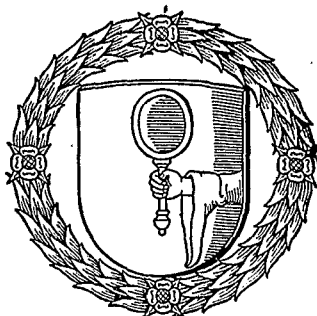


# SPECULUM

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# SPECULUM

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### LEX SALICA, I

BY SIMON STEIN

I

THE extent of German research in legal history and the predominant place acquired by the German school in this domain is well known; less known is the series of disastrous failures of the German school in publishing the sources of the ancient German law. Thus, all efforts made by the famous German institute, the *Monumenta Germaniae Historica*, to edit the *Lex Salica* were in vain. Of this important law we still do not have any edition which would meet modern scholarly requirements.

Shortly after the appearance of the first volume of the *Scriptores* in 1823-1824, G. H. Pertz and his friend F. Bluhme worked out the general plan of editing the Section of *Leges* of the M. G. H. *Lex Salica* was to occupy the place of honor and the distinguished jurist, L. W. A. Pernice, was entrusted with its edition. After two years of study, however, he abandoned the difficult task and Pertz himself took over. Pertz soon found editing the so-called 'Volksrechte' much more difficult than expected. Since Pertz was in a hurry to publish the first volumes of the *Leges*, it was decided to alter the general plan and to begin with the edition of the *Capitularia* on which the preparatory work was much more advanced. So, the first two volumes of *Leges* were published in 1835-1837 containing the *Capitula Regum Francorum* and the *Constitutiones Imperii*.

After completing this task, Pertz turned back to *Lex Salica*. In 1839, he published<sup>1</sup> a classification and a brief description of the principal manuscripts. He intended to follow it up soon with an edition of *Lex Salica* and the other laws. Unfortunately, Pertz' classification of these manuscripts did not meet with the approval hoped for. It was criticized sharply in France by J. M. Pardessus and in Germany by G. Waitz, the collaborator of Pertz. Pertz was not convinced by these criticisms and even prepared to disprove them. However, time went by and Pertz, overburdened with other work, found no opportunity to devote himself to the edition of *Lex Salica*. Not wanting to entrust anybody else with it, he decided to change again the general plan of the Section of *Leges*.

According to this new scheme, the 'Volksrechte' were to be edited alphabetically; thus *Lex Salica* yielded its place of honor to the *Lex Alamannorum* and

<sup>1</sup> *Archiv*, VII, 729 ff.

found itself almost at the end of the series. As could be expected, this new scheme was open to caustic criticism. However, this plan also found advocates, among them Harry Bresslau, the eminent historian of the M. G. H.<sup>2</sup> He repeated arguments used previously by Pertz and insisted that the alphabetical order had the advantage of putting related *leges* into close neighborhood, e.g., *Lex Alamannorum* and *Lex Baiuvariorum*; *Lex Salica* and *Lex Ripuaria*. Neither Pertz nor Bresslau seemed to realize that they could just as well have chosen ethnical affinities as an editorial principle of the various *leges* without regard to any alphabetical order. . . . That way they would have had no reason for moving *Lex Salica* almost to the end.

The new project was quickly carried out, so far as the first part was concerned. *Lex Alamannorum*, entrusted to Merkel in 1847, was published in 1850. It was followed by *Lex Baiuvariorum*, *Lex Burgundionum*, *Lex Frisionum*, and, ultimately, by the *Edictum Lombardorum* (1868). According to the alphabetical order the last one was to be followed by the *Leges Ripuaria* and *Salica*. But at this point that order was suppressed without any hesitation or comment and the editors went on to the *Leges Saxonum* and *Thuringorum* and to the *Edictum Theodorici*. With the last of these, Pertz's activities as chief editor of legal sources ended.

Georg Waitz was named (1875) his successor in the office of President of the M. G. H. As far as the *Leges* were concerned, he displayed his activities in two directions. First of all, a revision was started of the volumes published under Pertz's direction. A. Boretius was entrusted with the re-editing of the *Capitularia* and K. Lehmann with that of the *Lex Alamannorum*. At the same time, Waitz succeeded in securing the collaboration of one of the most distinguished German jurists, Rudolf Sohm, who agreed to prepare the *Leges Ripuaria* and *Salica* for publication by the M. G. H. He published the *Lex Ripuaria* but, while doing preparatory work on this *lex*, decided (1880) against editing the *Lex Salica*. In 1883 he again let himself be persuaded to prepare this edition. He asked for and obtained a five-year term but, shortly thereafter, he definitely declined the task.

After Waitz's death, the leading German jurists finally obtained the position with the M. G. H. which they had coveted for a long time. In 1887, for the first time, a jurist, H. Brunner, became the head of the Section of *Leges*. He had already played an important part in the management of the M. G. H. He had supported the candidacy of A. Boretius for the re-edition of the *Capitularia* and had arranged to have the task of re-editing *Lex Alamannorum* given to his disciple, K. Lehmann.

Brunner's program was very expansive. The re-editing of the *Capitularia* and of *Lex Alamannorum*, accomplished under his guidance, was followed by the re-edition of *Lex Burgundionum*. A new edition of *Lex Baiuvariorum* was confided to E. von Schwind, who finished this task in 1926. Thus, the re-editing of a very important part of the legal sources as published by Pertz had been accomplished or was being carried out.

<sup>2</sup> 'Geschichte der *Monumenta Germaniae Historica*,' *Neues Archiv*, XLII (1921), 312.

In 1899 Brunner's collaborator, K. Zeumer, became co-director of the Section of *Leges*. In 1884, still under Waitz, Zeumer had published the *Lex Visigothorum* for the M. G. H.

But it was not till 1902 that the principal problem, that of publishing *Lex Salica*, was attacked. On 1 October, 1902, a special collaborator, Mario Krammer, was commissioned to start the preparatory work for this publication under Zeumer's auspices.

From 1903 on, the minutes of the yearly meeting of the Central Management of the M. G. H. mention in encouraging terms the progress of work on the publication of *Lex Salica*.<sup>3</sup> Finally, the minutes of 1913 tell us the good news: 'Under the auspices of K. Zeumer, the permanent collaborator, M. Krammer, has promoted the printing of *Lex Salica* up to the eighth sheet.' The following year, E. Seckel replaced the late K. Zeumer as co-director of the Division of *Leges*. As far as *Lex Salica* was concerned, the minutes mentioned only that M. Krammer had nine additional sheets printed.

A slight change of tone can be observed in the minutes of 1915. 'M. Krammer has promoted the printing of *Lex Salica*. He published a study in the *Neues Archiv*, Volume 39, in which he tried to prove the advantages of text A over every other version.' For the first time the minutes swerve away from Krammer. Up to now he was spoken of in the affirmative. 'He pushed ahead with the work; he established a text'; etc. Now, for the first time, it was said that M. Krammer 'tried' to prove the advantages of text A; whether he succeeded was another question. This change of tone had its reasons. During the plenary session of 1915, Bruno Krusch, the distinguished publisher of the *Scriptores Rerum Merovingicarum*, spoke of the disastrous impression made by the sixteen printed sheets as well as Krammer's study in the *Neues Archiv*. He suggested stopping any further printing and scrapping the already printed sheets. This suggestion met with a categorical 'No' on Brunner's part and it was agreed to postpone the decision and to have Krusch publish his criticism in the *Neues Archiv*.

H. Brunner died on 11 August, 1915. The minutes of the M. G. H. session of 1916 mention first of all that E. Seckel succeeded Brunner as head and sole director of the Section of *Leges* and then go on to say: 'M. Krammer was busy with the last three versions (D, E, F) of *Lex Salica* and tried to reconstruct the original text, which, according to his hypothesis, can be worked out on the basis of his text A. He also published a study of "The Original Form and the Meaning of the Titles *De filitorto* and *De vestigio minando* of *Lex Salica*' in the *Zeitschrift der Savigny-Stiftung* (Vol. 36, *germ. Abt.*). As for Krusch's and von Schwerin's criticism, published in Volume XL of the *Neues Archiv*, questioning the soundness of the basis of the new edition of *Lex Salica*, that will be referred to the legal historians, the historians, and the philologists for their opinions, whereupon a commission is going to decide the fate of the edition.'

In carrying out this decision, sixteen scholars were asked to submit their expert opinion. Eleven of them accepted this task. Krammer was given an opportunity

<sup>3</sup> See Appendix for the minutes of the years 1903-1912 concerning this new endeavor.

to refute Krusch's criticism. His answer was published in Volume XLI of the *Neues Archiv*. In mid-April of 1917 the commission consisting of three members: M. Tangl, historian; E. Norden, philologist; E. Heymann, jurist, met to settle the question. They declared themselves unanimously against accepting Krammer's edition for publication in the *Monumenta Germaniae Historica*. Consequently, the plenary session unanimously decided to scrap Krammer's edition.

The last mention of Krammer's name in M. G. H.'s minutes is found in 1924; summed up, it says that on 1 April, 1924, Dr Mario Krammer, collaborator of the Section of *Leges* since 1902, retired because of reduction in personnel.

At the session where the fate of Krammer's edition was sealed, the administration of the M. G. H. unanimously decided to entrust Krusch with the edition of *Lex Salica*. That was in 1917. Krusch died on 29 June, 1940. In his polemics, Krusch had reproached Krammer for working twelve years to prepare his edition. More than twenty years passed and Krusch did not finish his preparatory work. Granted, Krusch was not idle during this time. In addition to his work on the new edition of Gregory of Tours, the first part of which was published before his death, he was busy with a critical study of *Lex Baiuvariorum*, the new edition of which was being prepared by Von Schwind. With the same fierceness shown in his crusade against Krammer, he launched his attack on Von Schwind. However, this time, he did not succeed. After all sorts of parleys and negotiations, it was decided to publish Von Schwind's edition with a reprobative preface by the Director of the Section of *Leges*. It was published in 1926.

In rounding out the distressing picture of M. G. H.'s activities in the field of legal sources, I want to add a few words on the new edition of Benedictus Levita. This edition was entrusted to Seckel in 1896; he died in 1924. P. Kehr, then President of the M. G. H., mentions in his obituary that the text of Benedictus Levita, as left by Seckel, needs only a slight retouching. He adds that a few difficulties arose in connection with the commentary but one could rest assured that the new edition of Benedictus Levita, Seckel's posthumous work, would not take long to be published. Juncker was entrusted with the final touches on this edition. He died in 1937. In the minutes of the 1938 session you can find the following remark: 'The work on the edition of Benedictus Levita has to be concluded as soon as possible. . . . We hope to find a collaborator worthy of this task.'

## II

Let us go back to *Lex Salica* and find out what the inherent difficulty is that defeats every effort to overcome it. It surely does not lie in the *lex* itself. There must have been something wrong with the approach to the problem, a grave mistake in the method. First of all, we must state a rather strange fact. A scholarly edition of the *Lex Salica* does not exist. But this rather important fact apparently does not prevent legal historians from treating all problems concerning *Lex Salica* as if such an edition were not lacking. *Lex Salica* is being quoted, described, taught, and praised in all languages in such a manner that you might think that a scholarly edition of it were a luxury to be dispensed with.

In 1867 R. Sohm, one of the leaders of the German school, dedicated a large

volume (*Der Prozess der Lex Salica*) to the procedure of *Lex Salica*. He had to have a proper text of *Lex Salica* to be able to comment on it, i.e., he should have had prepared a scholarly edition for his own use. He ought to have answered the following unavoidable questions: What is *Lex Salica*? When was it drafted? In what way was it distorted in the manuscript tradition? Where and up to what date was it valid? If he knew the answers to these questions, why then did he refuse to prepare an edition of this *lex* for the M. G. H.?

A few details of the Krammer affair present the circumstances of the scientific edition of the legal sources in an even stranger light.

I have mentioned that the management of the M. G. H., after hearing Krusch's criticism of Krammer's edition, decided to ask for the opinion of sixteen German scholars and eleven of them accepted. Of these eleven only two had the adequate knowledge necessary to undertake and, consequently, to criticize an edition.

The first of the reports starts with the following statement: 'I am lacking the necessary preliminary research to form an accurate and independent opinion on the basis of the (*Lex Salica*) edition. To fill in this gap, I would have to put aside the work which occupies me at present; and this would be impossible to do in the short time granted by the M. G. H.'

The logical conclusion to be expected after such a preamble would seem to me a refusal to submit the report requested. Since the distinguished author lacked the knowledge required for this task and he was not being given the time to fill in this gap, his refusal was to be expected. The fact is, however, that the author added to the preamble quoted above more than sixteen pages in which there is talk *de rebus omnibus et quibusdam aliis*. Thus, the author discusses the following questions: Did the ancient Franks live in a matriarchy or in a patriarchy? Was land property an object of trade? Who took part in the public assembly? etc., etc. This report was signed by Otto von Gierke.

This is the most explicit of all the reports. The others — some just a few lines — display amazing inanity. I quote that by P. Rehme: 'I just want to state that I tried to study a title of *Lex Salica* using Krammer's edition and consulting juridical special literature regarding this subject. This attempt was a complete failure. I felt like being in a labyrinth without any way out.' Rehme did not think it necessary to let his readers know the title in question and what difficulties he met with while studying it. He was sure that had he been given the original text, he would not have had any difficulty in understanding it; and if he should, juridical literature on *Lex Salica* would be of help to him. He evidently was an optimist.

Nevertheless, Rehme was not satisfied with mere criticisms; he also made a positive suggestion. According to him, the new edition of *Lex Salica* should represent an improvement over Hessels' edition. And since Gierke and some other experts were of the same opinion, I would like to dwell on this point.

Hessels' work on the whole is very well done except for a few inevitable blunders and the fact that the author does not quite realize the filiation of manuscripts. (I am going to quote some examples further on.) There is just one defect in his work, but that is a serious one. His book does not represent an edition of *Lex Salica*, but merely the reprinting of some of the manuscripts. Thus, as Krusch

says so well: 'It is a quite mechanical proceeding in which the reader is burdened with the solution of a problem which the editor could not solve himself.'<sup>4</sup> Besides, while unwilling to solve the problem himself, Hessels does not give his reader the means necessary to do so. To start with, he gives only a selection of manuscripts and the choice may be futile. He makes an even more serious mistake by giving only the text of each manuscript, and not the way in which the text is written, i.e., he gives neither the change of hand nor the change of ink nor any of the many details which sometimes are decisive. Moreover, he quotes the text of *Lex Salica* only and not the text of all the adjacent pieces which are sometimes the only basis for the solution of a question.

I want to make myself clearly understood: the issues of the problem of *Lex Salica* concern an important number of witnesses whose value depends on their intrinsic character. This character can never be established by the text of *Lex Salica* only. For instance, one of the texts of *Lex Salica* is written in a defective Latin. What are the conclusions? None that would be convincing. The corrupt Latin may be that of the original. It might correspond to the low standard of either the person compiling the material or the scribe. It might also be the conscientious effort of a forger trying to give the impression of ancient language so as not to leave any doubts about the authenticity of the document which he promotes, etc. . . . I cannot exhaust all possibilities; however, I want to bring up just this one question: How could even the most experienced judge pass judgment based on written anonymous testimony without being able to form an opinion on either the character of the witness or his trustworthiness. Moreover the judge does not even know whether the witness was at all present at the happenings about which he testifies.

The modern historical method is rather simple. It merely requires that before using a source you assure yourself of its purity. While reading most of the reports on Krammer's edition, I did not have the impression that their authors were conscious of the most elementary rules of the critical method. Otherwise, there would be no question of an edition similar to Hessels'.

To sum it all up, the M. G. H., the most competent institution on problems of editing, asks non-competent scholars for their opinion on an edition in progress and, armed with their approval, decides to scrap the almost finished edition. That is not a very jolly spectacle.

I would not want to defend Krammer's edition. To begin with, I have not read it. While in Berlin, I tried in vain to obtain it. Neither the author nor the Berlin Library had it. It really seems to have been destroyed down to the last copy. All I know of this edition are the above quoted reports and Krammer's essays on *Lex Salica*. It seems to me that Krammer's principal errors are the same as those of his critics and that it was wrong to make a scapegoat of him. By sacrificing him, one hoped to atone for the sins of the Section of *Leges*. That was a mistake.

