By the end of the war the two men agreed on the need for the abdication of the emperor and of the crown prince to make peace negotiations possible.64

The years of wartime propaganda were difficult to erase. For those who, like Brentano, first had been carried away by patriotic feeling and later had remained silent for the sake of national unity, or found themselves simply unable to counteract the monstrous fabrications of the propaganda machine, it was now time to exert their efforts for the cause of reason.

But these efforts did not result in a turn to serious reflection and introspection. Rather than a genuine apologia, much of Brentano's remaining career reflects the effort to justify post facto his attitudes and conduct during the war and to attack the peace imposed upon his nation by the victors. The close correspondence between academic principles and political conduct asserted for the Katholikoszialisten by Schwabe thus does not apply to Lujo Brentano. Rather, he clearly was subject to the Zeitgeist and, notwithstanding the overall liberal thrust of his life and work, he forms a link in the continuum of German history outlined in Fritz Fischer's work.

DEBATE: DAVID ABRAHAM'S
THE COLLAPSE OF THE WEIMAR REPUBLIC

A Collapse in Weimar Scholarship

GERALD D. FELDMAN

The purpose of these remarks is to contribute to the exposure of the egregious errors, tendentious misconstruals, and outright inventions contained in David Abraham's Collapse of the Weimar Republic: Political Economy and Crisis (Princeton, 1981). Clearly, therefore, this is not a normal book review. I would not consider it appropriate to review this book in any case, since I read two versions of the manuscript for the Princeton University Press in 1979 and recommended its publication. I did so despite severe disagreements with the methodology and the argument. I assumed, however, that the scholarship on which the argument was based was sound and respectable and that the argument of the book would stimulate fruitful discussion. Since publication, the book has been widely reviewed, and while reviewers have divided over the theses of the book, most have praised its scholarship. The notable exception was Henry Ashby Turner, Jr., who must be credited with first drawing attention to the book's deplorable scholarship. Turner, as well as another scholar whose review is soon to appear, Ulrich Noeken, had the advantage of knowing many of the documents with which Abraham "worked," and thus had reason to be

1. I should point out, however, that my first reader's report contained eight single-spaced pages of critical commentary, and that I refused to recommend publication until substantial revisions were made. I must, therefore, take credit for the dubious achievement of having helped to make this book better organized and more plausible. Recently, when I inquired as to whether there was another reader, the Princeton University Press replied affirmatively on July 15, 1981, informing me that the other reader "strongly recommended publication with such comments as 'the author's new interpretation based upon economic sectors is highly original and very convincing. It is also thoroughly documented with new primary sources (documents drawn from economic organizations and associations). It is of enormous significance; I believe it is the most important book on 20th century Germany written in the last fifteen years.'"
suspicions. Most other reviewers, as was true in my own case, operated under the usual assumptions, namely, that a scholar with a B.A., M.A., and Ph.D. from a respected institution who was teaching at Princeton University, where high tuition is paid for instruction by carefully chosen persons of demonstrated competence, would operate in conformity with accepted scholarly standards. There was no reason to doubt the author's ability to copy correctly, understand the difference between a quotation and a paraphrase, understand what quotation marks actually mean, and comprehend the most elementary laws of historical proof and evidence. Above all, it was reasonable to assume that the author would respect those standards and was committed to promoting and preserving the integrity of the work of his colleagues and readers.

2. For Turner's review, see _The Political Science Quarterly_ (Winter 1981-1982): 739-41. For a sampling of more or less favorable reviews, see Harold James in the _Times Literary Supplement_ (July 31, 1981): 891; Elaine Spencer, _Business History Review_ 55 (1981): 609; Michael Schneider, _Archiv für Sozialgeschichte_ 22 (1982): 72-73; Tim Mason, _American Historical Review_ 87 (1982): 1222-23; Joel Rogers, _Journal of Economic History_ 42 (1982): 422-23; Michael Porter, _Contemporary Sociology_ 11 (1982): 588; Kurt Bechard, _Habitus Zeitgeschichte_ 256 (1983): 483-86; Thomas C. Christ, _Journal of Social History_ 16 (1982): 199; Robert G. Mueller, _Labor History_ 4 (1981): 623-26; Jonathan Mose, _Social History_ 8 (1984): 310-11. By no means typical was Rogers' praise of the book for being "thoroughly researched, with extensive and intelligent use of archival materials." The importance attached to Abraham's scholarship by a reviewer more sympathetic with Abraham's theoretical efforts is demonstrated by Mose: "This is a first-rate example of historical analysis combined with theoretical scholarship—both are necessary if one is to make sense of the archives." While James made some critical comments about the use of sources, and Bechard noted that there were "some excellent ideas," the first review to recognize that there was "a significant lapse from the care in note-taking or transcription" required in a work of this kind was Charles Maier's "The Vulnerabilities of Interwar Germany," _Journal of Modern History_ 56 (1984): 80-93, p. 93. Unfortunately, Maier did not have sufficient information at his disposal when he went on to subject the book's arguments to a devastating if precise criticism in a way that could be called a tortured review. A full critique will be found in a forthcoming article by Ulrich Nocken, "Weimar Geschichtswissenschaft" in the series _Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte_.

3. I am indebted to Dr. Nocken and Professor Bechard for a great deal of the information on which this article is based. Nocken, in particular, but devoted countless hours to the archives and libraries in his service to the integrity of our profession. I should make it clear, however, that I have personally read every document or source concerning which I criticize Abraham. I should also point out that Nocken, like the ingenious author of one of the most conservative reviews of Abraham's work, is a former student of mine. Let there be any misunderstandings, however, it was Nocken and his evidence which persuaded me that Turner had only revealed the "tip of the iceberg" and that a full exposure of the truth can only take the time and energy that this required.

4. Dozens of scholars have had the privilege and pleasure of working at the Archives of the Gutschmidarchiv in Oelsnitz, from which many of the most extreme examples of Abraham's misuse of sources come. That archive does not provide its users with copies, but it does provide visitors with a table and a set to preserve users to type their notes.

5. It is difficult to understand how Charles Maier, who so splendidly worked upon the most careful integration of detailed archival research with structural arguments, can support Abraham's "structural" argument is not to be discredited by his "errors." This is all the more the case since Maier's review is largely a critique of Abraham for presenting an argument that does not conform to the facts. Maier, _Journal of Modern History_ 56 (1984): 91-95.
ful to provide a concentrated and concrete illustration of why this exercise is necessary and of what is involved.

In order to understand Abraham's modus operandi, it is useful to begin with a careful consideration of a purported "Christmas 1930 exchange" between Paul Reusch, General Director of a major industrial enterprise with headquarters in the Ruhr, the Gutlohnungshütte, and his representative in Berlin, Martin Blank. Since this alleged "exchange" figured prominently in the charges leveled against Abraham by Turner in the American Historical Review, it will also serve to shed some light on the character of Abraham's self-defense.6

The supposed "Christmas 1930 exchange" appears not once but twice in the Abraham book, on pages 164-65 and 316. The relevant paragraphs and footnotes are quoted below. I have italicized every error, misquotation, and invention.7

Reusch's Berlin representative, Martin Blank, reported in December 1930 that "the time for a so-called bourgeois concentration is past, . . . We must maintain and build the unity of the Hitler-Hugenberg opposition and pick a leader from it. We must be much more cautious in supporting Bruning and his half-measures." For his part, however, Reusch preferred an attempt to "crystallize the bourgeois right and the NSDAP into one." He and several colleagues were particularly impressed with Goebbels's statement that "wage cuts for the sake of reputations and the current system are unacceptable, but in a national system they would be acceptable." Reusch was further encouraged by Strasser's remark that "industry has exerted itself too much for exports; the economy must be switched over from the world to the domestic market. That would mean the reintegration of both agriculture and industry. That is a feasible form of austerity. It would be too much to infer from this a switch to a strategy based on the Nazis; Reusch considered the report so controversial that he asked Blank to destroy all his copies.11 Nevertheless, it does help demonstrate that the split between the liberal (pro-Bruning) and national (pro-Hitler-Hugenberg) factions of industry occurred almost as soon as the outlines of Bruning's program became clear.

