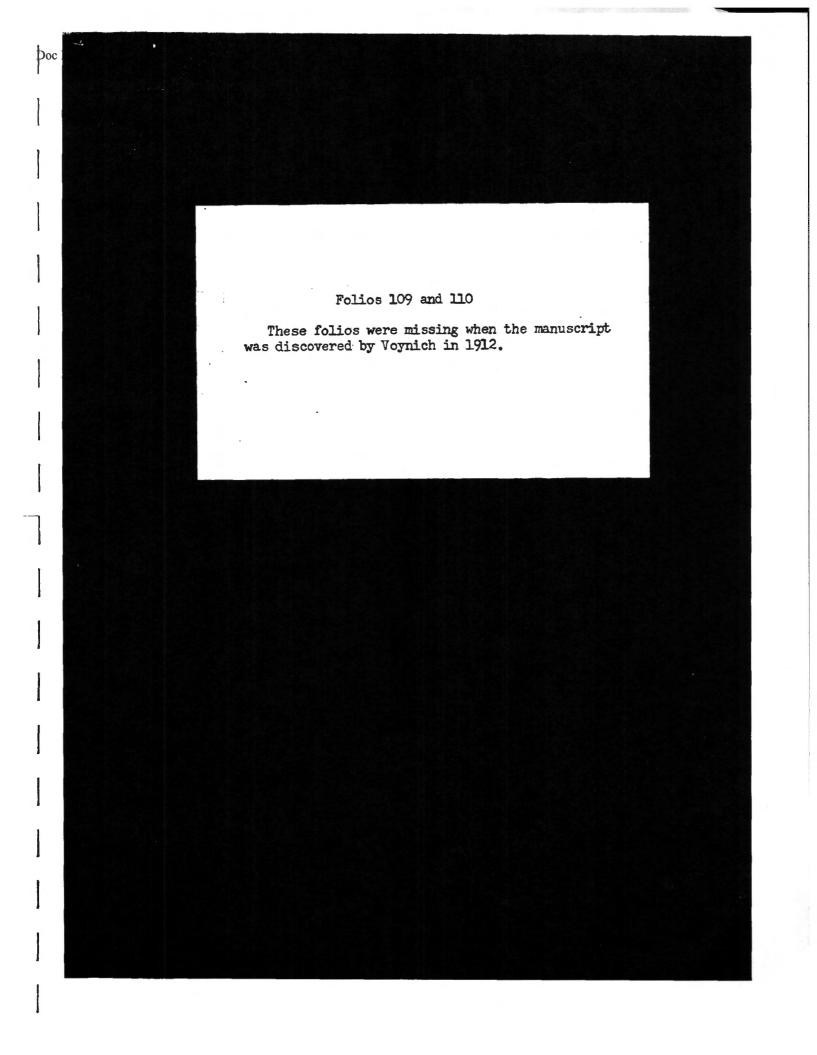
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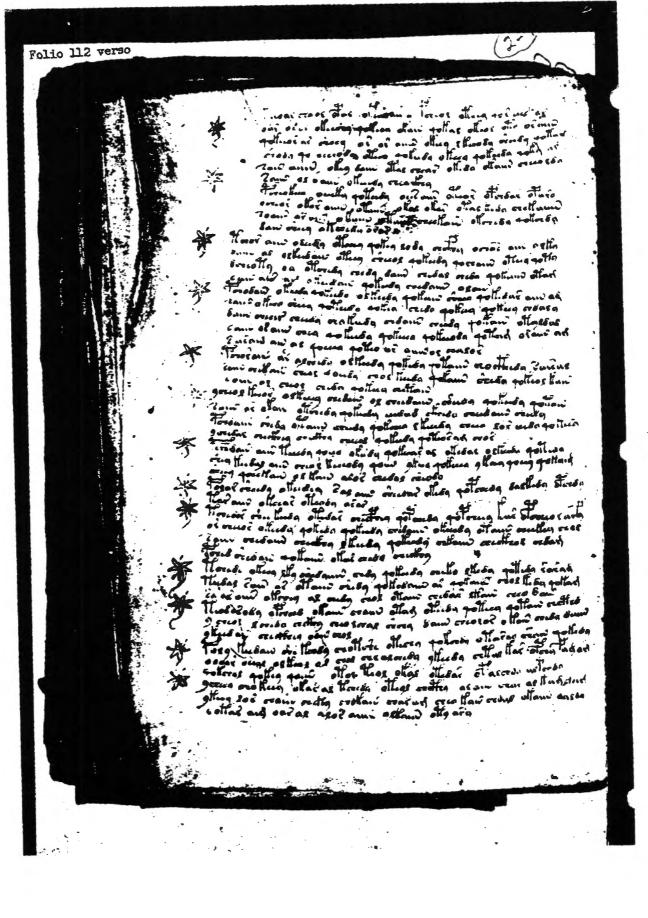
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