But let us proceed methodically and see what the problem is. The fundamental classification of all the manuscripts of *Lex Salica* was made by Pertz, who dis-

<sup>4</sup> *Neues Archiv*, XL, 502.

tinguished between the Merovingian and the Carolingian texts of that *Lex*. Furthermore, he divided all manuscripts by dividing the Merovingian text in three groups:

a. Original text

*Paris. lat. 4403 B.*

*Paris lat. 18237*

b. The shortened text

*Paris. lat. 9653*

*Guelferbytanus inter Weissenb. 97*

*Paris. 4404*

*Monac. 4115*

c. The transformed text (in 99 or 100 titles)

*Sangallensis 731*

*Montispezzulanus H. 136* } with Malberg glosses

*Paris 4627*

*Paris 4409*

*Sangallensis 729*

*Paris 4629*

*Middlehill. 566* } without glosses

In group *b*, I omitted the *Ms. Lugd. Bat. Voss. 119* which figured in Pertz' first classification, but which he subsequently transferred to the group of *lex emendata*. Likewise, in group *c*, I omitted Herold's edition since it is not a manuscript and even if Herold had at his disposal a manuscript now lost, it would not be possible to reconstruct it on the basis of his edition.

Pardessus' as well as Waitz's criticisms were directed against groups *a* and *b*. According to their opinion, the most ancient text is transmitted by manuscripts of group *b*, mainly by *Ms. Paris. 4404* of this group. This text was amplified (not shortened, as Pertz thought) in group *a*.

In his studies Krammer defended two different theories. At first, he thought that we have to go back to Pertz' system. Later, he changed his mind and stated that the most ancient text was represented by group *c*. Thus, the first attempt to restore the text, as mentioned in the report of 1904, was made on a quite different basis than that quoted in the report of 1907. I do not want to blame Krammer for this change of mind: the question is complicated enough to justify such change. But in doing so, Krammer made an unpardonable mistake; neither to his reader nor apparently to himself did he explain how the mistake was made which was, evidently, caused by a defective method. Thus, in his first essay,<sup>5</sup> after a short history of classifications of manuscripts of *Lex Salica*, Krammer tries to prove with the help of a large number of collations that Pertz alone was right and that the most ancient text was preserved by the two manuscripts, *Paris 18237* and *4403 B*. He concludes the first part of his critical study by remarking that the

<sup>5</sup> 'Kritische Untersuchungen zur *Lex Salica* i. Teil,' *Neues Archiv*, xxx.

'second part of this study will treat pieces of *Lex Salica* that might be even more instructive and that would support the above stated results with even greater certainty.' However, the second part of the study never appeared. After two years, in 1907, Krammer informed the readers of the *Neues Archiv* (Vol. xxxii): 'while I was preparing the second part of the study, the foundation of that work proved itself to be less solid than expected. Nevertheless, it was impossible to go back to the theory accepted by almost all before my study. The work which, I hope, will be published soon, might perhaps plead a completely new cause.' To me this announcement is rather mysterious. Since only one of the three groups could represent the most ancient text and since Krammer had eliminated two of them, it was evident that he would have to choose the third. As a matter of fact, seven years later, the first part of 'Research on *Lex Salica*' was published.<sup>6</sup> Like the first essay, he begins it with a short history of the classification of manuscripts. He concludes it by stating that he previously pleaded for group B,<sup>7</sup> but that 'subsequent investigations led him to acknowledge group C as representing the most ancient form of the *lex*.' After this historiographical introduction the author tries to prove this new thesis by a large number of collations. At the same time, Krammer attempts to find the original text of *Lex Salica*. At the end of the first part of his 'research' he expresses the hope to be able to complete his work by analyzing the title of *Lex Salica* that were not analyzed up to then and to restore in all its clarity the old Merovingian text as written under Clovis and his sons.

In his two studies Krammer does not seem to realize that the collation mentioned incessantly in the above quoted minutes, while being an important tool of the modern historical method, is far from being its only one. Furthermore, Krammer expects services of the collation which it cannot render. This point was recently stressed by Dom Quentin with perfect clarity. The collations can render the affinity of two or several manuscripts plausible, but they could never indicate in which direction the evolution proceeded. Even in the exceptional case of two texts whose affinity is such that one can be assumed to be the direct copy of the other, we cannot definitely prove through collations alone which one is the original and which the copy. The gaps of the one might have been filled by the other, just as well as they might be caused by the scribe's negligence. The original may be on either a higher or a lower literary level. Conclusions based on collations are always reversible. Krammer had every reason to convince himself of this fact when studying *Lex Salica*. According to Pertz, the text of group *a* had been shortened in the text of class *b*,<sup>8</sup> while, according to Pardessus and Waitz, the text of class *b* had been amplified in that of class *a*. And when Krammer changed his mind and chose class *c* as the most ancient text, was not that the best proof that all conclusions based only on collations are reversible?

It is unfortunate that one finds nothing but collations and vague hypothesis in Krammer's studies, and, curiously enough, they are based on Hessels' edition. No new observations, not a single new fact concerning any manuscripts, can be

<sup>6</sup> 'Forschungen zur *Lex Salica*,' *Neues Archiv*, xxxix (1914).

<sup>7</sup> This is group *a* of the Pertz classification.

found in his studies. Apparently, Krammer did not realize that the first duty of an editor of a mediaeval source is that of transmitting to the reader the characteristic features of the manuscript tradition. The above mentioned M. G. H. reports are much more loquacious than one might think. During fifteen years of work, Kramer spent only the first four years in direct contact with the manuscripts, the remaining eleven years he devoted to constructions based on . . . Hessels!

Like his predecessors, Krammer did not add anything new to Pertz' classification. He did not find anything remarkable in the fact that only a dozen or so manuscripts were being studied while the *Lex Salica* has been transmitted to us by some hundred. The minutes of 1904, mentioning Krammer for the second time, state that after having collated six manuscripts he finished his study of the most important manuscripts. However, the importance of a manuscript reveals itself at the end of a work and not at the beginning.

Let us add that when Krammer wants to give us a new interpretation based on some features peculiar to a single manuscript, he only proves his complete inexperience. Thus, he refers to the fact pointed out by Pardessus (preface, p. L) that *Lex Salica* of *Ms. Vat. reg. Chr. 846* ends as follows: *Anno feliciter undecimo regni domini Caroli gloriosissimi regis, mense martio*, and asserts that these words are a reliable *terminus ante quem* for all the texts of *Lex Salica*. These are his arguments: *Ms. Vat. reg. Chr. 846* contains version F, which is more recent than the *Emendata* and influenced by it. The *Emendata*, in its turn, is a little more recent than versions A, B, C, D which merely serve as its patterns. The date of *Ms. Vat. reg. Chr. 846* is corroborated, Krammer thinks, by the fact that the *Emendata* is dated as of 768.

I repeat, the peculiarity of *Ms. Vat. reg. Chr. 846* was not pointed out by Krammer. He merely quoted Pardessus without naming the latter. Hessels also did it, but he referred to Pardessus (p. xvii). However, Krammer interprets this fact diametrically opposing Pardessus. According to the latter, the words in question prove that *Ms. 846* had been copied at a time when Charlemagne, 'according to general opinion . . . had not had the text of *Lex Salica* revised as yet.' However, on page 265, Pardessus dates the *Emendata* as of 768.

Krammer never saw the manuscript in question. He trusted Pardessus completely, but Pardessus made a blunder — he shortened the quotation. In the manuscript the word 'martio' is followed by the following words 'facto capitulare qualiter congregatis in unum. . . . ' Thus, the sentence which Pardessus mistook for the date of the manuscript in question and in which Krammer saw a reliable *terminus ante quem* for all texts of *Lex Salica*, this sentence has no relation whatsoever either to the date of the manuscript or to *Lex Salica*. It is the preamble of *Cap. 20<sup>s</sup>* followed by the *Capitulare* itself. This was pointed out by L. Bethmann in the *Archiv*, XII (1874), 309!

During his lengthy studies of *Lex Salica*, Krammer did not enlarge our knowledge of the manuscript tradition of this *Lex*, nor did he add anything new either

<sup>s</sup> The *Capitularia* are quoted according to their numbers as given in the second edition of the *Capitularia Regum Francorum in M.G.H.*, edited by A. Boretius, 1883.

to the dating or to the characteristics of any of the manuscripts. The best proof of this fact is his answer to Krusch' criticism (*Neues Archiv*, xli). As a matter of fact, in this duel, Krammer was in a much more favorable position than Krusch. He ought to have known the manuscript tradition better than anyone else. Krusch, on the other hand, had never worked with legal manuscripts; all he knew about them came from defective editions. Now, the most outstanding feature of Krammer's answer is the fact that he does not refer to a single manuscript, as if his own personal acquaintance with manuscripts did not exist. Only general questions attract his attention. While reading his reply, you can better understand the tenor of Gierke's criticism. In Krammer's reply there is no mention of matriarchy and patriarchy, but he does talk about the Latin used by Clovis in his legislative inspirations, as well as about other questions as interesting as they are insoluble. And as a fundamental principle of the new edition Krammer promotes the criterion of the influence other German *leges* had on the different versions of *Lex Salica*. This tops it all. (Do we have to mention in criticizing this thesis that it lies in the nature of things to establish a text before proceeding to study the influence of other German *leges* which are neither more accurately dated nor edited?) Krammer refers to Brunner when stating this very special theory. And he is right. The honor of having completely reversed the natural order of things in the realm of legal sources unquestionably goes to Brunner. Krammer was merely an executor.

Despite these many deficiencies in Krammer's work, I would not want to pass over it without attempting to find something positive and useful in his work. So many problems arise in connection with *Lex Salica* and so many difficulties have to be solved that one should be grateful for each new idea. Let us ask then, for instance, where do so many versions come from? Are they the result of different official publications or are they due to negligence of the scribes? Even this cardinal question is not accurately answered by Krammer. Apparently, he thought them to be official versions. As for the dates of the different versions, Krammer finds a new and, it would seem, rather odd answer. Based on *Ms. Vat. reg. Chr. 846*, Krammer concludes that the last version of *Lex Salica*, version F, precedes the year 779. The most ancient one, preserved until today, is that written under Pepin from 762 to 764. Thus, within a period of fifteen years, one official version followed the other. But what for? And there is another question. Did each new version replace the preceding one? How is it then, that the manuscripts of the ninth century have preserved all the versions, even those that were no longer valid? In vain do we look for an answer to these questions in Krammer's work. They do not seem to interest him. He is primarily interested in the versions of *Lex Salica* under Clovis or his sons. Let us re-read the minutes of 1909. They are a little vague but they give the aspirations and the aim of the new edition. All interest is directed toward the 'original text', and the editor reluctantly admits that he has to limit himself to the text of Clovis or his sons. The simple idea that Charlemagne's text is just as interesting did not enter the editor's mind. There is, unfortunately, no rational means which would enable us to restore this text of Clovis even if the division of texts into 'Austrasian' and 'Neustrian' texts were better founded than Krammer did it.

In finishing this chapter, I would like to quote G. Seeliger: 'The reconstruction of the original lex (Ur-Lex) cannot be the principal aim of the editor since the 'where,' 'when,' and 'how,' could not definitely be answered.'<sup>9</sup>

### III

To round out the up-to-date picture of work done on *Lex Salica* editions, I want to dwell on Krusch's theories. Krusch was a great scholar, and I regret very much that I shall not have his opinion on my theories concerning this *lex*. Not only did he have an overwhelming knowledge of Merovingian sources, but he also had the most precious quality a scholar may be endowed with — courage. He did not compromise. What he considered to be right, he proclaimed without any ambiguity or qualification. An example of this courage is the fact that Krusch, contrary to the entire German school, did not hesitate to join Fustel de Coulanges. The theories of the German legal school on the ancient *Volksrecht* he called 'fables' and he stated plainly: 'Fustel de Coulanges (*Histoire des institutions politiques*, 1888, II, 109 ff.) is entitled to great praise for having protested against these heresies (*Irrlehren*). In Germany everybody joined Sohm with flying colors and at this point the French scholar — in sharp contradiction to Sohm — proclaimed that the legislative power did not belong to the people but to the king.'<sup>10</sup> It is not often that you hear praise of Fustel in Germany. However, I regret to state that Krusch's work, taken retrospectively as a whole, is less accomplished than might be expected. There is something unfinished in his work, just as there was something unbalanced and embittered in his character, something that kept his abilities from coming to full power. Heymann reveals, in his obituary in Volume IV (1941) of the *Deutsches Archiv*, that Krusch had had a difficult youth and that Waitz kept him from a university career to which he aspired by compelling him to join the Archives. Be it as it may, this feature was Krusch's greatest fault. An ardent polemist, he did not criticize his adversary, he annihilated him. He did not argue, he led crusades. Nevertheless, Krusch was a great scholar and it is our duty to find out what he thought about *Lex Salica*.

Unfortunately, his studies on this *lex* do not add any new laurels to his arduous and prolific life. The explanation is easy. The criticism of Krammer's theories was an incidental matter for Krusch. He tells us himself in *Neues Archiv* (XL, 499) how he prepared himself for the plenary session of the Board of Directors of the M. G. H. in 1915 by unwillingly relinquishing his customary work routine for two days to devote himself to the study of the printed sheets of *Lex Salica* and Krammer's research work. Obviously, two days were insufficient for him to form an independent opinion and to analyze the manuscripts. One can readily understand why Krusch at first refused to have his report on Krammer's edition printed, but later he let himself be persuaded. Two years later, he was offered the opportunity of preparing a new edition of *Lex Salica*. He refused, but again he let himself be persuaded. He was wrong both times. In criticizing Krammer he found himself at a dead end, but still he persevered. Here are the proofs.

<sup>9</sup> *Neues Archiv*, XLII, 411.

<sup>10</sup> *Neue Forschungen über die drei oberdeutschen Leges*, 1927.

First of all, in criticizing Krammer, Krusch extolled the work of Waitz. According to him, Waitz had set indestructible foundations for all studies of *Lex Salica* and all you had to do was to follow in his tracks. This invitation seemed rather strange to me. It seemed improbable to me that, in 1846, Waitz could have established solid foundations for the studies of *Lex Salica*. Nevertheless, I followed Krusch's advice. I did it in order to understand Krusch rather than to enlarge my knowledge of *Lex Salica*. Here are the results of my investigation.

Waitz sees no difficulties in understanding and dating *Lex Salica*: it is wrong to consider it a Merovingian *lex*; it is much more ancient. It reveals German institutions as they were after the disappearance of free ancient institutions, as described by Tacitus, but before the foundation of German kingdoms on Roman territory. Waitz does not hesitate in establishing a rather simple rule which gives us an infallible means of elucidating the most knotty problems of the manuscript tradition. According to this rule, the evolution of the ancient legislation expressed itself by amplification of the law. Thus, to find the most ancient text you have to turn to the shortest one. In following through this thorough method, Waitz traces the evolution of the civilization of the German tribes. He calls the attention of his readers to the fact that the shortest text—which to him is the oldest—only mentions the goshawk and the goose, while the others already mention the sparrow hawk, the rooster, the hen, the crane, the swan, the duck, and the pigeon. He makes analogous observations regarding the mentioning or non-mentioning of fruit trees, etc. He is so sure of his method that he does not hesitate in formulating his thoughts in a general manner. 'One could never find a reason for omitting all these names, while their subsequent addition is easy to explain.' Waitz did not doubt that the different versions could only be explained by more primitive needs and lesser comfort as revealed in the shortest and, therefore, most ancient text. What would he say if, absentmindedly, some scribe had mentioned the rooster without mentioning the hen?

At the end of his life, Waitz stated with satisfaction that nothing essential had been added to the work on *Lex Salica* which he published in his youth.<sup>11</sup> He was almost right, but he did not realize that it was not a question of adding something but of cutting out.

There is decidedly nothing worthwhile to look for, today, in Waitz's work. This is worst eighteenth century. And Krusch himself was far from approving all his theories *en bloc*. He did not share Waitz's opinion that *Lex Salica* was pre-Merovingian; neither did he want to admit that it had its origin in Toxandria. But he agreed with the principal points of classification of manuscripts as given by Waitz. Let us look into this subject for a moment.

According to Waitz, the most ancient text of *Lex Salica* is transmitted to us by *Ms. Paris. lat. 4404*. It is the most ancient one, not only because it is the shortest one and does not mention a series of birds as other manuscripts do, but also—and here Waitz takes up the argument of Pardessus—because it is the only manuscript where no trace of Christianity can be found.

<sup>11</sup> Krusch in *Neues Archiv*, XL, 501.

This is the best proof of the complete misunderstanding of the whole manuscript tradition of *Lex Salica*. Waitz, like Pardessus, did not realize the rather simple fact that *Lex Salica* has been transmitted to us only by the Carolingian church and through no other sources. What aim could the church pursue by propagating a pagan text?

How was it possible that Waitz did not notice the fact that *Lex Salica* in *Ms. 4404* has the great prologue in which Christian virtues of the *Gens Francorum* are extolled 'et munus (immunis) ab heresa'; and where, in comparing the Franks to the Romans and by straining the historical perspective a little, it is stated that 'sanctorum martyrum corpora quae romani igne cremaverunt vel ferro trunca-verunt vel besteis lacerando proiecerunt Franci super eos aurum et lapides preciosos superornaverunt' (f. 107).