6 Blank to Reusch, 29 Dec. 1930; Reusch's comments on Goebbels and Strasser and his instructions to Blank, 2 Jan. 1931; HA GHH/Nachlass Reusch 400 101 293/11. (The colleagues who noted Goebbels's statement are left unnamed.) It is not our intention here to detail the process by which industrialists developed their contacts with and affinities for the Nazis. [pp. 164-65] Those who were initially not of the Weimar system would save those whose dominance the system had threatened. As noted earlier, Reusch had begun to despair of a bourgeois concentration as early as Christmas 1930 and suggested instead off-the-record funding directed toward "crystallizing the bourgeois right and the NSDAP into one." The primary function of unifying the Bürgermänner rightward would be to "bring the NSDAP away from its economic utopias and make it capable of governing." At that time he was in a minority; a year later that was no longer so . . .

11 The Christmas 1930 exchange between Blank and Reusch in HA GHH/ Nachlass Reusch 400 101 293/11 . . . [p. 316]

In his letter to the AHR, Henry Turner charged that the two documents, namely, the letter of Martin Blank to Paul Reusch of December 20 and the "instructions" from Reusch to Blank of January 2, did not exist. In his reply, Abraham reported that he went back to the archive of the Gutlohnungshütte and found the documents, but that he had misidentified one of the persons involved. As it turned out, according to Abraham, it was Edgar Jung, "an adjunct employee of Reusch's in Munich," not Blank, who was the recipient of the instructions and who was also the "more likely" author of the report. Abraham suggested that the unsigned report, which bears only the pencilled date "Dec. 1930," might have been sent earlier than December 20, and said that there was "some scholarly disagreement" about the authorship of the report. Abraham thus declared himself cleared of the charge of inventing documents, and a subsequent reviewer accepted this extremely misleading contention.8

The explanation Abraham gave in the AHR perpetuates his claim that two documents which have nothing whatever to do with one another except to share a common lodging in the archival folder in question are part of an "exchange." Even the most superficial reading of the letter from Reusch to Jung of January 2, 1931, should make this clear. The letter was specifically designated by Reusch as his reply to a letter sent to him by Jung on December 31, 1930, and to another document, sent on or about December 23, which Reusch was returning with

explicit advice that it be destroyed. More important, however, is that the Reusch letter refers to the content of a subsection of Jung’s document of December 31 ("IV-3") which simply is not to be found in the anonymous report which Abraham now presents as the other half of the “Christmas 1930 exchange.” If Abraham had admitted in the AHR that the citations questioned by Turner were a result of having confused Blank with Jung as the recipient of the January 2 letter and of mistakenly connecting that letter with an anonymous report, which he misattributed to Blank and mistated so as to present the readers of his book with a non-existent “Christmas 1930 exchange” between Reusch and Blank, he would have exonerated himself of the charge of inventing the documents themselves while clarifying why it was so difficult to find them. That would also have explained why the archivist of the Gustav Kiyhannshütte had been unable to find, at Turner’s request, the document cited by Abraham. But instead of following that course, Abraham, after having obtained photocopies of the two documents, persisted in linking them as two halves of an exchange. He also further misidentifies Edgar Jung, a well-known conservative intellectual of the period, as an “adjunct employee” of Reusch, a mischaracterization that awaken a false impression of a formal tie between the two men. It is difficult to believe that Abraham went all the way back to Germany to find the documents and yet never bothered to reread them carefully and realize that they were unrelated.

Such doubts are reinforced by Abraham’s treatment of the other document in question, the anonymous December 31 report. Given the number of errors to which Abraham admitted in his AHR letter, it is difficult to see why he had to spin a tale about “scholarly disagreement” connected with the authorship of this report or about the possibility that he may have mistated it, originally claiming that it had been sent on December 29 but now arguing in the AHR that it might have been sent a week earlier. The author of the report has been identified with reasonable certainty as the right-wing, pro-industrialist journalist, August Heinrichsauer, by Reinhard Neebe. The date when the Heinrichsauer report was sent to Reusch is uncertain, but there is incontro-

vertible evidence cited by Neebe that Reusch sent the document to Reichsbank President Hans Luther on December 9 with a request that it be returned, and that Luther did return it on December 11. Abraham reviewed the Neebe book in the December 1982 AHR, and he cites one of Neebe’s footnotes in his October 1983 AHR letter which is only nine pages away from the footnotes dealing with the Heinrichsauer report, which then refers back to Neebe’s two-and-a-half page discussion of Heinrichsauer and lengthy citations from the anonymous document in question. For there to have been scholarly disagreement, scholars must have at some point disagreed and, to my knowledge, no one, including Abraham, has contested the attribution of the report to Heinrichsauer. I find it impossible to believe that Abraham was unaware of Neebe’s identification of Heinrichsauer as the true author of the report in question when he wrote his response for the AHR.

Turning now from the misidentifications, false datings, false attributions, and false connections discussed above, let us consider Abraham’s actual handling of the Reusch letter to Jung and the report attributed by Neebe to Heinrichsauer. On pages 162 and 316, Abraham misattributes Reusch’s letter to Jung by leaving out the crucial word “nach.” Reusch wrote that he did “nach” wish to create a point of crystallization between the bourgeois right and the NSDAP, thereby adopting a position which Reusch converts into a positive one. Abraham’s other “quotation” from this letter is also extremely reminiscent. While Reusch says that the NSDAP should be influenced “to direct their economic goals along reasonable paths,” he says nothing whatever about the NSDAP being made capable of governing. Insofar as the quotation from the Heinrichsauer report is concerned, Abraham puts words into the mouth of Goebbels, of all people. Nowhere in the document does Goebbels say that wage cuts would be acceptable in a “national system.” Here, then, we have good illustrations of the deficiencies which characterize so much of this book: misquotation, tendentious misconstrual, and outright invention of both archival citations and the content of documents.

11. Ibid., 231-19.
Before going on to other examples, it is important to note that these lapses from acceptable scholarly conduct directly affect the author's presentation and argument. Although Abraham claims in his book (page 165, n. 141) that he is not trying "to detail the process by which the industrialists developed their contacts with and affinities for the Nazis," he certainly attempts to point up relevant and significant aspects of that process and to portray Reusch as an early example of industrialists' support of the Nazis. The documents in question, however, prove nothing of the sort and could be used to prove the reverse. Furthermore, there is nothing in the documents to support the contention that Reusch was impressed by Goebbels’ supposed statement on wage cuts, while Abraham’s misidentification of the report makes his dramatic presentation of Reusch’s effort to have Blank destroy all the copies utterly ludicrous.

II

What has been described above is typical of the manner in which Abraham handles archival materials. A full treatment of the evidence would take up a whole issue of this journal. Nevertheless, it is important to provide a convincing number and variety of illustrations. What follows below is a selection of examples of Abraham’s quotations and paraphrases from primary and secondary sources. It should be noted that Abraham entertained the notion that it was proper to indent paraphrases in the manner of lengthy quotations without indicating that they were paraphrases. To assist the reader here, I shall distinguish between Abraham’s invention of quotations and his tendentious misconstruals in paraphrases. As in the previous illustrations, errors, misquotations, and invented words, sentences and phrases will be italicized, while the missing words unmarked by ellipses will be noted by an “xxx.” It should be clear, however, that in many instances I am being extremely generous in accepting portions of the quotations as genuine since many of the translations are also so loose as to be technically unacceptable.

1. On page 91, Abraham quotes from a published work by Bernhard Harms, a Kiel professor and the head of the Institut für Weltwirtschaft und

Seeverkehr, who is misidentified as the director of the Friedrich List Gesellschaft,16 as follows:

The peasantry is the chief pillar of that vital Mittelstand which is a prerequisite for the continued health of a people and its state... Only the national market can secure it. The free-trade era is over and integration into world trade is secondary. The state must now come to the active assistance of all of Landwirtschaft.

Not only is most of this quotation an invention, but it is also a gross distortion of Harms’s views. Abraham cites Harms to illustrate the persistent commitment of the “scientific-academic community... to support the agrarian ideology of a protected and united rural sector.” In his article, however, Harms argues that Germany must recognize her dependence on industrial exports.