No, we definitely cannot find anything worthwhile in Waitz's work. His book on *Lex Salica* is a complete and naive failure. But why did Krusch not notice it? We shall understand it better if we think of the ideas that guided him in his studies of *Lex Salica*.

At the beginning of his criticism of Krammer, Krusch reveals his ideas *expressis verbis*: 'I have no right to write a panegyric of this *lex* which surpasses all others in its importance: a worthy monument of German legal talent erected on the eve of the foundation of the *Frankish* Empire, a bulwark against Roman law on Gallic soil, the basis of the evolution of law on both sides of the Rhine.'<sup>12</sup> And a few lines before Krusch complains that in Krammer's edition the text which up to then had been accepted as the most ancient one 'which is *undoubtedly Merovingian*, had been printed in small letters.' This most ancient text is the one of *Ms. Paris lat. 4404*. What a pity that Krusch did not see this valuable manuscript when writing his panegyric. He would then have easily established the fact that this manuscript volume — as is mentioned in its preface — is dedicated to the Roman law, to the *Breviarium Alarici*, in the first place; that the *Breviarium Alarici* occupies the first 178 folios and that *Lex Salica* is second with its 28 folios. A rather odd bulwark against the Roman law to which it cedes the place of honor. Why did Krusch not consider the fact that if the church really wanted to suppress the Roman law, we would never have had any knowledge of it; in any case not of *Codex Theodosianus* which, according to its illustrious editor Th. Mommsen, *Gallia* (i.e. the Church of Gaul) *nobis servavit*.

How could he forget that the church itself lived by the Roman law ('... secundum legem Romanam, quam ecclesia vivit...' *Lex Rib.*, §LVIII) and that the church was not interested in constructing a bulwark against this law?

The situation seems to be quite clear. Krusch, crusading against Krammer, had not had enough time to form an independent opinion. He noticed the insufficiency of new ideas in Krammer's work; however, he did not realize that Krammer's worst misconceptions were not those that distinguished him from his predecessors, but those that he shared with them, and with which Krusch associated himself, as well.

<sup>12</sup> *Neues Archiv*, XL, 501.

As a matter of fact, the question which of the manuscripts of the ninth century transmits the Merovingian text is of secondary importance compared to the completely insoluble problem of reconstructing a text of the sixth century relying on a text of a ninth century manuscript; the more so as we cannot be sure that the former ever existed, according to Seeliger.

Krusch thought he had an infallible means to reach this aim and that this was his personal contribution to the study of the *Leges*. This means is the philological method which all previous editors had neglected. P. Kehr in summarizing the controversy raised by Krusch in the research on *Leges* stresses the fact that Krusch was the first to advance the philological method. 'This method is conceived and based on the most profound knowledge of all Merovingian literary production and it is a strong and solid foundation on which to judge the text and its wordings.'<sup>13</sup>

Unfortunately, P. Kehr is exaggerating. The Merovingian literary output is not excessively large, the more so as, thanks to Krusch, a series of *Vitae Sanctorum* have been eliminated which were previously taken to be Merovingian. More serious, however, is the fact that the manuscripts which transmit this literary output, almost in its entirety, are post-Merovingian. Thus, the reconstruction of the 'Merovingian language' meets with great difficulties since there is no sure method which would permit an exact reconstruction of an ancient text based on a much later manuscript. The difficulty of the problem shows also in the fact that the fundamental research work on the 'Merovingian language' — Bonnet's book *Le Latin de Grégoire de Tours* — which was acclaimed by Krusch when it appeared,<sup>14</sup> was sharply criticized by him in his last years of life. In the minutes of the M. G. H. of 1931 (*Neues Archiv*, L, 1935) we find a notice that, during his work to establish the text of the new edition of Gregory of Tours, 'Bonnet's research proved more of an obstacle (*belastende Hemmung*) rather than of help'.

The lack of contemporary manuscripts is not the only difficulty in the study of the Merovingian language; there is another one: We cannot even be sure whether the conception of a 'Merovingian language' is well formulated. As a matter of fact, the 'Merovingian language' is characterized by a purely negative principle: it is a *corrupt* Latin which was used in the Merovingian period. Was it one language or several? In other words, I wonder whether any uniformity could be found in this defective Latin. To get more information on this subject I turned to the excellent edition of Lauer and Samaran — *Les diplômes originaux des mérovingiens* (Paris 1908). I said to myself that if there really were a Merovingian language, the royal chancellery would be the first place in which to find definite traces of it.

Unfortunately, no uniformity can be found in the diplomas, not even in the most often used expressions. Here are two examples: the expression *in corpore requiescit* can be found in this form on table 15, line 13, and table 17, line 15. Other wordings are as follows:

<sup>13</sup> *Neues Archiv*, XLVI, 172.

<sup>14</sup> *Neues Archiv*, XVI (1891), 434: 'Mit trefflichen philologischen Kenntnissen, hat Bonnet durch intensiven Fleiss über die Sprache Gregors ein Werk von so minutiöser Gründlichkeit geschaffen, wie es nur für wenige von den Klassischen Autoren existiert.'

<i>in corpore requiescit</i>	Tb. 12, 2; Tb. 18, 4; Tb. 25, 4
<i>in corpore requiescit</i>	Tb. 20, 7; Tb. 24, 5
<i>in corpore requiescit</i>	Tb. 21, 2; Tb. 34, 4; Tb. 37, 3
<i>in corpore requiescit</i>	Tb. 38, 8

Another example: the word *nuncopanti* is to be found in the following different spellings:

<i>monasteriolo noncobante</i> (acc.)	Tb. 29, 3
<i>loca noncobantis</i> (acc.)	Tb. 19, 3
<i>villa nuncopanti</i> (acc.)	Tb. 24, 3
<i>villa nuncopanti</i> (abl.)	Tb. 24, 4
<i>villa nuncopanti</i> (acc.)	Tb. 20, 8
<i>locello noncupanti</i> (acc.)	Tb. 23, 12
<i>loco noncupanti</i> (abl.)	Tb. 25, 8

The greatest diversion reigns and — a most peculiar fact — the same diploma uses two different forms in two adjacent lines (Table 24, line 3 and 4).

One might say that the predilection to the forms ending in 'o' was a characteristic of the Merovingian language. As a matter of fact, many examples of this predilection can be found: *Ad basileca ipsius sancti Dioninsio* (Tb. 2, 4); *ex successione geneturi suo Chrodoleno* (Tb. 4, 3); *ad ipso maso* (Tb. 8, 4); *per nostro precepto* (Tb. 12, 9); *adversus apostolico viro domno Berachario episcopo* (Tb. 13, 2); *inter ipso Amalgano vel genetore suo Gaeltramno* (Tb. 16, 7-8); *ad ipso Latiniaco* (Tb. 17, 8); etc. But along with these forms you can also find perfectly correct ones.

Even if I were wrong in my preceding criticism and if the 'Merovingian language' had actually existed, one point is certain. The chronological limits of this language could never be traced clearly, i.e., it could never be exactly distinguished from the Carolingian language. By no means do I want to underestimate the importance of the 'Carolingian renaissance,' although it sometimes seems to me that neither its date nor its influence are exactly determined. Thus, I quite agree with Krammer when — contrary to Krusch — he does not want to admit the existence of a 'Carolingian renaissance' under Pepin<sup>15</sup>. Another aspect of this question, however, is more important. The Carolingian renaissance had not been decreed by a law to which all scribes and compilers of the ninth century submitted. It revealed itself in the elite of the Carolingian society. On the lower level, the faulty Latin — the Merovingian Latin — continued to reign. Therefore, I think it very difficult, if not impossible, to establish clearly the difference between a Merovingian and a Carolingian text if both of them are transmitted by Carolingian manuscripts.

We cannot agree with Krusch when he<sup>16</sup> compares the two following texts:

*Paris lat. 4627*  
*ut ista omnia que*  
*constituerunt starent*

*Guelferb. i. W. 97*  
*ut sta omnia sicut anteriore*  
*constructa starent*

<sup>15</sup> *Neues Archiv*, xli, 106.

<sup>16</sup> *Neues Archiv*, xl, 527.

and categorically states that the first one represents a Carolingian redraft while the second one is a Merovingian version. First of all, we must state that Krusch's interpretation is not the only possible one. There is no decisive reason for discarding a diametrically opposed interpretation: the text of *Ms. Paris 4627* might just as well be closer to the original and the text of *Guelferbitanus* might be its deterioration. A purely philological criterion cannot solve this question; we would have to look for others. And to this end I would like to inform my reader that in *Ms. Guelf. i. W. 97* I found a text entitled '*Totas maib*' which reads as follows (I am following Hessels' text, p. XLIV, adding some corrections according to a photo-reproduction printed in the *Scriptores Rerum Merovingicarum*, VII, 470):

In nomine dei patris omnipotentis, Sit placuit voluntas laidobranno et adono, ut pactum salicum de quod titulum non abit gratenter suplicibus aput gracia fredono una (?) cum uxore sua et obtimatis eorum, in ipsum pactum titulum unum cum deo adiuturio pertractare debirent, ut si quis homo aut in casa aut foris casa plena botilia abere potuerint tam de eorum quam de aliorum in cuppa non mittant ne gutta; se ullus hoc facire presumerit mal leodardi, sol XV conponat, et ipsa cuppa frangant la tota ad illo botiliario frangant lo cabo at scanciono tollant lis potionis; sic convenit observare aput satubo bibant et intus suppas faciant cum senior bibit duas vicis sui vassalli; la tercia bonum est ego qui scripsi mei nomen non hic scripsi culpabilis iudicetur.

I am sorry to have abused my reader's patience with such a trifle. I think, however, that it throws more light on the real character of *Guelferb. i. W. 97* than do all reflections on the Merovingian Latin.

In his comments on this appendix of *Lex Salica*, a great scholar did not hesitate to write that this 'titulus iocosus' had been composed 'ab hilari quodam potatore partim sermone rustico.'<sup>17</sup> The name of this great scholar is Bruno Krusch. Why did he not realize that the *sermo rusticus* of the compiler of *Ms. Guelferb.* was a much more plausible reason for the defectiveness of the Latin language in *Lex Salica* than the desire to conserve the Merovingian Latin in its original form? I would not know how to explain it unless he had not as yet seen the manuscript in question when writing his study on the *Lex Salica*. (His study appeared in the *Neues Archiv* in 1916, while volume VII of *SS. R. M.* is of 1920.) There is no doubt whatsoever that *Lex Salica* of *Ms. Guelf. i. W. 97* was written by the same *hilaris potator* as the *titulus iocosus*. The *explicit* of *Lex Salica* is on F 37<sup>r</sup>. The *titulus iocosus* on F. 37<sup>v</sup>. No change of hand can be recorded. Between the two texts is the Chronology of Frankish kings which follows *Lex Salica* in many manuscripts.

It seems to me that a general rule can easily be extricated from the preceding reflections. The will to conserve the original language in all its details can be attributed only to the scribe who is conscious of the difference between correct Latin and corrupt Latin. In other words, the scribe in question has to have a rather high literary standing. Based on this statement I could formulate the following general rule: 'Merovingian Latin' in a Carolingian manuscript can only be ascertained if this Latin stands out against a background of correct Latin.

Let us turn now to *Ms. Paris lat. 4404* and let us first of all look at the preface to get acquainted with the quality of its compiler's Latin. The preface is, how-

<sup>17</sup> *Scriptores Rerum Merovingicarum*, VII, 471.

ever, full of 'Merovingian' imperfections. I am going to point out the most striking ones only: *Quod nunc a multis constat codix istius dictatus . . . corpore theodosiani. . . Theodosiani iunioris Paulus sententiarum libelli quinque. . . Post modum in hoc volumine continet scriptus pactus salicae libellus unus . . . domino nostro Karolo imperatore edictus eius extremis scribitur.*

You can easily see — the Latin of the compiler himself is such that one has no desire to look for additional explanations for the flaws in the Latin language of *Lex Salica*. I would like to mention a rather odd mistake that can be found in the latter. In Title XLVII (*De filitortis*) the learned compiler succeeded in transforming the Loire into a verb. Thus, instead of writing 'si intra ligerem aut carbonariam ambo manent,' he offered the following wording: 'sic eligere aut carbonariam ambo maneant.'

However, decisive proof of the inferior literary level of the compiler of *Ms. 4404* can be found in the *Breviarium Alarici* which, as I said before, occupies its greater part. At the very beginning of this *breviarium* you find the following text in large capital letters: *Incipit auctoritas Alarici rege*, and in this *auctoritas*: *quod . . . habebantur; omnes . . . obscuritas; formola; . . . ut universus ordinationis nostrae . . . disciplina teneat*, etc.

I would not want to bore my reader by enumerating all the mistakes of *Ms. 4404*. They swarm all over. I want to quote only some of the most outstanding ones:

F. 4	<i>animi</i> instead of <i>nemini</i>
F. 11	<i>crimine</i> instead of <i>feminae</i>
F. 12	<i>De senatus consolato</i> and <i>inconsolato patre</i> instead of <i>inconsulto</i>
F. 13	<i>sipplit</i> instead of <i>supplicis</i> <i>caveat</i> instead of <i>causa</i>

Besides, I would not be able to say which peculiarities of the Merovingian Latin could not be found in the *Breviarium Alarici* of *Ms. 4404*.

Under these circumstances, I do not understand what Krusch meant by stating that few blunders are to be found in this manuscript.<sup>18</sup> I would rather join Knust, who, in a letter to Pertz (Paris, 30. 6. 1839) printed in *Archiv*, VIII, 113, complained of 'grammatical nonsense' as well as of the mix-up of the *es* and *is*, *quae* and *que*, *am*, *ã*, and *a*, in this manuscript.

I hope that the arguments above are convincing enough for my reader not to look for the *Lex Salica* text which is closest to the original in the *Mss. Guelf. i. W. 97* and *Paris 4404*. The philological criterion cannot be applied to these two manuscripts, because the language of the compilers is far from perfect.

In the preceding pages, I have carefully analyzed Krusch's thesis. Unfortunately, neither the attempt to galvanize the theories of Waitz nor his personal contribution to the solution of the problem — the philological method — give us any hope that the new edition prepared by him might be any better than the

<sup>18</sup> 'Neue Forschungen über die drei ober-deutschen Leges,' *Abhandlungen d. Gesellschaft der Wissenschaften zu Göttingen*, N. F. Bd. 20 (1927), p. 183.

preceding ones. The minutes of the M. G. H. of 1937 mention that the manuscript of this new edition has been finished.<sup>19</sup>

I would rather pass over the series of studies on *Lex Salica* which Krusch published in the last years of his life. There is something morbid and maniacal in these studies in which he forced himself to prove that the *Lex Salica* dates from 507 and that its author is Clovis. I feel, however, obligated to give my reader at least a few examples. I shall limit myself to the study printed in Volume 31 of the *Historische Vierteljahresschrift*. In this study, Krusch boasts of having exactly dated *Lex Salica*. His *argumentum maximum* is the following expression in *Lex Salica* of Ms. 4404 which is repeated verbatim in Clovis' letter of 507. The concordance is

*Lex Salica* (Ms. 4404) II, 6:

Quo numero usque ad duos porcos *simili*  
*conditione* convenit *observare*

*Epist. Clothovechi* a. 507 (Cap. 1):

*Simili conditione* et de clericis *praeceptum*  
*est observare*

Based on this concordance which is really not very important, Krusch arrives at this rather unexpected conclusion: '*Lex Salica* was issued in 507, neither earlier nor later' (p. 427).

In the same essay, Krusch presents a panegyric on Clovis. 'The king was a cunning person — much more so than his modern critics are. His monetary system was a great work, one more reason to call him "Great." He was a financial genius and more than that. His humanitarian feelings towards human beings and animals deserve admiration. If somebody else's cattle is found in your fields, you have no right to destroy it. Thus decrees a king of barbarians!'

In reading these deductions, I can hardly understand why somebody close to Krusch did not stay his hand to spare us this sorrowful sight of the decline of a once powerful spirit.

#### IV

In discussing some of the peculiarities of the manuscripts *Paris 4404* and *Guelferb. i. W. 97*, my aim was not only to disprove Krusch's theories, but even more to get my reader acquainted with some of the transmitters of *Lex Salica*. Besides, I would like to stress the fact that the above formulated general rule has been purposely worded in negative terms. Merovingian Latin in a Carolingian manuscript cannot be ascertained unless this Latin stands out against a background of correct Latin. This is a necessary condition, but it is far from being sufficient. To elucidate this difficult point, I am turning to Ms. *Paris lat. 10758* which served me as a guide in my research on the *Capitularia*. The center part of this manuscript (pp. 58–136; page 57 as well as page 137 are blank) is dedicated to a legal treatise whose heading and table of contents are reproduced in facing illustration.

The text that goes with that table of contents starts with excerpts from the *Etymology* of Isidor of Seville. Then follow *Cap. 3* (*Pactus Childeberti et Clotharii*); *Cap. 7* (*Childeberti II decretio*); *Cap. 20* (the so-called *Capitulare Haris-*

<sup>19</sup> *Deutsches Archiv*, II (1938).

INCIPIT LIBELLUS DECRETIO CHLODEUICI  
ET CHILDEBERTO SIVE CHLOTHARIO ET KAROLO  
FUIT LUCIDE EMENDATUM.

- i De legibus diuinis & humanis.
- ii Pactus childeberti regis In desunt cap̄ xxviii.
- iii Decretio chlotharii regis cap̄ xvii Lex suauoru.
- iiii Decretum childeberti regis. De eo qui hominē nobilē pla-  
gauerunt.
- v De rebus quibus conditiombus id sunt cap̄ xvii. 111
- vi Decretio icaroli regis. cap̄ xviii. Memoratio de octo <sup>1111</sup>  
bannos. id sunt cap̄ xxiii. id sunt cap̄. viii.
- vii De septem sopanis cap̄ xviiii. Iussio domini karoli.
- viii Sciendum est quod in quibusdā. id sunt cap̄ iiii.
- iiii Decretum est ut qui in iugit qualis ordinauit propter  
famis inopiam.
- x De quatuor legibus salicis.
- xi De legibus salicis.
- xii De homicidiis clericorum id sunt cap̄ xi.
- xiii Ut nullus presumat hominē. id sunt cap̄ vi.
- xiiii De manire. In desunt cap̄ lxxc
- xv Lex ribuariorū. In desunt cap̄ xxxi.
- xvi Lex alamannorum. P

*tallense*); *De septem septinas* (Behrend, *Lex Salica*, p. 177); *Recapitulatio solidorum*, the repetition of *Cap. 3*<sub>9-15</sub>, the great prologue of *Lex Salica*, the short prologue, *Cap. 39*; *Cap. 40*<sub>11-13, 15-16, 20</sub>, and *Lex Salica* with its 70 titles.

The table of contents does not exactly correspond to the contents which, to begin with, lack the *Leges ribuariorum* and *alamannorum*.

As for the rest, Chapters II-III of the table of contents correspond to *Cap. 3*, divided into two parts; but the figures xcviii and xvii of the table of contents are completely fantastic. There are only eight paragraphs in the first part and ten in the second one. The *lex suavorum* corresponds to just the first words of §11: *De servis ecclesiae*, put into our manuscript in red ink as heading of the subdivision of the second part of *Cap. 3* (§§ 11-18). The same proceeding is used for *Cap. 7*, which, in the text, is divided into three parts, numbered IIII-VI, the last two being headed by the first words of §5. The sentence 'De eo qui hominem nobilem plagaverunt' has no relation to the text, neither can it be found in *Cap. 7*; it is a distortion of a sentence of Title xvii, 5 of *Lex Salica*. The figure xvii of ch. v of the table of contents is again fantastic. Ch. vi of the table of contents corresponds to Ch. vii of the text, but only partly, because the *memoratio de octo bannos* is not in the text; and of the three figures mentioned in the table of contents only the second one (xxiiii) corresponds to the number of paragraphs of *Cap. 20*. Ch. vii of the table of contents is in the text without a running number, but I would not know what the figure xviii and the words *iussio domni Karoli* mean. *Sciendum est (Recapitulatio solidorum)* is in the text under the same running number (ch. viii) as in the table of contents, but it has only one chapter and not four as pointed out in the table of contents. Ch. viii of the table of contents corresponds to the repetition of *Cap. 3* in the text. But this repetition is done in a rather special manner. The distorted heading of *Cap. 3: Pactus Chlodoveo vel Childebert . . .* including §1 is written in red ink and capitals as heading of the following text. It is preceded by the figure lxxviii which gives us the impression that it is a quotation of *Lex Salica* where the *Cap.* in question can generally be found. It ends with the figure lxx followed by the heading of the second part of *Cap. 3 (De rege Chlothario)* still in red ink; and then come §§9-15 in brown ink. Furthermore, the words *propter famis inopiam* in the table of contents have no relation to *Cap. 3* (they resemble the beginning of *Cap. 48*). *Tractatus legis salicae* (ch. x) — that is the great prologue; *pro legis salicae* instead of *Prologus legis salicae* — the short prologue. Chapters XII-XIII correspond to *Cap. 39* and *Cap. 40*, 11-13, 15-16, 20 and Ch. XIII is the *Lex Salica* itself in 70 titles.

The Latin of this part of *Ms. 10758* is rather faulty, as the reader can easily ascertain from the table of contents.

Curiously enough, the direct copy of *Ms. 10758*—*Ms. Paris lat. 4628A*, which is generally on a lower literary level than *10758*—corrects many of the blunders of our legal treatise. Thus, instead of copying the heading as quoted above, it amends it to: 'Incipiunt libelli vel decreta Chlodevei et Childeberti sive Chlotharii et Karoli lucide emendatum.'

It is also important to note that the greatest blunders of *Ms. 10758* are not in the Merovingian *leges* but in the excerpts from Isidore of Seville. Here are some

examples: Instead of *transire per alienum fas est, ius non est*, you find: *transire per alienum fas eius noster*. Instead of *totaque sit inter improbos innocentia*, you read *totaque sit inprobus innocentia*; instead of *qui alterius thorum commaculavit*, it is *qui alter iustorum thorum commaculavit*, etc.

If I wanted to give an explanation of this curious style, I would have to begin by stating that our general rule cannot be applied here. This is not a case in which the scribe endeavors to transmit his Merovingian original in all its peculiarities. The original text which he had to copy, if he had it at all, could not be prior to the ninth century. Proof of it are *Capp.* 39–40, which are included in our treatise and which are dated as of 803. Furthermore, the table of contents, which is undoubtedly an original part of the treatise, is not correct. Under these circumstances there is no other explanation than to suppose that the object of this part of *Ms. 10758* evidently was to have the reader believe that the compiler was a rather illiterate and simple-minded person and, as such, unable to forge; and that he deserved confidence. This assertion is confirmed by the following two facts.

First of all, you do not find a single correction in the whole contents of the treatise. This fact, though negative, is very essential. I tried to prove in my studies on the *Capitularia* that *Ms. 10758* is by Hincmar himself.<sup>20</sup> I am happy to state that Frederic M. Carey ('The Scriptorium of Reims during the archbishopric of Hincmar,' *Classical and Medieval Studies in Honor of E. K. Rand*, New York, 1938), relying on purely palaeographical facts, is sure that the part in question of *10758* was written under Hincmar.<sup>21</sup> Is it not peculiar that in one of the best *scriptoria* of France there was nobody who would correct the impossible blunders of our manuscript?

On the other hand, if you analyze the treatise itself and its table of contents, you get the downright impression that its mistakes are exaggerated. Indeed, repeating the same paragraphs of *Cap.* 3 twice; taking as heading of a paragraph words arbitrarily chosen from the preceding one; transforming the heading *De servis ecclesiae* into *Lex suavorum*; adding sentences which are in no relation to the text in question; writing *Prolegis Salicae* instead of *Prologus legis Salicae*, etc. — all this imbroglia has an exaggerated and forged character. And this character is also stressed by the fine and rapid handwriting as well as by the fact that the digraph *ae*, for instance, is more frequently used than would be the case if the compiler were really rather illiterate (see in the table of contents twice *salicae* and *praesumat* and *passim* in the text itself).

On the whole, the explanation which I suggest is enforced not only by direct arguments which I just stated, but even more so by the fact that this is the only possible explanation and that it perfectly fits the essential character of *Ms. 10758*.

Thus, the fundamental misconception which easily explains the failures of the editors of *Lex Salica* lies in the essential misunderstanding of the character of the manuscript tradition. The faulty Latin of some of the manuscripts was taken for an evident proof of the compiler's desire to transmit to us an exact copy of the

<sup>20</sup> *Moyen Age*, 1941, fasc. 1, pp. 34–38.

<sup>21</sup> I am indebted to Léon Levillain for having called this study to my attention.

Merovingian original. Actually, the faulty language was that of the compiler himself. The second misconception was even more fatal. The editors of *Lex Salica* forgot that the ninth century was the classical era of forgers. And, failing to take some elementary precautions, they boasted of having imparted a more solid knowledge of the legal history, while, actually, they got caught in the most obvious traps.

CENTRE NATIONAL DE LA RECHERCHE SCIENTIFIQUE,  
PARIS, FRANCE.

## APPENDIX

Extracts from Minutes of Annual Meetings of Central Management  
of M. G. H.

- 1903 'Dr. Krammer . . . der am 1. Oktober 1902 als Mitarbeiter bei der Abteilung (Leges) eingetreten war, wurde mit Vorarbeiten für die *Lex Salica* beschäftigt und verglich zunächst deren hiesige (Berlin) bisher noch unbenutzte Handschrift'. (*Neues Archiv*, xxix, 8)
- 1904 Für die *Lex Salica* verglich Hr. Dr. Krammer sechs Handschriften (in zweien derselben auch die *Lex Ribuarica*) so daß die Mehrzahl der wichtigsten jetzt erledigt ist, und untersuchte die Affiliation der Handschriften, wobei neue Resultate sich ergaben. Mit seiner Hülfe konnte Hr. Prof. Zeumer schon einen ersten Versuch der Textherstellung machen. (*Neues Archiv*, xxx, 9)
- 1905 Hr. Prof. Zeumer selbst war leider durch ein Augenleiden im Winterhalbjahr in seiner Tätigkeit behindert, hat aber doch unter Hülfe seiner Mitarbeiter die Leitung der ihm unterstellten Serien fortsetzen und namentlich auch an den Vorarbeiten für die *Lex Salica* sich beteiligen können, die Hr. Dr. Krammer weiter fortgesetzt hat, wobei er wiederum aus Paris hergesandte Mss. collationierte. Über die Textgrundlagen hat er in einem Aufsatz im xxx. Bande des Neuen Archivs gehandelt. Die zahlreichen zu Paris befindlichen Mss. der *Lex Salica emendata* wird er auf einer Reise dorthin in diesem Sommer erledigen. . . (*Neues Archiv*, xxxi, 9)
- 1906 Unter Leitung des Herrn Prof. Dr. Zeumer hat . . . Hr. Dr. Krammer seine Arbeiten für die Ausgabe der *Lex Salica* fortgesetzt und eine Anzahl Handschriften, die nach Berlin versendet werden konnten, hier verglichen, die zahlreichen Pariser Mss. an Ort und Stelle zum allergrößten Teil erledigt und in Ivrea die dort befindliche Rezension eingesehen; die sonstigen italienischen Mss. verglich Hr. Dr. Perels: (*Neues Archiv*, xxxii, 9)
- 1907 In den von Herrn Prof. Zeumer geleiteten Serien der Abteilung Leges hat Herr Dr. Krammer die Vorbereitung der Ausgabe der *Lex Salica*, zum Teil in gemeinsamer Arbeit mit dem Leiter der Abteilung, soweit gefördert, daß die Konstituierung des Textes nunmehr beginnen konnte; der eingehende Editionsplan für die im Druck vorzuliegenden fünf Texte und der ihnen anzuhängenden Stücke wurde der Zentralkommission zur Kenntnis gegeben. (*Neues Archiv*, xxxiii, 7)
- 1908 Herr Dr. Krammer hat die Untersuchung des gegenseitigen Verhältnisses der einzelnen Mss. der *Lex Salica* innerhalb der Handschriftengruppen durchgeführt und die Konstituierung des Textes der von ihm mit A bezeichneten Klasse (sonst III, in der statt der bisher immer gelegten Ms. von Montpellier H 136 die Pariser Lat. 4627 sich als die beste erwiesen hat) soweit gefördert, daß mit dem Druck im laufenden Jahre begonnen werden kann. (*Neues Archiv*, xxxiv, 7-8)
- 1909 Herr Dr. Krammer hat bei der Konstituierung des Textes der *Lex Salica* vor allem die Frage vor Augen behalten, ob man über den Archetypus der neustrischen

- A-Redaktion (früher III) hinaus zum Urtext gelangen könne; insofern nun die älteste, um die Mitte oder gar in den Anfang des 6. Jh. zu setzende Form der nächst jüngeren (austrasischen) B. Redaktion (früher I) bereits der jüngeren Formen von A benutzt hat, wird geurteilt werden dürfen, daß, wenn es auch nicht möglich ist, zum Urtext selber zu gelangen, doch ein Text erreichbar wird, der aus der Zeit Chlodoverchs oder aus der seiner Söhne stammt. (*Neues Archiv*, xxxv, 7)
- 1910 In dem unter Leitung des Herrn Prof. Zeumer stehenden Bereiche dieser Abteilung hat Herr Dr. Krammer die Grundlage für die neue Ausgabe der *Lex Salica* so weit hergestellt, daß der Druck in absehbarer Zeit beginnen kann; zuvor sollen noch zwei Untersuchungen über die Entstehungsgeschichte und die Textentwicklung des Gesetzes veröffentlicht werden. (*Neues Archiv*, xxxvi, 6-7)
- 1911 Was die von Herrn Prof. Zeumer geleiteten Serien der Abteilung Leges anbetrifft, so hat zunächst der ständige Mitarbeiter Herr Dr. Krammer in der Abhandlung 'Zur Entstehung der *Lex Salica* (in der Festschrift für Heinrich Brunner, Weimar (1910) die Ergebnisse seiner Forschungen dahin zusammengefasst, daß unsere ganze Überlieferung der *Lex Salica* auf eine neue Bearbeitung des alten Gesetzes durch König Pippin, vermutlich aus den Jahren 762/64 zurückgeht; des weiteren beschäftigte ihn die Anlage des umfangreichen sachlichen Kommentars zu dem Urtext und die Abfassung einer als selbständiges Buch demnächst zu veröffentlichenden Textgeschichte der *Lex Salica*. (*Neues Archiv*, xxxvii, 7)
- 1912 Unter Leitung des Herrn Prof. Zeumer hat . . . zunächst Herr Dr. Krammer das Manuscript seines Textes der *Lex Salica* so weit hergestellt, daß es nach einer letzten Revision in Druck gegeben werden kann. Die Textgeschichte wird abschnittsweise im Neuen Archiv veröffentlicht werden. (*Neues Archiv*, xxxviii, 7)

## LEX SALICA, II

By SIMON STEIN

'De la méthode avant toute chose'

### V

THE only thing to do is to approach the problem of *Lex Salica* as if it were a recently discovered document about which nothing definite is yet established. Therefore, I ask my reader to forget everything he knows about this law, above all, the theory that formed the basis of every study and edition of *Lex Salica* according to which a new official version of this *lex*—known under the name of *Lex Emendata*—was composed under Charlemagne. This theory played a great role in legal history. Following the pattern of *Lex Salica*, the editors found two distinct forms for the other *Volksrechte* (*Lex Ripuaria*, *Lex Alamannorum*, etc.), namely: *Antiqua* or *Vulgata*, and *Emendata* or *Renovata*. Krusch's critical conception of *Lex Emendata* as a natural effect of the Carolingian Renaissance is the basis of all his constructions as well as his original sin. He did not realize that the division of all the manuscripts into two unequal groups, one containing a dozen manuscripts of the 'Merovingian texts' and the second the *Lex Emendata* of Charlemagne was far from having any foundation. And how could he have any doubts about this division? On page 265 of the Pardessus edition he found the chapter dealing with the 'fifth text.' This chapter bears the title: 'Lex Salica a Carolo Magno emendata anno DCCLXVIII.' In the very beginning of this chapter, Pardessus states: 'The text that I am publishing now is almost always called in the manuscripts *Lex Reformata*, *Lex a Carolo Magno emendata*; and it is also generally known as such by scholars.