2. On page 167, Abraham has the following two sentences, one with a quotation, the other with an interpretation which follows from it:

Ruhr industrialists informed Brüning that they were "no longer prepared to tolerate government inaction... even if it means calling Parliament back into session." Their anxieties over the 107-man Nazi Reichstag delegation had apparently ebbed.17

The interpretive sentence makes no sense without the inverted words in the quotation.

3. On pages 168-69, Abraham makes much of how the Ruhr industrialists attacked the policies of the liberal industrialists in the Reich Association of German Industry "primarily by assailing the RDI’s chief administrative officer, Kastl." Thus, the following sentence with quotation appears in the text:

Intimately involved in the export-labor coalition, he [i.e., Kastl-GDF] was now accused of "cowardice in the struggle with the unions."

Lost the opportunity be lost to add a little invention to the tendentious presentation of the quote, however, Abraham provides a longer quotation in the footnote from a letter from Reusch to Kastl:


17. Many of the documents in question have now been reprinted in Ilse Mauer and Udo Wengst, Politik und Wirtschaft in der Krise 1930-1932: Quellen zur Ara Brüning, 2 vols., Quellen zur Geschichte des Parlamentarismus und der politischen Parteien, 31 ser.; Die Weimarer Republik, vol. 4, pts. 1, 2 (Düsseldor, 1950). This document may be found in vol. 1, 621-22.
"Under your leadership industry has until now been too cowardly to take up the struggle with the unions in all its sharpness..."

There is no evidence whatever that Reusch was launching a personal attack on Kastl here, or anywhere else.

4. On page 170, discussing the relations between Brüning and industry, Abraham presents a totally invented quotation:

Brüning's government did not enjoy a popular base substantial enough to allow him to divide and conquer the industrialists. Martin Blank remarked that "the chancellor demonstrates a certain petulance toward western big industry and is always praising small, flexible entrepreneurs. Perhaps that is why the RDI used to support him more than we did, but that does not work any more, except maybe with Kastl."

As is often the case, Abraham does not provide a full citation, so that the date is missing, but internal evidence suggests that the "quotations" comes from a report by Blank of October 14, 1931. Abraham's treatment of that complex document testifies not only to his disdain for quotation marks but also to his complete insensitivity to and lack of interest in what actually took place in the past.

5. For a change of pace and location, let us now consider the novelties provided by Abraham's handling of a printed source, an article he ascribes to Max Schlenker of the Langmannverein on page 225. In Abraham's view, this article represented a "major step" in trying to form a united front between the industrialist "fractions" to conciliate agriculture. For reasons to be explained below, I am capitalizing the opening portion of the quotation:

WE MUST TRANSCEND NOT ONLY SLOGANS LIKE "AUTARKY VERSUS EXPORTS" BUT ALSO THAT OF "FREE ECONOMY VERSUS PLANNED ECONOMY." . . . The market for agricultural products must be thoroughly organized and the experience of industry in this area must be drawn upon and applied rationally and reasonably to agricultural relations. Those cartel-type measures industry has utilized XXX can be effective for limiting agricultural imports XXX without constantly bringing us into conflict with our best foreign trading partners. In addition, they would enable agriculture to satisfy its needs in cooperation with rather than in opposition to industry. 20

The first sentence of this quotation stems not from Schlenker but from an agrarian leader, von Knebel-Döberitz, whose article followed that of Schlenker. Whether this reduces the "major" character of the "step" being taken by Schlenker at this point might be a reasonable question. The concluding invented sentence is not incongruous with the argument, but the impression created by placing a fabricated statement of the argument in the mouth of an historical actor should be obvious. Quotation is a powerful tool of historical argument, and its abuse constitutes an egregious and inexcusable manipulation of the reader and violation of the trust which is placed in the author.

6. What should be emphasized most, however, is that Abraham's use of quotation and paraphrase is marred not simply by sloppiness or misreading but by consistently tendentious presentation. It serves to exaggerate the evil role played by industry, to overdramatize its power and self-assurance, and to make arguments far beyond what the evidence would allow. Thus, on page 295 Abraham illustrates the inability of Brüning to persuade industrialists to enter his cabinet in October 1931 with a purported quotation from a letter of Paul Silverberg to Gustav Krupp von Bohlen und Halbach:

Brüning no longer enjoys the confidence or even neutrality of western [heavy] industry which considers him too bound to this political system to make the kinds of policy changes needed, even if he himself really wants them, which is also not entirely clear. He could not or would not tell me and Vögler what he really wants. XXX Up to now the government's policies have been hostile to business, despite Brüning's denials . . . If Brüning wants me, he will have to take Schmitz, Vögler, and Warnholtz as well and leave all economic, financial, and social-welfare policies to us.

This "quotations" should read something like this:

As a conclusion to the discussion, I [Silverberg-GDF] declared [to Brüning-GDF], that I did not have the conviction of having my industrial friends, especially western industry, behind me [and] that explicitly did not have the conviction that the Reich Chancellor can bring about what he desires, namely at least the neutrality of western industry. Furthermore, I also did not feel strong enough to enter the government alone . . . [At a discussion the next day, at which Vögler was present, the negotiations continued-GDF.] I [Silverberg-GDF] added that the general view was that the government was now following an explicitly anti-business policy. The Reich Chancellor energetically denied this . . . We were in agreement that the matter would have to be thoroughly discussed in our circles, but that if the general situation required, that we would only enter the government together, i.e., Herr Schmitz, Herr Vögler and I, in order to have a sufficiently strong position.
against purely political efforts with respect to economic, financial and social policy matters. These last agreements were confidential and were not communicated by us to the Chancellor.21

7. On page 297, Abraham discusses a meeting between the RDI and the Green Front in March 1930, and has representatives of the former organization saying with respect to the depaertamentalization of economic affairs:

that goes for the entire area of economic and finance policy... The revolution toward Parliament is general; xxx we must get out from under parliamentary bumbling and take advantage of the weakness of the unions.22

In the document, the two issues are not related to one another, and the RDI representatives express their belief in the possibility of negotiating with the unions.

8. A good illustration of Abraham’s indifference to what documents actually say and his fanatic attachment to his preconceived notions is to be found on page 312, where an entirely misleading interpretation is given to a relatively accurate paraphrase of a letter from Reusch to the DVP politician Dingeldey that is nevertheless put in quotation marks:

By early 1932 Dingeldey was reduced to pleading for money at the doorsteps of Ruhr industrialists. Their preconditions remained the same: purity in representing the interests of the entrepreneur and the unity of all those prepared to do so, including the NSDAP. Or, as Reusch told the hapless party leader, “I shall provide no money until there is serious, wholehearted movement toward unity among all parties between the Zentrum and the NSDAP, xxx You, among others, have promised this before but have not delivered.”23

In reality, Reusch had no intention of including either the Center or the NSDAP in the single large bourgeois party for which he was calling.

9. On page 319, Abraham uses a quotation to produce pure fiction with respect to the Stahlhelm:

Reusch recalled that during the 1928 Ruhr lockouts “they [the Stahlhelm-GDF] performed very good services” as strikebreakers and thugs and “should be kept active, not only for these purposes but as an alternative to the unions for propaganda and organization.”24

Reusch was indeed anxious to regain influence over the Stahlhelm, which he feared had fallen sway to Hugenberg, but there scarcely was any need to employ “strikebreakers” in a lockout and, insofar as he remembered what the Stahlhelm was up to in 1928, it could only have been their supplying—in opposition to the interests of the employers—their locked out members with food.25

10. Finally, let us conclude this collection of examples with an illustration of Abraham’s inventive misuse of documents. On page 320, he sets forth three passages, attributed to an exchange of letters between Hjalmar Schacht and Reusch in the spring of 1932. These passages are indented, one of them containing an ellipsis and one containing words in brackets. In short, they appear in a form that leads the reader to assume they are direct quotations. Here, with the fabricated portions in italics, is what appears on that page with the object of providing evidence for what Abraham considers characteristic of the manner in which the decisive German business leaders were coming to the view that the Nazis had to be brought into the government:

The Nazis are not to be circumvented; more than that, they are the positive force. We should contribute to them and their efforts and assist them in altering some of the utopian aspects of their economic policies. [Schacht]