The first part of this statement is completely inaccurate. Far from 'almost always,' *never* does a manuscript describe this text in the manner stated by Pardessus. Thus, in the corresponding chapter of Hessels' preface, only the second part of Pardessus' affirmation is reproduced: 'The text of this family is *usually* called *Lex reformata*, *Lex (a Carolo Magno) emendata*.' Not a single word on the subject of manuscripts! The second part of Pardessus' statement is, however, exact: All<sup>1</sup> scholars, starting with Jean du Tillet, Bishop of Meaux, were convinced that Charlemagne had *Lex Salica* revised. In the middle of the sixteenth century, Jean du Tillet published *Lex Salica* for the first time as a small, very elegant in-16° edition under the title: 'Libelli seu decreta a Chlodoveo, et Childeberto et Clothario prius aedita, ac postremum a Carolo lucide emendata, auctaque plurimum.' There is no doubt that Jean du Tillet relied upon *MS. Paris 10758* and that the title cited is only a rectification, though unfortunately an arbitrary

<sup>1</sup> I believe J. Grimm was the only one to deny, in his preface to Merkel's edition of *Lex Salica* (1850), the existence of a version of this *lex* edited by Charlemagne: 'An eine Karlische Recension des salischen Gesetzes, die man *lex emendata* oder *reformata* zu benennen pflegt, kann ich auf keine Weise glauben' (p. lxxxiv). But Grimm's remark, not sustained by any proof, did not influence subsequent research and it has not even been taken up by any scholar.

one of the title of the legal treatise mentioned above (*Incipiunt libelli vel decretio Chlodoveo et Childeberto sive Chlotario et Karolo fuit lucide emendatum*).<sup>2</sup>

To repeat, Jean du Tillet arbitrarily corrected *MS. 10758*, and this correction has had its fatal consequences in all the studies on *Lex Salica*. According to the manuscript tradition and in flagrant contradiction to all scholars, *Lex Emendata* is a Merovingian *lex*. Indeed, the great prologue (*MS. 10758*, pp. 79–80) which states that the ‘gens francorum . . . dictavit salicam legem per proceres ipsius gentis qui tunc tempore eiusdem adherent rectores . . .’ continues as follows: ‘Adubi favente deo rex francorum chlodoveus torrens et pulcher et inclytus primus recepit catholicam baptismum quod minus habebatur in pactum idoneum. Per praeclso reges chlodoveo childeberto chlothario fuit lucide emendatum.’ Thus, according to the great prologue, there were two versions of *Lex Salica*, but neither has anything to do with Charlemagne. The first was composed before the conversion; the second and only one preserved, the *Emendata*, was finished by Chlotharius.

As for the character of the title of the legal treatise — corrected in such an untoward manner by Jean du Tillet — there can be no doubt that the compiler of *MS. 10758* wanted his readers to believe that the scribe of the little treatise had been so simple-minded and so little-lettered he was not even able to make up a title of several lines. He had to borrow a quotation from the prologue of *Lex Salica* and had to adapt it to his ends by awkwardly adding ‘libelli vel decretio’ in the beginning and interpolating the name of Carolus.

However, concerning the date of this version of the so-called *Emendata*, there is some disagreement between the ancient and the modern scholars. Pardessus states that ‘two manuscripts, one in France and the other in a foreign country . . . mention a date for this redraft (*lex emendata*).

<sup>1</sup> *MS. 4626* of the Royal Library contains the following mention: ‘Anno ab incarnatione D.N.I.C. DCCLXVIII, indictione VI, dominus rex noster Carolus hunc libellum tractati legis Salice scribere iussit.

<sup>2</sup> *MS. 728* of St. Gall Chapter, in an almost identical formula, offers the year DCCLXXVIII.

Of these two dates, DCCLXVIII and DCCLXXVIII, only the first one concurs with the indiction VI. However, it failed to draw the attention of Du Tillet, Fr Pitou, Bignon or Baluze. They do not give their reasons for that neglect. Maybe they did consider it of little probability that Charlemagne decided to have *Lex Salica* revised immediately after his father’s death, in September 768, at a time when Charlemagne still shared the throne with his brother Carloman. Consequently, but without reference to any manuscript, they adopted the date of DCCXCVIII.<sup>3</sup>

<sup>2</sup> See *SPECULUM*, XXII, 2 (April, 1947), p. 130.

<sup>3</sup> As for Baluze, Pardessus was wrong. It is true that Baluze adopts the date 798 without motivating it, but if you read his commentaries attentively (*Capitularia*, Vol. II, col. 1047–1048), it is not difficult to realize why he neglected the date offered by *Ms. 4626*. Indeed, Baluze is faced by a very important problem which, unfortunately, existed neither for Pardessus nor for Pertz, nor for any of the modern scholars namely: How did it come to pass that after the promulgation of *Lex Salica* by Charlemagne, prior versions were still being copied — versions which, in all probability, should have lost their value and validity? Baluze was of the opinion that ‘semel emendata et promulgata a Karolo Lege Salica, vetere editione tum Franci non utebantur.’

Pardessus adds the following note to the quotation of *MS. 4626*: 'It is true *MSS 4628A and 4760* contain the same statements, but without either a date or a blank space to put it in; they both are probably copies of *MS. 164 bis (suppl. Lat.)* in which most certainly a date was given. But since the days of Baluze the figure had been *cultelli ope erasus*, as Baluze says (Vol. II, col. 1048), and as I made sure myself. Upon my request, attempts were made by the Royal Library to restore the date, but they all were unsuccessful.'

*MS. 164 bis (suppl. Lat.)* is now *MS. 10758*. Hessels scrupulously follows Pardessus in the part of his preface dedicated to *Lex Emendata*. He even repeats Pardessus' mistakes in the quotation of *4626* (though it did not seem difficult to convince himself that the *MS.* in question did not show the words 'dominus rex noster Carolus,' but 'donnus Karolus Rex Francorum'). Hessels only omits to mention *MS. 10758* while repeating Pardessus' utterances on *MSS 4628A and 4760*. This omission is especially strange as, according to Pardessus, the last two manuscripts are nothing but copies of *10758*.

On the other hand, the method used by Pardessus is far from satisfactory. It is not enough to choose one of the manuscripts which advances a seemingly probable date; the filiation of manuscripts must first be established. It is true that it is not always easy to do so, but luckily our case does not present any such difficulty. The manuscript *princeps* of the five manuscripts which transmits the preamble of the *Lex Salica* version in LXX titles — the so-called *Emendata* — is undoubtedly *MS. 10758*. Its influence on *4628A and 4760* has been mentioned already by Pardessus. In my essay on the *Capitularia*, I have shown its influence on *MS. 4626*.<sup>4</sup>

I might add that the odd form *Donnus* of *MS. 4626* can be found as well in *MS. 10758* in which a subsequent addition of a third jamb transformed *n* into *m*. The fifth manuscript, *Sangallensis 728*, might have also been written under the influence of *MS. 10758*. This is quite probable, although I could not advance any specific argument, since I never saw this manuscript and its descriptions are imperfect. In any case, the inverse filiation, the influence of *Sangallensis 728* on *MS. 10758*, is quite excluded.

Thus, the problem changes its aspects. It is not so much a question of adopting the date advanced by *MS. Sangallensis 728* or any other manuscript. We must

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Furthermore, he distinguished between the *lex vetus* and the *lex emendata*, placing all manuscripts containing Malberg glosses in the first group. Now, among these manuscripts, *Ms. Sangallensis 731* had the date 'anno xxvi rigai domno nostro carolo regi' which he erroneously interpreted as 793. The only date concurring with *indictio VI* after 793 but before 800 (*Carolus rex!*) was 798. Consequently, this latter date was advanced by Baluze, for the Carolingian text. I want to add that Baluze when writing the commentaries to his edition of *Lex Salica* (1676), did not yet know *Ms. Paris. lat. 4404* of the ninth century which also contained the Malberg glosses. The latter manuscript (f.I.) bears the following inscriptions:

Codex iste in bibliothecam  
Colbertinam delatus est è  
Gallia Narbonensi anno  
MDCLXXXII

Steph. Baluzius.

<sup>4</sup> *Le Moyen Age*, 1941, fasc. 1., p. 41 ff.

first explain why the date in *MS. 10758*, the manuscript *princeps*, had been carefully erased. Pardessus affirms that the barbarism was committed since 'the days of Baluze.' That is not true. It dates from much earlier times. It may have occurred as early as the ninth century, at any rate not later than the tenth century. For this date had already been erased when it was handled by the scribe of *MS. 4628A*. This manuscript is of the tenth century and represents a direct copy of *MS. 10758* in the part in question.

The explanation of this curious fact seems to me quite simple. I do not doubt that the *Carolus Rex* of *MS. 10758* is not Charlemagne but Charles the Bald. I believe the arguments in my Essay on the *Capitularia* sufficiently prove this.<sup>5</sup> I want to stress that the date of *MS. 10758* was not 768 or 798, but decidedly either 843, 858 or 873. But these three latter dates, challenge the general tendency of the mediaeval legal sources which aimed at extolling the legislative activities of Charlemagne.<sup>6</sup> In this conflict between grandfather and grandson, settled only after the death of the latter, Charles the Bald lost out: The date referring to his reign was suppressed. I could not say whether attempts were made to rectify the date before it was completely suppressed in *MS. 10758*. The erasure is so deep as almost to make a hole. Neither could I express a definite opinion on whether the dates of *MSS Paris 4626* and *Sangal. 728* are not the result of such a transitory correction. This question, however, is of no great importance.

Now allow me to quote in full the little preamble of *Lex Salica* as it is transmitted through *MS. 10758* (p. 85):

ANNO AB INCARNATIONE DN̄I N̄RI  
 IH̄U XPI INDICIONE SEXTA.  
 DOMNUS KAROLUS REX FRANCORUM  
 INCLITUS HUNC LIBELLUM TRACTATI LE  
 GIS SALICAE SCRIBERE IUS SIT.

IN XPI NOMINE INCLITUS INCIPIT  
 LEGIS SALICAE

It is easy to perceive that there is no question either of a new edition or of a re-draft of *Lex Salica*.

How odd! From these five lines all possible futile conclusions — except the right one — have been drawn.

Let me summarize: According to the manuscript tradition of *Lex Salica*, all

<sup>5</sup> *Op. cit.*, pp. 37 ff.

<sup>6</sup> *Op. cit.*, p. 46.

our manuscripts represent *Lex Emendata*. MS. 10758 affirms that the prototype of the most important group (the LXX title version) was written by order of Charles the Bald. From this last statement a strong and essential conclusion must be drawn. Up to the reign of Charles the Bald and during the more or less important part of it, the Carolingian Chancellery did not possess any copies of *Lex Salica*. This conclusion is convincing even if the statement of MS. 10758 that the version in LXX titles had been written by order of Charles the Bald is wrong.

In advancing this thesis I was careful enough to verify whether it could be proved that any of the manuscripts transmitting the so-called *Lex Emendata*, were written prior to the reign of Charles the Bald. The result of my investigation was very encouraging; none of the manuscripts of this version was written prior to the reign of Charles the Bald.

## VI

The version of *Lex Salica* in LXX titles is not prior to the reign of Charles the Bald. Are other versions prior to it? That question should be decided primarily by the date of the manuscripts. Now, the following five manuscripts are dated as of either the middle or the end of the eighth century or the beginning of the ninth century:<sup>7</sup>

1. *Paris 4404* — early ninth century
2. *Guelferbytanus i.W. 97* — middle of the eighth century
3. *Monacensis 4115* — end of eighth or beginning of ninth century
4. *Paris 4403<sup>B</sup>* — end of eighth or beginning of ninth century
5. *Sangallensis 731* — the only one with a precise date — October 793.

I divide these five manuscripts into two groups placing manuscripts 2-4 in the first group; the date of these manuscripts is not based on explicit arguments. In their case, I merely remark that scholars with authority and experience beyond doubt have advanced more recent dates for these three manuscripts.

As far as *Guelferbytanus inter Weissenburgenses 97* is concerned, Pertz insisted on assigning this manuscript to the eighth century: 'Only a person who never saw the manuscript in question or has no judgment at all about manuscripts may doubt the fact that the handwriting of this manuscript is of the eighth century.' (*Archiv*, VII, p. 732).

This categorical statement did not prevent G. Hänel, the eminent editor of *Lex Romana Visigothorum* (1849) and the best judge of legal manuscripts, from dating our manuscript as of the ninth century. He mentioned that Pertz dated it as of 752-768, 'neque, qui IX saec. accipiat codices vidisse concedit' (p. LXXI, n. 338). In the preface of his edition of *Codex Theodosianus*, Th. Mommsen joined Hänel in his dating; though, at the same time, he falsely ascribed the opinion of Pertz to Hänel.

I turn now to *Monacensis 4115*. The Catalogue of manuscripts of Munich (III, II, p. 139) assigns it to the eighth or the ninth century. The same vague date was assigned to it by L. R. de Salis (*M.G.H., LL. nat. Germ.*, Vol. II, p. 17) and by

<sup>7</sup> See SPECULUM, XXII, 2 (April, 1947), p. 121 on the date of *Ms. Vat. Reg. Chr. 846*.

Werminghof (*M.G.H., Capitularia r. Fr.*, II, p. xviii). In his edition of the *Capitularia* (*LL*, I, p. xviii), Pertz dates it as of the end of the eighth or of the ninth century; while his essay on the manuscripts of *Lex Salica* (*Archiv*, VII, p. 735) assigns to it a more precise date, namely the end of the eighth or the beginning of the ninth century. This last date is repeated by Lehmann (*LL. nat. Germ.*, v, p. 11) and by Sohm (*LL.*, v, p. 196). Merkel does not hesitate to state laconically that this manuscript is of the eighth century (*LL.*, III, p. 3). Pardessus does not give it any date at all. All these dates are arbitrary and their divergence is the best proof that our manuscript might date from the end of the ninth century as well.

The last manuscript of this group is *Paris 4403<sup>B</sup>*. Regarding it, I merely quote Hänel (p. LXXXII): 'Secundum Catalogum et Pertzium saeculo VIII. scriptus est. Pardessus distinguit et recte quidem. Priores enim septem membranae saeculi VIII, reliquae IX.' *Lex Salica* is among those *reliquae*.

I must now analyze the two remaining manuscripts, *Paris 4404* and *Sangallensis 731*. The date of the former is supplied by its preface, where the compiler writes '... domino nostro Karolo imperatore edictus eius extremus scribitur.' At the end of this manuscript (ff. 232<sup>v</sup>-234<sup>v</sup>) there are *Capitularia* 39, 40<sub>1-6</sub>, 8-20, 29, 67 and 82 usually ascribed to Charlemagne. Thus, our manuscript should have been written between 800 and 813. If, however, you look at it closely, an important fact is revealed. The series of *Capitularia* beginning at the second column of F. 232<sup>v</sup> have been subsequently added to the preface. This can be proved by the fact that the ink used from the head of the second column of F. 232<sup>v</sup> to the end of the manuscript, is of a quite different color compared with the entire manuscript and its preface.

On the other hand, the single *edictus* of the preface could not possibly refer to the above quoted series of *Capitularia*. Evidently, the end of *MS. 4404* in its original state, included an *edictus* which did not reach us and which commenced at folio 233. It might just as well have been quoting an *edictus* by Charles the Bald. Consequently, the preface could not serve as a decisive proof for dating our manuscript as of the beginning of the ninth century. I would add that Krusch was inclined to assign *MS. 4404*, on the basis of its handwriting,<sup>8</sup> to the middle of the ninth century. We now come to the last manuscript, *Sangallensis 731*. I am happy to furnish a convincing proof that it dates from the time of Charles the Bald. The date of this manuscript is based on the note of the scribe written on p. 342: 'Expleto libro terio die veneris kl. novembris anno xxvi regni domno nro Carolo regi.' Nobody doubted that 'King Charles' was Charlemagne, although it is obvious that a scribe living under Charles the Bald would employ the same words. Clearly, the date of the above manuscript can not be solved by page 342 alone. It may belong to 793 as well as to 866.<sup>9</sup> Page 111 provides us with a decisive proof

<sup>8</sup> 'Der Schrift nach möchte ich die Hs. eher in die Mitte des 9 Jhs. setzen als an den Anfang.' *Neue Forschungen*, p. 180.

<sup>9</sup> According to A. Giry's *Manuel de Diplomatique*, 1894, p. 728: the years of the reign of Charles the Bald are computed beginning with 21 June, 840. Thus, November 1 of the 26th year of the reign of Charles the Bald should be in 865. This was the date I first mentioned in the draft of my essay. I am obliged to the late Marc Bloch and Léon Levillain who both called my attention to the fact that I

of the latter date. On this page we find a drawing representing a young man who carries in his raised arms an object which Baluze called 'sigillum Karoli Magni.' I have not seen *MS. Sangallensis 731*, but fortunately page 111 is reproduced in Zimmermann's *Vorkarolingische Miniaturen* (1916), Vol. II, Tb. 151. In a note added to the manuscript in question (see Hänel, p. LXXII, n. 270), Baluze wrote: 'Pag. 111 nota sigillum Caroli M. quod in diplomatibus occurit, in quibus eo loco imago imperatoria ponitur, ubi hic eius monogramma legitur.' Baluze's statement is incorrect. Looking at the picture attentively, one can see clearly it does not represent a seal but, rather, a coin (*denarius*). To become convinced, it is sufficient to read the text facing the image; 'Quicumque (the Q is drawn on the clothing of the young man) adulterum monedario prodedire et his qui proditus est se de monida adulteracione fuerit adprobatus ignibus concremettur.' This is the corrupted text of the *Breviarium Alarici*, L. IX, 17, the correct text of which reads: 'Praemium accipiat quicumque adulterum monetarium prodiderit et is qui proditus est si de monetae adulteratione convictus fuerit, ignibus concremetur.'