After a productive two hour talk with Hitler yesterday, I fully and completely agree with your suggestion... I find myself in complete sympathy with the National Socialists, though they are a bit tactless. [Reusch]

[I have begun] a collection for the purpose of supporting them and enlightening them on economic issues. [Schacht]26

Is more tendentious misconstrual and misleading use of documentary evidence possible, and how can Abraham obfuscate the issue, as he does in his AHR reply, by characterizing them as “deficiencies”? The correspondence between Schacht and Reusch was concerned with the setting up of a bureau of economic advisers to try to influence Nazi economic


22. Ibid., 17: 70.

23. Reusch to Dingeldey, Jan. 30, 1932, HA/GFFH, Nr. 400101291/12.


22. Ibid., 17: 70.

23. Reusch to Dingeldey, Jan. 30, 1932, HA/GFFH, Nr. 400101291/12.


26. The three letters in question are now in print. Two of them, Schacht to Reusch, March 18, 1932 and Reusch to Schacht, March 20, 1932, are to be found in Dirk Stegmann, “Zum Verhältnis von Gewerkschaft und Nationalsozialismus, 1930-1931,” Archiv für Sozialgeschichte 13 (1973): 450-52. The third, Schacht to Reusch, June 6, 1932, is reprinted in Kurt Koszyk, “Paul Reusch und die ‘Münchener Neuesten Nachrichten,’” Vierteljahreshefte für Zeitgeschichte 20 (1972): 99-100. It should be noted that the alleged quotation on page 321 of Abraham from a letter by Springorum to Freiherr von Wil-
policy. It reflected their belief that one had to deal with the Nazis in the light of the election results. This hardly was a praiseworthy policy, but it is very different from an enthusiastic or collaborative one, and there is no more justification for misrepresenting the words of Schacht and Reuss than there would be for misrepresenting the words of Goebbels, Stalin, Gramsci, Poulantzas, Marcuse, or any other historical actor, thinker, or even historian.

III

Something must be said about the way Abraham deals with the historical work of others, since they might object to having their work distorted and abused to buttress Abraham’s interpretations. Nowhere in the article I wrote for this journal on “Big Business and the Kapp Putsch” do I argue, as Abraham claims on page 126, that Albert Vöge1, Hugo Stinnes, Emil Kirdorf, and other steel industrialists “supported Kapp.” I say that “It is difficult to tell exactly how Stinnes felt about the Kapp Putsch” and then, in a style so unfamiliar to Abraham and so necessary in dealing with the muddled approach of German industrialists to politics, I discuss some aspects of the question as I can on the basis of the evidence and come to what I consider a balanced conclusion.

27. Gerald D. Feldman, “Big Business and the Kapp Putsch,” Central European History 4 (1971): 99-130, p. 108. It is now less difficult. My recent research shows that Stinnes opposed the Kapp Putsch along with most of his colleagues in heavy industry.

To quote the confident report of the April 13, 1920, meeting of the directors of the RHE to be found in the Stinnes papers: “Here Stinnes then reported that the circles represented by him along with the representatives of the iron and steel industry had already taken a position in the Kapp Putsch on March 13. It was completely clear that time did not suffice for the demonstration of the Kapp Putsch and he was opposed to foreign policy reasons.” Nevertheless, the Reichswehrlog so fell in a similar situation, which could easily occur again, as in this case. Under no circumstances ought Germany to come again under military rule, which it would inevitably lead to civil war. In economic life one must come to terms with the present constitutional conditions, for Germany’s economic life can only be built up again in cooperation with the democratic states of the West. Germany, however, will never come to a tolerable relationship with England and America if pre-revolutionary conditions are taken to be an option here.” Bericht über die vertrauenswürdige Besprechung der durch den Kapp-Putsch geschehenen Lage der Staatsverwaltung des Reichsverbandes der Deutschen Industrie am Dienstag den 11. April 1920, veranstaltet am Hotel Esplanade, Berlin, Bollwerkstrasse, Archiv für Christlich-Demokratische Politik der Konrad-Adenauer-Stiftung, Band 1-240 (Nachlasse Harry Stinnes), Nr. 027/1. Unless Stinnes was a “dust,” chemical industrialist, this renders much nonsense of Abraham’s attempt, totally unsupported by the evidence I present, to make industrialist differences over the Kapp Putsch an “indication” of alleged “things to come” in later industrialist politics.

28. Whether one agrees with Neebe’s argument that the German industrialists were “premodern” in their political outlook, Abraham’s reference to this argument as a “bit of a cop-out” and his charge that Neebe’s effort to make up for the “empirical defect” is “not fruitful” and “is a bit like fishing with a barn in place of a fishnet” is more than a bit ironic in the context of this discussion. See Abraham in American Historical Review 87 (1982): 1414-15.


30. Abraham, Collage, 132-133.

31. Dick Geary, “The Industrial Elite and the Nazis in the Weimar Republic,” in Peter D. Stechura, ed., The Nazi Machtergreifung (London, 1983), 93. He also picks up on one of Abraham’s misleading “quotations” on page 97, but he wisely sides with Bernd Weisbrod’s interpretation of the 1926 change in the RDI leadership.

which follow support Abraham's contention that there was a truly contested election for the RDI presidency. Weisbrod shows that Vögele was unwilling to take the position, that Borsig turned it down after being offered it, and that Duksgub was a compromise candidate chosen by mutual agreement. As Weisbrod's careful analysis, which is a model of its kind, demonstrates, the election was by no means a clear-cut victory for any "fraction." This last point, however, is a true scholarly issue which can be debated. What cannot be debated, at least until evidence to the contrary is discovered, is such a simple matter of fact as whether or not there really was a contested election with victors and vanquished. There is no excuse for Abraham's simply disregarding the facts presented by his own source, Weisbrod, in this regard.

Nor is there any excuse for the amount of misinformation of every kind presented in this book. From the perspective of Abraham's argument, it may be regrettable that Otto Wolff chose at once to make his fortune in heavy industry and to behave like a member of the "dynamic export fraction." But to identify him, along with Carl Friedrich von Siemens, as "executives" of the Deutsche Bank, allegedly allied with the "dynamic export fraction," because they were members of the Supervisory Board (Aufsichtsrat) of the bank, is about as absurd as arguing that Werner Kehl, the one properly identified Deutsche Bank director, was a textile industry executive because he sat on the supervisory board of a textile firm. If Abraham wishes to show connections between those holding power in the RDI and certain industrial "fractions," then he had better get his facts straight. e.g., that Hermann Büchel was not, as Abraham blithely claims, the assistant to the man who became his successor as Business Manager of the RDI in 1925, Kastl, and that Büchel did not assume a directorship with the German General Electric Company until 1929. These errors, like Abraham's inability to sort out exactly when various persons held the posts of economics minister and finance minister, would be trivial were it not for the importance which Abraham attaches to their connections. They are, in any case, very odd for someone studying the history of German political economy.

Finally, some attention must be paid to the bizarre treatment of statistics in this book. The statistics are of central importance for the book since they are used to develop the categories on which Abraham's interpretation rests. Yet the value and reliability of many of his statistics prove under scrutiny no higher than his use of documents and secondary works. It is difficult to tell how Abraham could make such a gross error as to estimate the increase in German industrial production between 1927 and 1928 at 15 percent, when the real 3 percent might have been a sufficient, if somewhat less extraordinary basis for his argument that "industrial interests could afford to govern with the working class and accede to some of its demands." It is utterly impossible, however, to understand what Abraham was up to in his use of the statistical tables in Walther G. Hoffmann's standard work, Das Wachstum der Deutschen Wirtschaft. Table 9 on page 81, which contains agricultural price indices designed to illustrate points the author is trying to make about peasant political behavior rounds off numbers in such a way as to produce false averages and mistranslates "other animal products" as "dairy" products. The prices of the latter column thus include wool and skins as well as milk products. On page 90, Table 12, the line with figures for meat import volume indices is one year off. Table 15 on page 144, which is meant to assist in illustrating the relationship between the political attitude of various industrial groups and their levels of production contains mind-boggling errors aside from the failure to mention that Hoffmann includes craftswork as well as industrial production in his figures. Hoffmann's figures for "leather goods and clothing" are placed in a column labelled "textiles and clothing." Hoffmann's statistics on textiles, by contrast, do not appear. His column for "metals production" is entitled "iron and steel" by Abraham. The statistics Abraham gives for lignite production simply cannot be found in Hoffmann. To cap things off, Abraham places the column "all mining" among a group of "stagnant" industries while placing "lignite," which is of course mined, in the category of "expansive industries." It would take considerable labor to figure out how Abraham derived many of the

13. Ibid., 217-45.
15. Abraham, Collage, 411; Wenzel, Wirtschaftsführer, 316.
16. Abraham, Collage, 159, 187, 197, 166, 171. Paul Molsenbauer was finance minister from March to June 1920; Hermann Wartnabold was economics minister from October 1911 to May 1922, and again under von Papen and Schleicher.