Thus, the suggested date of 866 in close proximity to the *Edictum Pistense* (864) which introduced the new *denarius* of Charles the Bald, marvellously falls in with this *Edictum*. Besides, the picture of the *denarius* is a true copy of the numerous *denarii* of Charles the Bald.

My interpretation could be opposed by the fact affirmed by numismatists that the *denarius* with the monogram of Carolus on it had already been coined under Charlemagne. I consider it quite impossible that the new *denarius* introduced by Charles the Bald and supposed to replace all the preceding *denarii* could have been a copy of the *denarii* of Charlemagne. I am happy to find support in the opinion of A. Dieudonné reported in the *Revue Numismatique*.<sup>10</sup> To sum it up: we do not possess a single manuscript containing *Lex Salica* which can be proved to date from prior to the reign of Charles the Bald. The most ancient manuscripts the dates of which might be convincingly proved, were written during this reign.

## VII

If the manuscript tradition transmitting *Lex Salica* does not reach beyond the reign of Charles the Bald, are the other testimonies more ancient?

Some fifteen years ago, I undertook to prove that in the ancient language the term 'Lex Salica' not only had the significance which we now attribute to it but was also a general term signifying *lex dominica*, the royal law.<sup>11</sup> Now I am happy to support my old thesis with some more precise information.

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would have to verify whether November 1 of 865 is a Friday. Since it is not, I subsequently changed the date to 866. I would like to add that this confusion in the dates of the reign of Charles the Bald is not an exception. Here is another example of an analogous confusion: '... la charte épiscopale a été déjà approuvé par un synode tenu en 849, l'an 9 du règne, aux nones de novembres.' (F. Lot and L. Halphen: *Le règne de Charles le Chauve*, Paris, 1909, p. 197, n°6.).

<sup>10</sup> *Revue Numismatique*, 5 série, Vol. 3 (1939), Procès-verbaux de l'année 1938-1939, Séance du 3 décembre 1938: 'J'exclus les Carolus qui peuvent être tous de Charles le Chauve.'

<sup>11</sup> 'Lex et Capitula,' *Mitteilungen des Österreichischen Instituts für Geschichtsforschung*, Vol. XVI.

I maintain that up to the reign of Charles the Bald the term 'Lex Salica' never signified the code we now know under that name. To become convinced, you have but to refer to the *Edictum Pistense* (Cap. 273). Indeed, Cap. 34 of this edict begins:

Notum fieri volumus omnibus Dei et nostris fidelibus quoniam quidam comites nostri nos consuluerunt de illis Francis hominibus, qui censum regum de suo capite, sed et de suis rescellis debebant qui tempore famis necessitate cogente se ipsos ad servitium venderunt. Unde cum episcopis et ceteris Dei ac nostris fidelibus tractavimus, quid nobis esset agendum; et quod cum eis inde invenimus ac constituimus, praesenti edicto decrevimus: Id est, *quiva non in lege Salica ex hoc expressius quiddam invenimus, continetur tamen in tertio capitulorum libro capitulo XXIX de homine libero 'qui se loco vadii' . . .* (a quotation from Ansegisus *L.*, III, XXIX follows).

It is clear that the question posed in this quotation has nothing to do with *Lex Salica* in the actual meaning of this term. More curious, however, is the ending of the quotation. It strongly indicates that the author of this *capitulare* considered Ansegisus' Collection a part of *Lex Salica*.

The fact observed in *Edictum Pistense* may be corroborated by the 'responsa missa cuidam data' (Cap. 58). In the second part of my *Etude critique des Capitulaires Francs* (not yet published), I formulated the thesis that these *responsa* were of the time of Charles the Bald, and must be brought into relation with Cap. 274,15 (anno 865). Be it as it may, the second paragraph of this document gives us interesting details concerning the meaning of the term 'Lex Salica.'

2. De secundo unde me interrogasti, si comes de notitia solidum unum accipere deberet et scabinii sive cancellarius. Lege Romanam legem, et sicut ibi inveneris exinde facias; si autem ad Salicam pertinet legem et ibi minime repereris quid exinde facere debeas, ad placitum nostrum generale exinde interrogare facias.

It would be a waste of time to resort to the principle of the 'personality of law' when interpreting this paragraph. It does not depend upon the person asking for the *notitia* whether the Roman law or *Lex Salica* will be referred to. The Royal Chancellery mentions both laws only because they are not sure in which of them this question is settled. But there is an even more interesting fact in this paragraph, namely the term 'Lex Salica' cannot have the meaning that we are now giving to it. The Royal Chancellery could not ignore the fact that none of the seventy titles of *Lex Salica* had dealt with the question asked by the *missus*. Referring the inquirer to *Lex Salica*, the Chancellery might have had in mind either the Collections of Ansegisus or of Benedictus Levita, or some other analogous collections; they might also perhaps recur to customs confirmed by their antiquity; but never would they have thought of referring the inquirer to a law which, according to modern scholarship, was promulgated by Clovis and his sons and supposedly redrafted by Charlemagne.

Are there any arguments opposing these two formal evidences? What proof do we have of the existence of *Lex Salica* prior to the reign of Charles the Bald? There is just one bit of evidence — Cap. 142. Cap. 142 is not a *capitulare*, but, as Boretius says, a 'iudicium scriptumve scabinorum vel iuris peritorum Salicorum ad legem Salicam suppleendam et controversum ius dirimendum . . . imperatore

ipso absent conceptum.' In my essay 'Lex et Capitula,' I have already proved that this *capitulare* has nothing to do with §5 of *Cap.* 143. Therefore, its surmised date (819 *vel paulo post*) has no foundation whatsoever. 'The absent emperor' may be Charles the Bald as well as Louis the Pious. The most ancient manuscript transmitting this *capitulare* is *MS. Paris Lat. 10758*. Thus, no argument can be advanced by the manuscript tradition either. In any case, theories based on *Cap.* 142 will never shake my thesis: Charles the Bald's Chancellery, for a time at least, did not possess the text of the law we now call 'Lex Salica'; and even under Charles the Bald this term signified quite another thing.

## VIII

It is much more difficult to fight a theory that is not founded at all, but still is accepted as evident, than to fight a well-founded theory. It is possible to shake the foundations of a theory, but what can you do if it has none?

Several years ago, in a friendly chat with an eminent historian, I expressed my first doubts on the subject of *Lex Salica*. My partner listened to me indulgently for a certain time, but then he interrupted me and said: 'Leave it alone; *Lex Salica* is *Lex Salica*.' After many years of renewed research, I still insist on the thesis that *Lex Salica* is not *Lex Salica*.

Returning to the argument, I shall take Theodulf of Orleans as witness. His little poem is printed in the first volume of *Poetae aevi Carolini* (*M.G.H.* in 4<sup>o</sup>, pp. 517 ff., number xxix). If I am not mistaken, G. Monod alone commented on this poem. This is what he wrote:

Theodulf appears indignant at the severity of the law which strikes theft with most severe chastisements, even with capital punishment, while at the same time indulgence is shown towards murder punished by a pecuniary compensation only.

Hirsutam capiat si forsan quisque capellam,  
Stipite suspensus excruciatu obit;  
Si furibundus atrox homines percusserit amens,  
.....  
Vile datur pretium tanti pro crimine facti  
Aut numi aut pecudis.

(*De comparatione legis*, v. 49 et sqq.)

We know, indeed, that the Merovingian laws were merciless in case of theft. They often punished it by death. (*Pactus pro tenore pacis*, c.10. — *Childeberti decretio* a. 596, c. 7.) However, efforts made to punish homicide severely in imitation of the Roman law, could not triumph over the quite natural indulgence of the Merovingian society towards acts of violence. No other reaction of these acts could possibly be expected on the part of a society lacking police and in which individual force and courage were the only guarantee of security. This inequality — shocking as it is for us — may be easily understood in an era in which wealth had more value than human life; murder was much more frequent than today; and theft appeared to be more criminal than murder.<sup>12</sup>

I must confess my inability to understand that quotation. How did it happen that G. Monod, in commenting on a poem of Theodulf of Orleans, refers to Merovingian laws and does not breathe a word about the *Lex Salica emendata*.

<sup>12</sup> *Revue Historique*, xxxv (1887), p. 11.

When quoting *Childeberti decretio*, chapter 7, which provides capital punishment for theft, why did he not confront it with chapter 5 of the same *decretio* ordering capital punishment for murder? Meanwhile, had he merely read the whole *decretio*, the final sentence of chapter 5 — ‘quia iustum est, ut qui novit occidere, discat morire’ — could not have failed to impress him.

I shall pass over in silence the rest of this quotation, which is nothing but a vague deduction from inexact premises. I shall not even ask whence Monod derived his *special* ideas about the indulgence as far as murder was concerned, and what kind of comparative criminal statistic he had at hand.

I turn now to Theodulf himself. The characteristics of this little poem as given by Monod are far from being exact. Theodulf does not show any interest in the subject of the ‘indulgence towards murder,’ but he does so on quite another question. He compares the criminal code of the Old Testament with that of his times and is indignant at the severity of the latter: ‘Omnes diverso nunc currunt tramite leges, *Nescio, sint leges an mage sitque furor.*’

It is advisable to furnish a greater part of this poem and to present Monod’s quotation in its natural setting. After having characterized the precepts of the Old Testament, Theodulf continues:

- |    |  |
|----|--|
| 45 | Omnes diverso nunc currunt tramite leges,<br>Nescio, sint leges an mage sitque furor.<br>Sanguine pro porci, fuso datur immo caput iam,<br>Pro pecude humanus funditur ecce cruor.   |
| 50 | Hirsutam rapiat si forsitan quisque capillam<br>Stipite suspensus excruciatu obit.<br>Si furibundus atrox homines percusserit amens,<br>Et calor exanima membra relinquat ebes,<br>Vile datur pretium tanti pro crimine facti<br>Aut nummi aut pecudis <i>aut rude saepe metum</i> |
| 55 | Cara creatori maneat cum cuncta creata,<br>Carior in cunctis, crede, creatus homo:<br>Nec vili pretio pecudis nec aequus haberi<br>Celsior in cunctis iste putandus erit.  |

Monod committed two grave errors. Not only did he remove the quotation from its context; he also suppressed the last four words of the quoted sentence. These four words make the meaning of the quoted sentence quite unintelligible. Reading verses 51–54 attentively, one arrives at a strong and forceful conclusion: either verses 53–54 are quite corrupted (the words ‘vile pretium pecudis’ are repeated in verse 57), or verses 51–54 are nothing but an interpolation into which verses 55–59 do not fit. Actually, what sense do the words make ‘Carior in cunctis, crede, creatus homo,’ when there is talk about murder for which Theodulf (according to Monod), demands capital punishment as expiation? Suppressing verses 51–54, this *maxim* presents itself quite naturally, being *nothing* but a new formulation of the idea already expressed in verses 47–48:

Sanguine pro porci fuso datur immo caput iam  
Pro pecude humanus funditur ecce cruor.

Since the little poem is transmitted by only one manuscript of the fifteenth century, nothing prevents us from supposing an interpolation.

Even were I mistaken in my argument about Monod, one thing is still certain. In Theodulf's day theft was chastised by death, whilst *Lex Salica* provided only a fine for theft. Thus, Theodulf did not know the law we now call 'Lex Salica.' This latter was not valid at that time. I doubt that an historian could arrive at any more forceful conclusions.

To corroborate this thesis I quote the last chapter of *Cap. 20*:

De latronibus ita precipimus observandum, ut pro prima vice non moriatur, sed oculum perdat, de secunda vero culpa nasus ipsius latronis abscidatur; de tertia vero culpa, si non emendaverit, moriatur.

Is there still any need of repeating that *Lex Salica* did not foresee capital punishment for the *culpa prima* of a *latro* and that our *Capitulare* did not modify this law, but quite another one.

## IX

How did it happen that an eminent French historian like Monod managed to advance affirmations as fantastic as his?

I already stated it and I repeat it. The greatest difficulties of studying *Lex Salica* do not lie in the law itself. They consist of the strange ideas formed on the subject of the Middle Ages. According to these conceptions, only the most absurd affirmations are taken as the plausible ones. The modern historians, as true heirs of the Age of Enlightenment, appear to be presumptuous and haughty to such a degree as not to concede to the Middle Ages even the most elementary common sense. Imagine only, there was a time 'when wealth was worth more than human life'! This, in our times, shocking inequality — Monod wrote — may be *perfectly* understandable to the Middle Ages! That sounds convincing, does it not? The more so as Monod begins his reasoning by the affirmation that Theodulf of Orleans himself became indignant about it. I would not want to leave my reader under the impression of Monod's quotation. I feel obliged to make amends. I am now going to quote a gem of the legal literature of the Middle Ages. It is a model of a testament in the form of a letter from a father to his daughter, transmitted to us by Marculf (*L.*, II, 12):

*Dulcissima filia mea illa ille. Diuturna sed impia inter nos consuetudo tenetur, ut de terra paterna sorores cum fratribus porcionem non habeant; sed ego perpendens hanc impietate, sicut mihi a Deo aequales donati estis filii, ita et a me setis aequaliter diligendi et de res meas post meum discessum aequaliter gratuletis. Ideoque per hanc epistulam te, dulcissima filia mea, contra germanos tuos, filios meos illos, in omni hereditate mea aequalem et legitimam esse constituo heredem.*

Is not this letter the very pattern of perfection in its serenity and humanity?

I do now want to take part in the difficult controversy concerning the date of Marculf's collection. I merely point out that the three principal manuscripts transmitting this collection are of the ninth century. In the absence of strong proof to the contrary, I am not inclined to suppose that a work was written much earlier than the most ancient of its copies. But for the solution of the question I

am now discussing the date of the collection is of no importance. Whether it is of the eighth or the seventh century matters little.

The quoted letter, at any rate, concerns a norm of *Lex Salica* and even more than that. It refers to the most essential norm of this law, the one that for many centuries was considered as its principal contents. This norm is characterized as *diuturna, sed impia consuetudo*.

No doubt is possible: the term 'consuetudo' was used only in opposition to the *lex scripta*. ('Lex est constitutio scripta. Mos est vetustate probata consuetudo, sive lex non scripta' — Prologue of the *Lex Baiuvariorum*). Besides, it would never do to characterize as "impia consuetudo" something which formed a part of the most ancient and most venerable code.

There's no need to add anything on the subject of Marculf's collection and on the extent of his legal knowledge. The quoted letter will play its decisive role in my research by itself without the help of any commentaries.

Here is my summary. Since the middle of the ninth century, there appear numerous manuscripts transmitting a code which, according to their affirmations, was promulgated by Clovis and his sons. The title of this code — *Lex Salica* — is not fantastic; it is an ancient term signifying the Royal Law. The Chancellery of Charles the Bald does not possess this code and, while referring to *Lex Salica*, quotes the Collection of Ansegisus. Moreover, there is no proof that this code had been valid at any time. One part of the norms of this code was not valid either under Charlemagne (Theodulf of Orleans) or under Charles the Bald (*Cap. 20*)<sup>13</sup>. The most characteristic norm of this code is qualified, in a very important legal collection, as being 'diuturna sed impia consuetudo.'

It seems to me that my final judgment on *Lex Salica* cannot be considered too daring: **LEX SALICA IS A FORGERY!**

## X

If *Lex Salica* is a forgery composed in the middle of the ninth century, it should have betrayed itself as such. And so it does more than once.

Let us turn to the great prologue. The most irreconcilable enemies agree on its date. Krammer and Krusch both think it was composed during the reign of Pepin, but the date advanced by these two scholars appears to be quite inadmissible. No reign was less propitious for extolling the work of the Merovingian kings than the reign of Pepin. After his *coup d'état*, Pepin was anxious to annihilate every trace of the Merovingian activities. This crusade accounts for the almost total lack of sources of this era that we now experience. Einhard, in his *Life of Charlemagne*, pays tribute to this anti-Merovingian tendency when he portrays the 'lazybones kings.' The first Merovingian rehabilitation — as it seems — was effected under Charles the Bald simultaneously with the appearance of the first pamphlets directed against Charles Martel (See *Epistola synodi Carisiacensis ad Hludovicum regem Germaniae directa* [*Cap. 297, anno 858*] with the well-known *Narratio de*

<sup>13</sup> See *Le Moyen Age*, 1941, fasc. 1, pp. 38 ff.

*perditione aeterna Caroli principis*). Therefore, the date of the prologue could never be opposed to my theory. Besides, all the stories told in this prologue are quite fantastic stuff! The Wisogastis, Bodogastis, Salegastis, and Widogastis are no more historic personalities than Sachamae, Bodoamae and Widohamae are geographic realities.