40. Ibid., 142.
41. Ibid., 342-43, 392-93.
figures on page 143, Table 16 on employment in the major industries, and I think that the effort exceeds my responsibility here. By now, it should be clear that the material in this book is sufficiently lacking in credibility as to make it not simply useless to scholars but dangerous if quoted or believed.

* * *

Some may find these remarks overly harsh, but I stand by the tone as well as the content of this discussion. It is proper and necessary to be angry at the violation of the basic standards of our profession, or any respectable profession, as well as of the principles of trust on which it is founded. It is appropriate also to condemn an effort to cover up the truth. It was nothing short of mendacious for David Abraham to suggest in his reply to Henry Turner in the AHR that there is "a Methodenstreit lurking here between what one might call positivists and conceptualists," and to suggest that Turner was "exploiting a few errors in an attempt to discredit a detailed and comprehensive argument without confronting it" as well as to impute that Turner's critical stance toward Marxist scholarship combined with his attack on Abraham "do not inspire confidence in his tolerance for plurality of scholarly traditions." Abraham's performance is in no scholarly tradition known to me. Historians do not have scholarly discussions over the presence of the word "not" in a quotation. I share Professor Mason's view that "proven errors of this kind have done a lot to discredit Marxist and neo-Marxist interpretations of fascism or Nazism." The work of Mason himself and of Marxist scholars working in the field of Weimar's political economy like the late Manfred Nussbaum will insure that Marxist scholarship will continue to be taken seriously. The work of Abraham is not serious Marxist scholarship and is, indeed, not scholarship at all. It abuses our trust by feeding us a steady stream of misinformation; it damages the name of American historical scholarship abroad; it sows

distrust among archives and archivists who have generously and democratically opened their materials for the discovery of historical truth even if that truth is not flattering; it undermines the integrity of our profession. In short, whatever the motives and whatever the reasons for what Abraham has done, his performance is inexcusable and intolerable. It is the responsibility of the publisher to cease marketing a work that is so filled with error, and it is the responsibility of the profession to make clear that it will not condone or permit this kind of performance by anyone now or in the future.

42. Compare with ibid., 195, 198.
44. Ibid., 1144.
45. In this context I must express some dismay over the political tone of Henry Kandler's review of Abraham in the Frankfurter Allgemeine Zeitung (March 19, 1983). Nothing whatever is added to scholarly discussion or to his well-founded criticisms of the Abraham book by arguing that "empirical history does not stand a chance against Marxist scholarship." There is plenty of good Marxist and neo-Marxist history around.
A Reply to Gerald Feldman

DAVID ABRAHAM

FOR more than a year and a half Gerald Feldman has waged an unrelenting campaign against my work and character. By his own declaration, the object of this campaign has been to deny me employment as a professional historian, and to establish within the historical profession that my work is a fraud, and that I am a liar. Hitherto Professor Feldman has used private correspondence, countless phone calls, irregularly circulated "open" letters, and lapel-grabbing confrontations with interested (and some not so interested) colleagues to advance these ends. After much of that, he has availed himself of the services of a scholarly journal. I am pleased with his decision to do so, and grateful to the journal's editor for the opportunity given me to respond. But there is no escaping history. And to understand the present exchange, some history is required.

As many readers are doubtless aware, my book, *The Collapse of the Weimar Republic: Political Economy and Crisis* (Princeton, 1981) has already been the subject of an exchange between Professors Henry A. Turner, T. W. Mason, and myself in the October 1983 *American Historical Review.* Before that exchange appeared, Feldman's attacks on me were largely limited to abusive personal correspondence, and one letter he wrote me in May 1983 to my publisher, Princeton University Press. In that letter, Feldman sided with Turner—before seeing my reply to his charges—in the dispute later aired in the *AHR,* and withdrew the endorsement he had earlier given my book as a reader for the Press.

With the publication of the *AHR* exchange, however, Feldman's engagement in my case took on a qualitatively new dimension. Immedi-

2. April 29, 1983, May 17, 1983, March 14, 1984, author file. As with the circular letters referred to here, they are on file with the editor of this journal and are available from me.
3. May 26, 1983. In a letter to the Press of March 27, 1984 he made veiled threats if my book was not withdrawn from the market, catalogue, etc., in *ibid.*

4. Widely available, including in *ibid.*
5. In the case of Texas the publication and application materials "requested" to me after my candidacy was dropped included Feldman's "Dear Colleagues" letter of November 28. At Tel Aviv University in late February-early March, Feldman, visiting the university, told the department which had earlier extended me a one-year visiting mutual-look-see-leave offer with a likely-to-be permanent arrangement thereafter that my work is fraudulent and I am a liar. (Report of department members to the author, further documentation available.) Feldman went on to say that a former student of his had "democratized" this. Negotiations regarding a position, which had been taking place for several months, were then not pursued further by the department. At Catholic University documentation on Feldman's intermeddling is more extensive. After an initial interview and expression of interest, Catholic University interviewed me a second time on January 24, 1984, and the history department search committee expressed enthusiasm about my candidacy. Shortly after my second interview, I received numerous phone calls from various department members, including members not on the search committee, encouraging me to accept an offer. Described as "imminent," it came to Washington to explore housing possibilities, etc. At the beginning of February, the department recommended to the dean's committee on tenure and promotion at Catholic that I be hired, and at the end of the month the dean's committee approved this recommendation, and passed it on to the dean. In the meantime, however, Feldman had learned of Catholic's interest in me. Beginning on February 13, Feldman initiated a series of phone calls to search committee and other department members, and mobilized other German historians to do the same to protect my prospective hiring. In conversations with Catholic University's history department members, Feldman declared, in effect, that hiring me would be "undemocratic" for the University, and that in fact I was "not worthy of teaching in any American university." After all, Feldman said at one point, "what can you think of a book you leave out a 'not' from a quotation." After several weeks of such phone calls, many letters, and asserted other interventions by Feldman, the dean finally decided not to accept the department's and promotion committee's recommendations that I be hired. Subsequent to this decision, and on the basis of these interventions, all the members of the history department search committee resigned the letter addressed to President Richard Kirkland, Vice President for the Professional Division of the American Historical Association. After detailing some of Feldman's interventions, the search committee members urged the Professional Committee of the A.H.A. to "establish some guidelines to assure fairness to any candidates who might, in the future, be the object of similar campaigns to deny them..."
A central part of Feldman's campaign has been his circulation (to my publisher, prospective employers, and many, many colleagues) since February 1984 of a typescript of a "review" of my book written by his former student, Dr. Ulrich Nocken, now at the University of Düsseldorf. This "review" was, to say the least, highly critical. At the outset of his discussion, for example, Nocken declared that my book "in no way satisfies the demands of scholarly work, since the author violates every rule for the correct handling of primary sources, statistics, and secondary literature." Pronouncing my book "one of the saddest phenomena in modern historical scholarship," Nocken alleged that I had used "non-existent archival materials" and "long out of date" theories to concoct "an enormous, partly false, simplification of complex reality." 6

Much might be said about this text, and the uses to which Feldman put it. In the interests of space, I enter only one observation here. It is reasonable to assume that throughout its circulation the Nocken typescript's credibility as an accurate rendering of my work was enhanced by Feldman's repeated characterization—as made as early as late February 1984—of the "review" as "forthcoming" in the Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte? In fact, (1) the circulated typescript was only part of the manuscript Nocken finally submitted to the Vierteljahrschrift, (2) the manuscript submitted to that journal was either only just completed, or not yet completed at all, at the time Feldman began describing it as already accepted and "forthcoming," (3) the editors of that journal subsequently rejected the manuscript as submitted, returning it to Nocken for substantial alterations and a change of its focus along lines he began to suggest in that part of it not included in the version circulated by Feldman, 10 Feldman's description of the "review" as "forthcoming," Santa Cruz, Feldman stated that the "review" was "to be published" in the Vierteljahrschrift, 11 ibid.

8. I have seen two versions of the Nocken manuscript. One is the Feldman circulated. It contains 28 pages of text, 7 unnumbered pages of footnotes, and bears the subtitle "Teil I: Methodologische Kritik." The second version contains 20 pages of text and 8 pages of notes all numbered consecutively. In the second version, the subtitle omits the "Teil I," but in the text itself Nocken has two main sections, "I" and "II." With a few important changes, "I" of the second version is the same basic text as the entirety of the first version. "II" of the second version is new text, and offers a substantive, as opposed to "methodological," criticism of my book. I know that the second version of Nocken's text is the one submitted to the journal for review.

9. In my letter of February 23, Feldman informed me of this himself in a letter dated March 8, 1984: (2) in response to a query from me about the disposition of the Nocken manuscript, the editor of the journal provided me with a copy of correspondence from the journal's book review editor to Nocken that clearly refers to "Teil II." See note 10, below.

10. The first I learned of the completed manuscript was from Nocken's letter to me dated March 8, 1984 announcing that "Under separate cover, I am sending you the completed, partially revised and corrected manuscript which has been submitted to the "Vierteljahrschrift." (ibid.) A month earlier, on February 7, Nocken had sent my editor at the Princeton University Press a copy of the first chapter of the manuscript. That letter begins: "Dear Feldman, I am sending you the MS of an article which I am not now finishing for a German journal to you." (ibid.) Slightly edited, this seems safe to conclude that Nocken actually finished and submitted his manuscript to a German journal shortly after his letter to my editor, and he may have only finished and "corrected" that part he wrote me. If the first is true, then Feldman was allowing less than two weeks for submission, review, and acceptance for publication. If the second is true, then he allowed no time at all.

12. After receiving the revised, expanded version of Nocken's manuscript, I wrote the editor of the "Vierteljahrschrift" on two occasions, April 3 and June 9, 1984 (ibid.), informing her about the Nocken manuscript and requesting an opportunity to respond to it. She should be published there. In response to these queries, the chief editor of the journal, Professor Wolfgang Zorn, informed me in a letter dated June 12, 1984 (ibid.) that the Nocken manuscript "will not be printed or appear in it as submitted without our permission and ahead of time." It had been sent back to Nocken in early May with the invitation to which there had been, by the time Zorn wrote me, no reply from Nocken—too late either a "mistake" ("Miseille"), which for the most part cover several books, or a real "article," in which case Nocken's manuscript "would have to be structured differently from the submitted manuscript, but also would have the advantage of having as its object a substantive treatment in Part II of the submitted part." Again, "Part II" of the Nocken typescript was precisely that part not included in the version widely circulated by Feldman and identified as "forthcoming."
"forthcoming" was thus inaccurate at the time he offered it. Since he and Nocken were in contact during this period about the typescript, it is reasonable to infer that Feldman knew his description was inaccurate at the time he offered it.11

Nocken has every right, of course, to write as sharp a criticism of my work as he likes. But the misrepresentation of that which he did write, and the manner in which it was circulated, deprived me of any opportunity to respond to charges carrying, falsely, the imprimatur of a scholarly journal, and proved a lethal weapon against me. Feldman was instrumental in bringing about this result.12

In addition, Feldman continued in 1984 his practice of widely circulating "Dear Colleagues" letters commenting on my character and work. These too were received by my publisher, prospective employers, and many, many colleagues. Among the changes Feldman raised in these missives were that I "bamboozled" him and implicitly other colleagues in the profession, engaged in "academic fakery," and "knowingly lied" in my exchange with Turner in the AHR.13 Feldman found it "incredible" that I have found any employment as a historian and accused me of a "systematic effort to cover up the truth."14 "This profession needs to be protected against people like David Abraham," he declared, "however pleasant and convincing he may appear on the surface."15 Given what he claimed to know about my book, which was based on my Ph.D. thesis at the University of Chicago, he wrote that he "would move to rescind the doctorate of anyone who did a dissertation under my [Feldman's] direction of that character."16 To note my exclusion (at least in his eyes) from the ranks of professional historical scholarship, beginning in February 1984 Feldman began referring to me (in ec's in his open letters and elsewhere) as "Mr. Abraham," a practice not violated in his article here.

In sum, for more than a year and a half Gerald Feldman has, in a variety of venues and through a variety of informal means, advanced the claim that I am a liar, that my book's argument rests on egregious errors, distortions and fabrications, that I am unfit to practice historical scholarship, and that I am therefore unworthy of being employed as a professional historian. He recommends the bonfire for my book and the leper's bell for its author. This campaign has thus far been largely successful. This is what is at stake in the present exchange. Let us get to it.

* * *

Feldman here, again, accuses me of "egregious errors," systematically "tendentious misconstruals," and "outright inventions." Using the device of italics, he conflates both the entire range of charges and the evidence for them. This is a rhetorical masterstroke, for it permits Feldman repeatedly to say to the reader, in effect: "Here Abraham is guilty either of a fault, a misdemeanor, or a felony—take your choice—but since we know what sort of a fellow he is, we can assume the worst." When is a mistake only a mistake, and neither tendentious, nor a deliberate fraud? On Feldman's presentation such distinctions cannot be made, indeed can hardly be imagined. Instead, every evidence of error supports every single charge, and the presumed truth of the lesser charges provides, in turn, evidence for the truth of the greater ones. That my errors are "egregious" suggests the plausibility of "systematically tendentious misconstrual." That the "misconstrual" is "systematically tendentious" suggests the probability of outright "invention" and "fabrication." Like nesting Chinese boxes, each charge supports another inside.

If the charges and the evidence are sorted out, however, a different picture emerges. Yes, there are mistakes in my book. There are far too many of them and there are more of them than I thought at the time of my AHR exchange with Turner. I am painfully aware of this, and it is inexcusable. The mistakes are of various sorts. They include miscitations and incorrect

11. That Feldman and Nocken were in contact during this period regarding the Nocken manuscript is evident from Nocken's February 7, 1984 letter to my editor (see note 9 above), and from Feldman's "Dear Colleagues" letter of February 26, which states in relevant part: "A few months ago he [Nocken] called to tell me that the charges leveled against the Abraham book by Professor Henry Turner actually understated the amount of invention, misquotation, and egregious error to be found in that book."

12. Needless to say too, the force of this objection to Feldman's behavior will in no way be diminished by any subsequent publication of Nocken's views.

Note: Shortly after submission of this article in mid-November, I received, without any accompanying letter, directly from the publisher of the VSWG, the Steiner Verlag, what appear to be galleys proofs of a review essay by Nocken. It bears considerable resemblance to his previous effort, and I shall respond to it in an appropriate manner. I regard the objections voiced here as uninvited.


14. Ibid.

15. Letter of March 27, 1984 to Princeton University Press with numerous ec's, also in ibid.

identifications, imprecise translations, and, most seriously, paraphrases and interpolation mistakenly identified as full quotations. Whether my book contains more lapses of this sort than comparable works that have not been subjected to the ardent line-by-line critical attention that mine has is a moot question. Not everyone goes to a book in order to evaluate its particular arguments and overall thesis; historians (and others) often use a work for particular items of information: quotations, citations, etc. And here there are simply too many errors in my work.

How did these errors occur? I am afraid my explanation is mundane. Much of the research for *The Collapse of the Weimar Republic* was done during a year-and-a-half stay in Germany while I was a graduate student. While doing that research, I committed the embarrassing and elementary error of hasty and higgledy note-taking. The consequence was that my transcriptions sometimes yielded quotations that were elided or not precise. My then practice of sometimes translating and transcribing on the spot served me ill, particularly given my technical competence in German at the time. Worst, over the next several years, going back to my notes, I sometimes mistook summaries of documents for quotations. Such research practices, of course, cannot be defended.