The falsity of the prologue is almost universally recognized, if one discards Krammer's opinion that the prologue was Pepin's patent. But, while yielding to the evidence, the historians at the same time were eager to save the precious information transmitted through the prologue. To this end they invented an explanation stating that if the prologue were false, the kernel thereof must be true. I am more inclined to believe that details of a forgery may be true while the kernel is false.

The decisive fact is not only the existence of a false prologue but the absence of a true one. Suppose Clovis and his sons did promulgate the first code of the Frankish law. Then they would have to furnish it with an explanation, an *auctoritas*, analogous to Alaric's *auctoritas*, and to urge counts and judges to act by this code. Let us suppose this preamble was lost. If the code actually was in force, who would think of replacing the preface by a false one? Decisively, the false prologue impairs the code itself. An honest person does not use false recommendations.

The fact that the forgers did not content themselves with one prologue but made up two of them is characteristic enough. The essentials of the little prologue are repeated in the great one. The little prologue seems to refer to the first draft of *Lex Salica*, which mentions no kings; the great prologue refers to the *Emendata* of Clovis and his sons. Both prologues use the third person. The author or authors do not claim to have been witnesses of the facts they relate. There is a curious detail in the little prologue: it characterizes the 'villae Bodochem, Salechem and Widochem' as situated beyond the Rhine ('*ultra Renum*'). Thus, this prologue was written in France.

## XI

The second feature by which *Lex Salica* betrays itself is its monetary system.

First, we must state that the annoying repetition of the formula — *tantos denarios qui faciunt solidos tantos* — sounds evidently false. Why should the legislator so many times repeat the equation:  $40 \text{ denarii} = 1 \text{ solidus}$ . If it was a usual equation, why repeat it? Was it a new one? If so, the author should be more explicit and more concise. He should inform the reader that this is a new system and present it as such in calling the *denarius* a *denarius novus*, or in some analogous manner. He should be content with one or, at most, several mentions of the system to which he refers.

The author's annoying repetitions give me the impression he is using a system no longer valid. He seems to feel obliged to warn his reader incessantly: do not deceive yourself; it is not a question of the actual *denarii*, but of some quite different ones. This first impression is corroborated by arguments based on numismatics. B. Hilliger first studied *Lex Salica* from this point of view. Before him,

scholars treated numismatical problems relying on *Lex Salica*. Maurice Prou, in his *Catalogue*,<sup>14</sup> placed the analysis of the monetary system of *Lex Salica* at the head of the description of Merovingian coins. It was Hilliger who first confronted the numismatical data of *Lex Salica* with information from other sources and the coins themselves. Unfortunately, his belief in the authenticity of this law vitiated his conclusions. He limited himself to shifting the date of *Lex Salica* from the sixth to the seventh or even the eighth century.

Without going into the complicated and uncertain details of Merovingian numismatics, I shall cite two undeniable facts: (1) The Merovingian *denarius* does not appear before the second half of the seventh century.<sup>15</sup> Hence, the monetary system of *Lex Salica* is incompatible with the ascription of this law to Clovis or to his sons. (2) Up to now, no *denarius* which would answer the equation  $40 \text{ denarii} = 1 \text{ solidus}$  has been found. The only proof of its existence is Ch. XLII of *Concilium Remense anno 813* (*M.G.H., Concilia*, II, p. 257):

Ut domnus imperator secundum statutum bonae memoriae domni Pippini misericordiam faciat, ne solidi, qui in lege habentur, per quadragenos denarios discurrant, quoniam propter eos multa periuria multaque falsa testimonia repperiuntur.

This chapter was interpreted as meaning the criminals aimed at by the law were ready to plead guilty and to pay the fine due. The only reason that stopped them and forced them to commit perjury was the excessive amount of the fine. They were ready to pay at the rate of  $1 \text{ solidus} = 12 \text{ denarii}$ ; they resorted to perjury if the exchange  $1 \text{ solidus} = 40 \text{ denarii}$  was insisted upon.

This interpretation is far from plausible. It does not sound credible that threats of perjury would be sufficient to bring about a diminution of all fines. Moreover, it is not quite clear whether this chapter refers to *Lex Salica*. The first sentence with its verb 'discurrant' rather makes one think of a law introducing a new *solidus*. But this is contradicted by the fact that in the eighth and ninth centuries the *solidus* was just nominal money and only *denarii* were in circulation. Furthermore, it is not clear why the statute of Pepin, which we do not possess, was not still in force in 813 and by whom and when it was abrogated. The number of insoluble questions on the subject of Chapter XLII of *Concilium Remense anno 813* can easily be increased. I believe, however, that the questions already asked are sufficient to shake the authenticity of this chapter — the more so as the acts of the Council of Reims are transmitted only through two manuscripts of the tenth century. And in Reims, precisely, do we find an easy solution of all the difficulties raised by the monetary system of *Lex Salica*.

It is beyond doubt that in the sixth century the *denarius* did not exist; and still less so did the equation  $40 \text{ denarii} = 1 \text{ solidus}$ . However, in the middle of the ninth century, a person of great authority pretended to know that the monetary system accepted in France from the days of Clovis to Charlemagne, was based on the equation  $1 \text{ solidus} = 40 \text{ denarii}$  and that Charlemagne replaced this system by

<sup>14</sup> *Catalogue des monnaies françaises de la Bibliothèque Nationale, Monnaies Mérovingiennes* (Paris, 1892).

<sup>15</sup> See Le Gentilhomme, *Mélanges de numismatique mérovingienne* (Paris, 1940), p. 138.

another, namely: 1 *solidus* = 12 *denarii*. This person was Hincmar, Archbishop of Reims. In his *Vita Sti Remigii*, the false testament of St Remigius is preceded by the following remark:

Exemplar testamenti a beato Remigio conditi, in quo lector attendat, quia solidorum quantitas numero XL denariorum computatus, sicut tunc solidi agebantur, et in Francorum lege Salica continetur et generaliter in solutione usque ad tempora magni Karoli perduravit, velut in eius capitulis invenitur (*M.G.H.* in 4<sup>o</sup>, *SS.r.M.*, Vol. III, p. 336).

The editor of this *Vita*, Bruno Krusch, adds the following very judicious note to this remark: 'Haec Hincmarus adnotavit, ut testamentum genuinum crederetur. Solidi autem ibi legati utrum XL an XII denarios caperent, scire nemo peterat nisi falsarius ipse . . .' I completely agree with Krusch here as well as with his excellent essay on the forgeries of Hincmar<sup>16</sup> I think that Hincmar's remark could also have had the purpose of eliminating any doubts about *Lex Salica* itself.

At any rate, Hincmar's words can not be considered proof of the existence of the monetary system of *Lex Salica* in the sixth century. What knowledge could Hincmar possess on this subject? Indeed, this remark could only be an additional proof of the creation of *Lex Salica* towards the middle of the ninth century. In other words: the monetary system of *Lex Salica* does not correspond to the monetary system of the sixth century; nor even to any of the following centuries from the sixth to the ninth; but it corresponds perfectly to the idea formed in the ninth century about the monetary system which functioned in France from Clovis to Charlemagne.

I do not want to affirm that Hincmar was the author of the forgery known under the name of *Lex Salica*. Not because of lack of clues, but solely because I do not want to spend my time on questions of secondary importance. My purpose is to show that *Lex Salica* is a forgery conceived in the middle of the ninth century, and that this misconception concerning the essential character of this law has, up to now, obstructed its edition.

## XII

The third way *Lex Salica* betrays itself is represented in the Malberg glosses.

The study of these glosses offered almost insurmountable difficulties. Thus, most scholars who have studied *Lex Salica* left it to the philologists to elucidate the questions raised by the glosses. Among the philologists, J. Grimm (Preface to Merkel's edition of *Lex Salica*) and Kern (in Hessels' edition of *Lex Salica*) claimed to have reached definite conclusions. The difficulties offered by the glosses are manifold. At first, one assumes that they were composed in the language of the Franks; for which we have no other sources. Therefore, we have no means of verifying the more or less daring hypotheses about the meaning of different terms. Secondly, the tradition of these glosses is far from perfect. Grimm and Kern agree that the scribes of the still existant manuscripts did not understand the language of the Franks; consequently, they mutilated and corrupted words of

<sup>16</sup> 'Reimser Remigius Fälschungen,' *Neues Archiv*, xx (1895).

this language — sometimes to complete disfiguration. Therefore, before finding out the meaning of any gloss, Grimm and Kern reconstruct or redraft the gloss in its presumably primitive form based on the meaning of the Latin sentence in question, as well as on other Germanic idioms. This method does not impress me as being sound enough to permit definite conclusions. Indeed, neither the point of departure nor the point of arrival — neither the form of the glosses nor the language of the Franks is certain; therefore, all hypotheses expressed on the subject of the glosses are vague and arbitrary.

Scientific hypotheses make sense if the supposed facts do not contradict each other and if they are in concordance with facts assumed. Now, the well established facts show that the glosses of all manuscripts do not play the role attributed to them by the hypothesis in question. The glosses do not and cannot explain anything, because the scribes understood Latin but not the Frankish language. None of our manuscripts can be supposed to have been written in a Germanic speaking country or by a scribe of Germanic origin. None of our manuscripts adds glosses to the glosses found in the original, but many do suppress them. Thus, *MS. Monacensis 4115*, taking the glosses for Greek words, suppresses them completely 'to avoid annoying the reader and to facilitate comprehension.' That is the scribe's remark at the end of the table of contents. He also suppresses any mention of the amount of *denarii* corresponding to a *solidus*, as I mentioned above. Here is what he writes:

Hoc autem super omnia diligenter consederandum est quod in lege Salica unusquisque solidus quadraginta dinarius habet ita ut centum viginti dinarii faciant solidos tres.

Sed nos propter prolixitatem uoluminis uitandam seu fastidio legentium uel propter utilitatem intellegendi abstulimus hinc uerba grecorum et numero dinariorum quod in ipso libro crebre conscribita inuenimus. (Hessels, p. xxix)

*MS. Paris 9653* contains but two glosses on the titles 17 and 35. If we take the family of manuscripts furnishing the greatest number of Malberg glosses, i.e. the family of the version in 99 titles, we find that the most recent manuscripts completely suppress them. Thus, in accordance with the general impression suggested by the manuscript tradition, the Malberg glosses form a body alien to *Lex Salica* which the scribes, explicitly or tacitly, wanted to get rid of.

Such is the impression suggested by the extant manuscripts. The supposition that the Malberg glosses played a positive role in other manuscripts, now lost, runs into many difficulties. First of all, the form of the glosses is too concise to explain anything. In most cases there is but one word and only seldom two or three. They may be rather conceived as a summing-up, but never as an explanatory gloss. Let us take an instance. Title LV of *Lex Salica* in *MS. Paris 4404* is worded as follows:

Si quis corpus occisi hominis antequam in terra mittatur expolauerit in furtum mal uaderido hoc est MMD din. qui fac. sol. LXIII culp. iud.

In *MS. Guelf. i. W. 97*, the gloss is not *uaderido* but *muther*; *Paris 18237* has *freomosido*; *Montepess. H. 136* has *creomardo*; *Paris 4627* has *chreomardo*; *Sangall. 731* has *crehomardo*.

Kern suggested (p. 545) reading *murderido*, not *uuaderido* in MS. 4404; *murther* — in *Guelf.*; *hrëomordio* or *hrëomorido* in MS. Paris 18237; and *chrëomurdo* in the last three manuscripts.

Let us admit he is right and that in all these manuscripts we have to do with a word of the same root which is found in the Anglo-Saxon *morðer* and means 'murder.' But the word 'murder' does not fit Ch. LV, because it is a matter of spoliation of a corpse, and not murder. But this does not embarrass Kern. He explains (cc. 471-472) that 'murdo' sometimes signified murder, but 'more often a heinous crime in general.' Let us grant that, too. Where do we arrive? The precise meaning of the Latin text dissolves in a rather vague Germanic term. If the reader of the manuscript did not understand Latin, how would a vague Germanic term help him? If he did understand Latin, the role of the term signifying a heinous crime is the more inconceivable.

Similar difficulties led J. Calmette to the following hypothesis:<sup>17</sup> Malberg glosses are not explanatory glosses, but rather *renvois* (references). He uses Kern's thesis that the term *Malberg* had the meaning of *forum* and, like the Spanish *fueros*, signified the code of the Franks worded in their own language. Calmette assures us: 'qu'il est très naturel de penser qu'en traduisant en latin (ce texte original) on ait eu l'idée de renvoyer au texte original pour chacun des articles, de façon à obtenir une correspondance permanente. Comme on ne numérotait pas les articles, on a pu procéder par incipit — soit du titre soit dans la teneur, — ce moyen de citer qui a été si employé et si longtemps.'

The last sentence is correct, with this limitation: only existing texts were quoted in this manner. Now, the existence of Frankish *fueros* remains to be proved. Besides, it seems to me inadmissible to fall back on a hypothesis to explain a detail, when this hypothesis has been rejected by the research aiming at the whole of *Lex Salica*. Now, modern scholarship long ago abandoned the hypothesis of the existence of a Germanic text of *Lex Salica*. Surmising a Germanic text, we rob the Latin text of its reason of being. Indeed, what sense could the Latin translation have, designed to serve a population that did not understand it? But we have already accepted so many impossible hypotheses that one more would not change anything. Let us accept the existence of a Germanic version and suppose that the Latin text is only a translation of it. I still do not understand Calmette's reasoning. Is it really quite natural to refer the reader to the original text to obtain a permanent correspondence? A 'permanent correspondence' is always given in the translation. There is no need of establishing it by references. And what is the meaning of the next sentence: 'since the articles were not numbered'? The articles of *Lex Salica* are all numbered!

Calmette probably does not mean a *translation*, but rather a *draft* and an *amplification* of the Germanic text in the Latin language. In this case, other questions arise. If it was an official rewrite, what is the sense of references to the original text which the new text would necessarily abrogate? Or was it a private compilation, something like a forgery? How could the compiler have the impudence to

<sup>17</sup> Bibliothèque de l'école des Chartes, Vol. 60 (1899), pp. 397-413.

refer the reader to the official text which would unveil his forgery? Clearly, it is difficult to share Calmette's view that his 'hypothesis is very plausible in itself.' It is curious that this hypothesis suffers a mortal blow from Calmette himself. He points out the strange fact, that the same gloss or reference — *leodardi* — is repeated in different titles. This observation forces Calmette to draw the following conclusion: 'Looking at the long series of paragraphs, it is quite clear that the word *leodardi* cannot possibly play the role of an explanatory gloss for each of them.' But it is also clear that the word *leodardi* can in no way play the role of a reference unless this reference should be destitute of any sense. Thus, the supposition of the existence of a Germanic text is not only impossible but also quite futile. It does not give us anything.

My explanation of the Malberg glosses seems to me the only possible one. The author of the forgery known under the name of *Lex Salica*, desiring to lend his word the patina of age, has inserted a series of foreign words to give the impression of having been conceived in a language familiar to the pretended authors of this law. These words have been taken partly from the Germanic language, and partly they are fantastic. Sometimes, lack of imagination forced the forger to repeat the same word several times. He was sure that nobody could detect him since nobody knew the language which Messrs Visogaste, Widogaste, and their colleagues used.<sup>18</sup> His efforts proves superfluous; *Lex Salica* was accepted as authentic without any accessory proofs; in the manuscript tradition the glosses were soon completely suppressed. But they did not fail to impress the erudite philologists of the nineteenth century who believed everything the forger wanted his contemporaries to believe; and by stretching and straining the form of the glosses a little, they have found a more or less plausible parallel word for each gloss in one of the Germanic languages. This is a posthumous success which the forger could not have expected.

Allow me to go back to the excellent essay of Krusch quoted above. Analyzing the false testament of St Remigius, he points out (p. 549) that this testament contains some sixty proper names, of which thirty-nine are found nowhere else. These proper names are of Germanic origin, or at least were supposed to be so; as such they have long attracted the interest of German philologists including J. Grimm who, according to Krusch, displayed all his knowledge to explain and to correct them. With convincing arguments Krusch proves that most of these names are fantastic. Moreover, as the imagination of the forger was not always at its height, he was forced to repeat analogous fictitious names. Krusch quotes the following passage as proof: '*Profuturo Leudocharium puerum trado; Profuturae dari iubeo Leudoveram.*' Krusch is surprised that he was the first to recognize these manifestly false names as forgeries.

<sup>18</sup> On the margin of F. 8. of *Ms. Paris Lat. 10911* containing the text of Ch. 9, *Liber Historiae Francorum*, the reader finds the following remark:

'Omnesque Romanos tunc, qui tunc in Gallia habitabant, exterminavit Clodoveus, ut unus vix potuisset inveniri. Et videntur Franci, illis temporibus linguam Romanum, qua usque hodie utuntur, ab illis Romanis qui ibi habitaverant, didicisse. *Que autem eis prius naturalis lingua fuerit, ignoratur in partibus istis.*' This is quoted by Krusch in Vol. VII of *Scriptores Rerum Merovingicarum*. (*M.G.H.* in 4°, p. 773.)