What have I done about the errors? First, as is obvious from the above, I do not deny them. Indeed, in the past I have, in the case of Turner, Professor Knut Borchart, and others, solicited evidence of them. More immediately, perhaps, I have undertaken a comprehensive review of the scholarly apparatus of the book. Doing this has meant, among other things, going back, so far, to five of the six archives at which I worked; I intend to revisit the sixth. Neither a future English edition of my book nor the translation currently under consideration will appear without all corrections made.

All this aside, however, what Feldman charges is not just sloppiness but fraud, fakery, and the cooking of evidence; an intentional attempt to construct arguments that could otherwise not be sustained. One does not fabricate unless there is a need for fabrications; one does not invent unless there is a concept guiding the attempt; one does not misconstrue unless a case could otherwise not be made. But there is no fraud or fakery here, and the "egregiousness" of the errors made is far from clear. My "misconstruals" of archival evidence are not "tendentious." They do not, as a pattern, help my argument. There is no pattern of deception betrayed by my mistakes. In short, I am not, as Feldman has claimed, a "fraud," and there is no real harm done to my arguments by the correction of those mistakes or the reevaluation of particular pieces of evidence. I will show all this below in a comprehensive, detailed response to Feldman's immediate charges.

But what, in Feldman's view, was I attempting to construct? What arguments could only survive through tendentiousness, invention, and the like? Feldman's response is not clear. On the one hand, he hides behind his refusal "to dignify this book with a systematic discussion of its thesis." On the other hand, he persistently intimates that "what Abraham was up to" was "dogmatic," "fanatical," "preconceived," "insensitive[,...]," "ideological" and the like. As he has put it, I do not "write history," I "write sociology." 17 And I am like this and do all this essentially for the purpose of "exaggerating the evil" will and "overdramatizing the power" of industrialists. This won't do. Feldman leaves us no choice but to begin where we have to. How did Abraham come to his book and what is it about?

* * *

For quite some time, I have been interested in the conditions of regime stability in capitalist democracy and the peculiar requirements placed on such systems by the simultaneous maintenance of a capitalist organization of economic relations and a parliamentary democracy. And the purpose of my dissertation, completed in 1977, and of *The Collapse of the Weimar Republic* was to offer a case study of how these conditions were at first maintained, and then undermined in one very particular capitalist democracy. I was drawn to Weimar Germany because that society seemed to me to be almost a kind of laboratory for all of that in which I was interested, and I had seen in the classic 1930s works of Arthur Rosenberg exactly such an attempt. 18

My working hypotheses, and now my thesis, were: for stability to be maintained in a society like Weimar, there must be (1) some measure of elite consensus on the most salient public matters (after 1924, economic, social, reparations, trade, and democratization issues); (2) some compatibility and equilibrium between elite needs and the needs of substantial organized portions of the popular citizenry, because of their organiza-

17. Feldman to Abraham, May 17, 1983, also reprinted by Feldman with his circular letter of March 20, 1984; widely available, also in ibid.
18. The works of his by which I was most influenced at the time were, not surprisingly, his *Geschichte der Weimarer Republik* and his *Sozialism us und Demokratie*. The penultimate paragraph of my book, concluding my analysis, begins: "To paraphrase Arthur Rosenberg, we can say that the 'middle-class republic collapsed,'..." (p. 356).
between the more dynamic, export-oriented, and liberal industrialists and the leaders of socialist and Catholic organized labor—the need for immediate gains, electoral strength, and capable liberal political leadership. I tried to demonstrate how this system worked—largely by politicizing the economy; whom it benefitted—employed organized labor, the exporters, and the advocates of a peaceful revision of Versailles; whom it compromised—part of the Mittelstand, much of heavy industry, and most of agriculture; and what brought it to an end. Because of the political weaknesses of Germany’s middle-class parties and the fragmentation of the middle strata themselves, only the working class, through its socialist party and trade unions, could supply the mass support for a liberal capitalist society and political system biased toward large, well-organized interests. And, I argued, from roughly 1925 to 1930 it did so. German labor could provide such support not only thanks to its own vaunted organization, but also because it shared interests with those more dynamic and export-oriented industries, which during these years were ascendant within the ranks of German industry generally. With the onset of the depression, the end of American loans, the split over reparations, and the shrinkage in world trade in 1930, the balance within German industry shifted to more protectionist, labor-intensive, and nationalist-conservative industries. Under these conditions, the Weimar compromise could no longer hold.

Neither obdurate labor nor business manifested the readiness to compromise that might have saved the Weimar system. For reasons that I discuss in detail, starting in 1931 neither Chancellor Brüning, Papen, nor Schleicher could successfully win popular support and also offer a way out of the Weimar system, which had proven both costly and unstable. Labor’s price was too high (especially in costly Sozialpolitik), and the middle strata too fragmented and blocked out of the “System.”

Once capital withdrew from the social compromise that underlay stability, this stalemate was the source of Weimar’s collapse, and it provided a political opening for the mass authoritarian populism of the Nazis. For Weimar industrialists, the Nazi groundswell was something they had to address, particularly “in light of the elections” (Foldeman), and they were thus faced with the difficult task of choosing among political alternatives not directly of their own making. (The alternatives they tried to make did not work.) As I argue in my book, their response was various, inconsistent, and contradictory. What is important for my argument, however (and more so for German history), is that by late
1932 most of them could see “no other way out of the crisis” (Abraham, p. 323) and, therefore, to protect their social dominance “exposed themselves to a potentially uncertain future” (p. 327). By that time, for the industrialists, there was simply no feasible and acceptable alternative to the NSDAP, because no other force could claim real popular support while also demonstrating a credible commitment to eliminating Weimar’s fragmented political democracy and generous social welfare system. It is worth emphasizing, here again, that the question was not one of “evil” but of “how industrialists and estate owners, in light of the Nazis’ independently achieved successes, attempted to insert their interests into what Bracher has called the ‘power vacuum’ of the last eight or nine months of the Republic” (p. 315). Both the question and the answer to it just provided left the particular behavior of individual industrialists—and with it the possible presence of smoking guns—moot.20 Put simply, the Bonapartes are the unintended consequences of more generalizable and analyzable democratic politics.

Consistent with this structural focus on Weimar’s collapse, and despite the inherent urgency of the question, I offered no sustained explanation for the actual growth of Nazism, beyond observing how the Weimar “System” was pivoted around big labor and big capital, thereby squeezing the Mittelstand. And I very consciously separated the question of Weimar’s collapse from that of Nazism’s growth and coming to power. The former seemed to me to be part of the general phenomenon in which I was interested, the latter a unique development for which one could account but not in any generalizing or generalizable way.

As I wrote in the concluding chapter of Collapse, in “Weimar Germany with its very strong and mutually antagonistic reformist and communist parties, a fragmented bourgeois political opposition, and no unified or coherent leadership of the dominant economic classes—an internal stalemate—the outcome [of the economic political conflict in a period of sharp contraction] was a crisis marked by a rupturing of links between representatives and represented and a catastrophic equilibrium that opened the door to extrasystemic solutions. Of course, having the door open and letting Caesar/Bonaparte/Hitler in are two very distinct processes or acts; the collapse of the Republic and the Nazi assumption of power were by no means the same... That no stable ruling bloc could be organized under a democratic form of state did not, of itself, indicate that a fascist solution, whatever its nature, would follow.” (pp. 284, 285)

* * *

The analysis of late Weimar Germany has been a “dangerous” and controversial field for nearly 55 years, vulnerable to shifting intellectual and political currents. There is no space here for an adequate discussion of the historiography. Much of the marxist work has, in one form or another, remained prisoner to Dimitrov’s 1933 thesis that, in the period of state monopoly capitalism, fascism represented the triumph of the most reactionary and chauvinistic groups of monopoly capitalism. This viewpoint, by exonerating other groups of capitalists, was and still is meant, among other things, to facilitate fascist politics. A non-marxist variant of this approach might be described as constructing a powerful elite conspiracy model: powerful, evil members of various social and political elites conspired to bring down the Republic and transfer power to a camouflaged right-wing dictator.21 On the other side of the divide from the leftist smoking gun school lies the anti-leftist no smoking gun school, whose most prominent current representative is Turner, whose central thesis—in some ways indisputable, at least at the level at which it is given—has been that “the great majority of German businessmen neither wanted Hitler’s triumph nor contributed to it materially.”22

A more complex approach to the relationship between German business and fascism examines the political and economic divisions among capitalists and identifies some as somehow more in need of a fascist-like solution to the crisis than others. This type of argument was first devel-

20. The perceived inadequacy of the political system—particularly after Papen, whom many of them supported whole-heartedly, could win no popular support, and Schleicher, whom some others of them supported, threatened to reintroduce unions and a repatriated army to the political life (see, for example, pp. 171-79, 210-21, 321)—ultimately allowed, according to support or surrender to the Nazis, particularly as neither a presidential nor a military dictatorship was possible and greenhorn conservative “mice” movements never got off the ground.