## XIII

My theory, however, would not be worth anything if it did not, better than the preceding theories, lend itself to giving an answer to the question which pre-occupied historians for more than a century: Which is the most ancient version of *Lex Salica*?

I have always maintained that this question was not asked with the necessary precision. There was a certain inclination to consider different versions as successive official drafts, each of which would necessarily supplant the preceding one. Baluze in dating the so-called *Emendata* as of the year 798, maintained this opinion vigorously. Baluze was wrong. The *Emendata* is not of 798, and it did not abrogate any other versions. The most ancient manuscripts of the different versions are contemporaneous.

This curious and most important fact should have been elucidated in the first place; it remains strange even if one attributes to different versions the dates acknowledged until now, i.e. the period from 750 to the beginning of the ninth century. Indeed, what might be the purpose of the scribe who, in the beginning of the ninth century — let us assume this date — recopied the most ancient pagan version of *Lex Salica* which, according to the opinion of so many scholars (including Krusch) is transmitted by *MS. Paris 4404*? Pardessus, the first to affirm the 'pagan' character of this version of *Lex Salica*, at the same time thought that *MS. 4404* offered official documents. 'It is evident' (*sic!*) — so he wrote (p. xi) — 'that this collection was a legal book, i.e., a gathering of laws for the use of a tribunal. We have many examples of books of such character.' Pardessus did not take into account the fact that the 'legal book' written in 803–813 would contain only the *Emendata* version of *Lex Salica*. The statement that we have 'many such books' is quite gratuitous. We have no certainty, even, that such books existed at all in the Carolingian era. Our manuscripts rather present books for reading and studying designed to be used by the clergy. None of these books is a 'legal book'; none gives us the least indication of having been used as such.

Thus, existence of different versions of *Lex Salica* in manuscripts of the same or almost the same era, it seems to me, should have long persuaded legal historians that these texts are not at all different official versions, their differences being due to the natural distortion suffered by every work in the manuscript tradition. In this case, the question of priority of such or such manuscript becomes a secondary and, at the same time, an almost insoluble question. *A priori*, a document of the sixth century is equally distorted in different manuscripts of the ninth century. One can become aware of the nuances while studying the general character of the manuscripts, and not only the text in question; that is to say, by studying the sources from which such or such manuscript drew its information and the manner in which it transmitted other equally ancient documents, the original of which is in our possession, etc . . .

In the case of *Lex Salica*, one more supposition could be used, adopted until now tacitly; namely: the presumption of the good faith of the scribes of those manuscripts that have reached us, as well as of all the intermediary ones.

On the basis of this presumption, though, no doubt is possible that only Pertz was right. The scribes of good faith could negligently or inadvertently shorten the text, but they never could amplify it. Pertz's adversaries failed to realize that by criticizing him, they inadvertently sacrificed their belief in the good faith of the manuscript tradition and approached the point of view I am defending in this essay.

The divergence of different texts of *Lex Salica* is rather too small to be easily explained by multiple transcriptions spreading over several centuries. But Pertz's point of view collides with group C.<sup>19</sup> The text of this group in itself does not differ appreciably from other texts. Its division into 99 or 100 titles does not prove manifest and willful alteration. In the manuscript tradition, the division into chapters is rarely conserved, especially when there is a great number of chapters. Rather often one may detect in quite different works a fusion of two chapters into one or a division of one chapter into many. But the text of this group has another trait indicating a deliberate character: an alteration of the sequence of chapters. Comparing the text of the *Emendata* with the text of group C (Hessels, pp. xxx ff.), we notice that Title 24 of the first corresponds to Titles 28-29 of the second; Title 26 to Title 30; while Title 25 corresponds to Title 63. The sequence of Titles 56-59 of the *Emendata* corresponds to the following series of the text in 99 titles: 89-18-17-76-75-77-90. Many analogous divergencies can be noted. Undoubtedly, change in the sequence of chapters in a text is as natural in the manuscript tradition as the alteration of numbers of the chapters. Often the scribe, after omitting a part of the text, realized his mistake and corrected it by altering the succession of the text. But the cases quoted above are too complicated to be explained as a result of the scribe's inadvertence.

Summing it up, the different versions of *Lex Salica* are neither successive official versions of *Lex Salica* nor natural distortions suffered by the text through a long manuscript tradition. Assuming good faith on the part of the compilers and scribes, we have come to a dead end. Therefore, this premise is false. *Lex Salica* is the work of forgers; the divergence of different texts is due to the desire to simulate a long manuscript tradition and an imposing number of different witnesses.

I am anxious to stress that this reasoning, which to me seems convincing, is based on the plain fact of co-existence of different versions of *Lex Salica*. It does not make any use of the manifold corrections of the dates of the different manuscripts or of the non-existence of Charlemagne's *Emendata*; nor does it make any use of the meaning of the term '*Lex Salica*,' or of any other observations which have built the foundation of my principal deduction.

Thus, different scholars defending the priority of the one or the other group of manuscripts were right in their arguments about details, but wrong in their arguments concerning *Lex Salica* as a whole. They were not aware that the question had not been posed with necessary vigor, nor did they realize that their partial theories did not exclude one another, but could very easily be made to

<sup>19</sup> 99 title text — see SPECULUM, XXXII, 2 (April 1947), p. 119.

harmonize in a theory of simultaneous different versions which, in its turn, should have necessarily made them realize that *Lex Salica* was a forgery.

The co-existence of different versions of *Lex Salica*, none claiming priority, is demonstrated by an interesting document known under the name of *Recapitulatio solidorum*. It is a summary of *Lex Salica* arranged on the basis of the amount of the fines, beginning with seven *denarii* and ending with 1800 *solidi*. This summary is preceded by a short preamble concerning different versions of *Lex Salica*: 'Sciendum est quod in quibusdam legis Salicis inveniuntur capitula principalia LXV; in quibusdam vero LXX, in quibusdam etiam paulo plus aut paulo minus . . .' (Hessels, p. 425). Nothing could better clarify the fact that the different versions represent the same text and that neither can claim to be the official version.

I am approaching the end of my essay; its purpose was not to present a theory that would answer all the questions raised by *Lex Salica*. My purpose was only to lay a foundation for a future fruitful study of this law.

I should like to discard one possible misunderstanding. When qualifying *Lex Salica* as a forgery, in no way do I want to affirm that it is a forgery 'from one end to another.' On the contrary, I am convinced that it also contains norms which were in force at the time of its compilation, e.g., the norm against which the formula of Marculf protested, as quoted above. But *Lex Salica* undoubtedly is false in its entirety, as a law promulgated by Clovis and his sons. It is false in its essential parts trying to make believe that penalties for murder and theft consisted of fines only.

On the other hand, I could not affirm that *Lex Salica* at any time played the role of a valid code or was designated for this role by the forgers. It seems to me, rather, that their aim was to extoll the splendors of *temporis acti* and to furnish accessory norms supplementing those in force.

The only commentary of *Lex Salica* transmitted to us is *Cap. 142*. I maintain that it is of the time of Charles the Bald; and that, as it mentions (Chapter 7) the absence of the emperor it cannot be prior to the year 875. It discusses only several titles of *Lex Salica* of no specific interest, namely: I; XI, 3; XIV, 11-12; XXVI, 9; XXVII, 3; XXXVII, 8; XLVI, 1 and XLVII, 4. Questions discussed in this commentary should have long been settled if it were the matter of a fundamental Frankish code. But the most characteristic chapter of this *capitulare* without doubt is Chapter 8:

De XLVI. capitulo, id est qui viduam in coniugium accipere vult, iudicaverunt omnes, ut non ita sicut in lege Salica scriptum est eam accipiat, sed cum parentorum consensu et voluntate, velut usque nunc antecessores eorum fecerunt, in coniugium sibi eam sumat.

This text gives a distinct impression of *Lex Salica* being a law recently discovered and not representing a valid code.

#### XIV

If the theory I have just advanced proves true, it will at the same time decide the fate of other *Volksrechte* in the first place, *Lex Ripuaria*, *Lex Baiuvariorum*, and *Lex Alamannorum*.

My first idea was to consider all these laws together in a single essay, since they usually are transmitted not separately but as a series including all or most of them. A separate study of each law tearing it out of its setting necessarily falsifies the results of the research and, in diverting history from the right way, forces it to make a sacrifice of the fundamental principles of the critical method. These principles demand the study of a source as a whole before it is used; they forbid tearing the source to pieces by separating parts which are intimately and organically tied together.

Many scholars studied the *Volksrechte*, but none of them took into consideration the fact that these laws are not transmitted by official documents, or even by copies of such, but only by manuals and legal compilations which have not been studied as a whole and whose very nature has never been stated.

Different scholars, failing to realize the nature of our sources, classified legal manuscripts arbitrarily, not caring for the place assigned to the same manuscripts by the editor of another law. They acted as if the texts evolved by themselves; as if different manuscripts were nothing but a place of fortuitous encounters where different laws had fleeting dates; and as if the repetition of the same series in different manuscripts was not a sufficient proof of a durable liaison between these laws.

I limit myself to two instances. *MS. Paris Lat. 10753* (of which I have spoken in my essay on *Capitularia*)<sup>20</sup> occupies a place of honor in the classification of manuscripts given by J. Merkel for *Lex Alamannorum* (*LL.*, III) and by R. Sohm for *Lex Ripuaria* (*LL.*, v). Both care very little about the fact that the same manuscripts transmits the so-called *Emendata*, i.e. the most recent version of *Lex Salica*, according to the judgment of these two scholars. These scholars not only gave no explanation of this strange fact; they did not even notice it, though this fact in itself would have been sufficient to shake the foundation of their classifications. Indeed, what motive could the compiler of *MS. 10753* have to collect the most ancient versions of *Lex Ripuaria* and *Lex Alamannorum*, and the most recent one of *Lex Salica* if it were not to please the two illustrious editors?

One more example: In R. Sohm's classification of the manuscripts of *Lex Ripuaria*, second place is given to *MS. Paris Lat. 4404*. In *Neue Forschungen* (quoted above) Krusch criticizes Sohm and demonstrates that the text of this law, as transmitted by the manuscript in question, is full of deficiencies and in no way can claim the place assigned it by Sohm. Sohm is more consistent than Krusch. Seduced by the preponderant role which this manuscript played among the scholars studying *Lex Salica*, Sohm assigned the second place in the tradition of *Lex Ripuaria* to *MS. 4404*. Sohm was consistent, though defending a bad case (*MS. 4404*), while Krusch was inconsistent, because he did not take into account the fact that his criticism of the text of *Lex Ripuaria* necessarily shattered the authority of the text of *Lex Salica* transmitted by the same manuscripts.

I want to emphasize that it is absolutely essential to study *Lex Salica* with the other *Volksrechte* in their setting in our manuscript tradition. However, I have to

<sup>20</sup> *Le Moyen Age*, 1941, fasc. I, p. 9.

postpone the publication of my essays on the other laws to a more favorable time. For the time being, I lack some books and — what is more important — certain manuscripts.

In completing this essay, I am anxious to show that in my principal thesis I am not without predecessors, of whom the first is Krusch himself. In Krusch's work, I was mainly influenced by his thesis that *Lex Ripuaria* is fundamentally only a redraft of *Lex Salica*.<sup>21</sup> I am certain that Krusch did not realize the logical implication of his statement. In my view, it contains the roots of my entire theory and is equivalent to the same. Indeed, if *Lex Ripuaria* is nothing but a disguised *Lex Salica*, is not this statement equivalent to the affirmation that *Lex Ripuaria* is a forgery? On the other hand, if *Lex Salica* is authentic, it would not be easy to find a valid motive explaining this disguise or even making it possible.

Konrad Beyerle, in the preface to his edition of *Lex Baiuvariorum*, came close to my theory in an even more evident manner than Krusch.<sup>22</sup> According to him, *Lex Baiuvariorum* is a compilation and as such a product of scientific work, but not a clear manifestation of the legislative initiative of the Bavarian people or its princes, i.e. the indigenous dukes or the Franc kings. Being essentially a compilation and as such surpassing all related contemporary laws, *Lex Baiuvariorum* can only be understood as a *literary work* (underscored by Beyerle).<sup>23</sup> Continuing his analysis, Beyerle points out that the scheme of *Lex Baiuvariorum* seems to be conceived like the Decalogue and he concludes that the origin of this law can be found only in ecclesiastical circles (pp. Lvi-Lvii).

Nevertheless, all these clear observations do not lead Beyerle to what seems to me the only logical conclusion. At decisive turning points, he disfigures his most certain statements by superfluous and misleading reservations. He allows himself to be carried away by badly founded hypotheses according to which *Lex Baiuvariorum* was composed in Bavaria in the first part of the eighth century, while all signs indicate it made its appearance in France in the middle of the ninth century. In a supreme effort at reconciling all his adversaries, he arrives at the following conclusion: '*Lex Baiuvariorum* is the work of the Pirmin monks of the Niederalteich monastery in South Bavaria. It was composed by the order of the Frank government, but with consent of the Bavarian ducal authorities and after a consultation with Bavarian judges.' The date of this *Lex* is 741-743 (p. Lxvi).

Thus, after having made splendid observations, Beyerle draws back from the only logical conclusion. His concessions, being based on vague hypotheses only, are moreover vain. They do not save the authenticity of the Bavarian law. Beyerle did not take into account that his compromise leaves the principal fact quite unexplained, namely that this law was pseudonym. Indeed, a complete agreement having been established between Pepin, Odilon, the Bavarian duke,

<sup>21</sup> 'Die *Lex Ripuaria* ist im Grunde nur eine neue Redaktion der *Lex Salica*' — N.F., p. 157.

<sup>22</sup> *Lex Baiuvariorum*, Lichtdruckwiedergabe der Ingolstadter Hs. — München, 1926.

<sup>23</sup> 'Kompilatorisch zu arbeiten, ist Gelehrtenart. . . Die Lb. ist darum keineswegs ein reiner Ausfluss des Gesetzgebungswillens des bayerischen Volkes oder der Herrscher über dasselbe, seien sie einheimische Herzöge oder Frankenkönige. Weil Kompilation in einem Masse, die alle verwandter Rechte der Zeit überragt, ist die Lb. nur zu begreifen als eine *literarische Schöpfung*' (p. xxxix).

the Church, and the Bavarian judges, why was this agreement not explicitly mentioned in the preamble of this law? And why omitting the names of Pepin and other parties to the concluded agreement, why does the prologue mention but the names of Merovingian kings beginning with Theodoric and ending with Dagobert? I repeat: an authentic law has no need of and cannot be preceded by a false prologue.

I do not want to enter into details of the Bavarian law, but merely quote the opening lines of this law, which seem to me to reveal convincingly its character and essential purpose:

Hoc decretum est apud regem et principes eius et apud cunctum populum christianum, qui infra regnum meruuungorum consistunt. Ut si quis liber persona voluerit et dederit res suas ad ecclesiam pro redemptione animae suae, licentiam habeat de portione sua postquam cum filiis suis partivit. Nullus eum prohibeat; *non rex*, non dux nec ulla persona habeat protestatem prohibendi ei . . . (*M.G.H. in 4<sup>o</sup>, LL. nat. Germ.* Vol. v, II, pp. 267-8).

Please note: the author of the law lets us believe that the king-legislator was not sure that either he or his successors would be always benevolent to the Church and would defend its rights. Therefore, the author makes the king take his precautions though they are totally futile. But by doing that, he betrays himself in a manner sufficiently manifest, as it seems to me.

A definitive study of *Lex Salica*, I repeat, can be accomplished only in connection with other *Volksrechte*. But the isolation of *Lex Salica* in this essay may have certain advantages. It will facilitate criticism — which I want to be severe and well founded. I hope this criticism will reveal the real foundations of the theory against which I am fighting. During my long study I have had the feeling of combatting a phantom. In my opinion, the dominant theory is based on suppositions which it considers evident, but which have nothing in common with evidence whatsoever. They are paradoxical — paradoxical to the point which hardly could be surpassed. Indeed, I cannot imagine anything more paradoxical than the affirmation according to which mediaeval legal history begins with protest and revolt against the Roman law, but ends with the renaissance of the latter law.

CENTRE NATIONAL DE LA RECHERCHE SCIENTIFIQUE,  
PARIS, FRANCE.

Attention is here called to two numerical errors in Part I of this essay, in *SPECULUM*, XX, 2 (April 1947), page 125. In line 12, for '(f. 107)' read '(f. 197)'; in line 29, for 'its 28 folios' read 'its 19 folios.'