21. See, as an example, G. W. Hallgarten, Hitler, Reichswehr und Industrie (Frankfurt, 1955).

oped by socialists of the 1920s and 1930s, particularly in writings indebted to Rudolf Hilferding's discussion of "organized capitalism," and especially in East Germany the argument has been taken over by various Stammkap historians and theorists. In West Germany, the sorts of questions raised have recently been explored through empirical work of the most varied analytical and political sort examining the relationship between industry and politics in Weimar.

I have learned from all of these schools and scholars, most particularly from the recent empirical work just noted. But, in the end, I was and remain an outsider to them all. Bearing in mind that most of my book is about the period before 1932, my own framework and analysis derive, in the first instance, from the analysis Marx provided in the *Eighteenth Brumaire* and that has come to be known as Bonapartism. What is emphasized here is the mutual exhaustion and stalemate of contending social groups and vanishing parties that enabled a mass-based dictator to enter and take over the political scene, while simultaneously saving and threatening the social elites. Such analysis has always led to an uncomfortable existence within as well as outside marxism. Structural in its approach and first-admumbrated for the Weimar case by August Thalheimer, it has always stressed the unintended consequences of elite and mass action and has been discomfiting perhaps precisely for this reason. To this basic Bonapartist model, I joined conceptualizations about capitalist democracy, consent, and hegemony drawn from the work of recent marxists, especially Antonio Gramsci and Nikos Poulantzas, and, for me, influentially developed in the work of Adam Przeworski.

Whatever the merits of an argument such as this, it soon became clear to me, if it was not at the outset, that certain dangers would nevertheless attend its reception in the historical community. First, the work was identifiably marxist, and one which took sides in certain identifiable disputes within marxism. An obvious danger here was political hostility, commonly reserved for those works that discuss the privileged position of capital in modern politics. Fortunately, my work has encountered virtually none of this. A more subtle, but intellectually more debilitating danger it courted, was simple misunderstanding. Given the general unfamiliarity with recent marxist scholarly debate and disputes, my work might be identified as one of precisely those forms of marxism it was meant to distinguish itself from. Disastrous, for example, would be Collaps's identification as a Stammkap or teleological argument, although both *Collapse* and Stammkap theory owe debts to Karl Marx.

A second, and more general, danger was my book's highly structuralist approach, and this, in two senses. Many of the questions that have concerned historiography of the period—most prominently the connection between individual businessmen or groups of them and Nazis—simply do not concern me. In addition, my book's structural analysis, beginning with a lean and explicit model in the first chapter, to cover a great deal of ground at relatively short length and is, both for

22. For a good discussion of these two theories and how closely related they in fact are see Ulrich Kuehle, "Organisierter Kapitalismus und Staatsempiristischer Kapitalismus," in H.A. Wintzer, ed., *Organisierter Kapitalismus* (Göttingen, 1974), pp. 19-35. Examples of the Stammkap schools include, Kurt Gosweiler, *Großkapital, Industriemonopole, Staat* (Berlin, 1971); Jörgen Kukenski, *Studie zur Geschichte des deutschen Imperialismus I* (Berlin, 1978); Eberhard Crechen, *Hohes Flötendach* (Kölnige, 1961). The list of those three is the only one from which I have derived anything. Nearly all Stammkap—state-monopoly-capitalism—analyses are grounded in the view that leading monopoly capitalists control the state and its functions.

23. Any examination of my book's bibliography makes its debt to this literature clear. The work of Friedel We以下内容被截断，无法完整阅读。
Given the historiographical inheritance and the dangers in the field and my approach to it, I was delighted with the reception of my book. I was especially pleased that reviewers (by now about thirty in five languages), whether or not they agreed, consistently and overwhelmingly appreciated what the arguments were and what they were not.

My focus on the conditions of stability in capitalist democracy was clearly understood by sociologists as well as historians, by West Germans as well as Americans. The specific site of Weimar's stability and instability was appreciated in different disciplines and countries. The larger theses and the individual arguments of *Collapse* were understood independent of age or language. "Evil" and "conspiracy" did not appear at issue for British, American, or German reviewers of the book.

20. Michael Burawoy, *Contemporary Sociology* 14 (1959): 504; "Although it is never described in the historical analysis, a theory of the conditions of capitalist democracy is nuanced . . . " and Michael Schneider, *Archiv für Sozialgeschichte* 22 (1982): 290; "The problem of accounting for the decline of the Weimar Republic, however, is the central issue."

31. E. G. Spencer, *Business History Review*, Winter 1951, 1. "The "business wholesome life" of the world crisis in the depression and the inability of programs of governmental and business leaders to influence corporate and political behavior"; and James Hunt, *Review of Politics* (forthcoming); "The Weimar Republic failed because of the inability to balance political and economic interests with the existing constitutional framework. . . . The most compromise bloc [first] emerged [and then shattered]; an alliance, preserved by Garrey Steenbrink, between the dynamic export industries and the liberal socialists and Catholics." "A greater attention to empirical data might well enliven Marburg's book."

32. Richard Hofstadter, *Toward the Liberation of Education*, September 24, 1955; "During the prosperous twenties it had been possible to follow a 'lively' and 'lively' dominated by the export industries with the willing support of organized labor and the expanding economy of heavy industry; this was shattered by the Depression. For 1924-1925 only had been a battle for survival and the social foundations upon which the Republic had rested could not be brought back."

..."Given the economic crisis, the drying up of American capital and the contraction of world trade, things changed. No longer could even the 'economic fraction' of industry afford Socialpolitik and . . . the pluralist democracy which it needed . . . The result was a growing determination to use the crisis not only to diminish any opposition but also to eliminate economically threatening political structures. This, in Althusser's view, was the basis on which the dominant social class, itself unable to confront postwar problems, sought an alliance with the public sector and an eventual control of opposition."

..."The German bourgeoisie proved that it was necessary . . . but it was no different with the working class. A reformist party like the SPD should not have remained a class party."

..."Hof's policy," "isolated the working class from the other salient groups and strata of society."

..."In his holocaust essay [at the end] those who had no policy and even less an ideology . . . were compelled to think of themselves in their whole . . . their fate was no other way out by 1934."
indeed readers were struck precisely by their absence. On the role of

marxism in my work there was general appreciation of its nondogmatic
type, although different reviewers expressed that conclusion in
different ways. And, finally, on the question of the relationships between
businessmen, Weimar's collapse, and the Nazis, reviewers appreciated
that it was never my attempt to impute individual (or collective) intention
or responsibility.

late 1928 on was, on this reading, not a willful exercise in authoritarian union bashing but
began definitely in the realm of economic necessity...."

question of capital's financial support for the NSDAP to a broader structural analysis. ..."

33. "Marxist models of fascism"; Sven Papke, _West Deutscher Rundfunk_ 3 April 4, 1983:

34. "Der Glaube an die absolute Macht des Marxismus in der DDR" (in _Marxismus
die absolute Macht des Marxismus in der DDR")

35. Richard Evans, _Historical Journal_ 26 (1983): 1008: "This is the first real attempt at a
serious Marxist interpretation of these events that gets away from the sterilities of the
toward a more sophisticated structural approach." W. Ruge, _Zeitschrift für Geschichts-

36. First was Henry A. Turner, _Political Science Quarterly_ 97 (1982): 210-212: "The industrials and the agrarians had, as Dr. Abraham shows
earlier, and certainly skill in confronting a radicalized bourgeoisie and peasantry....

37. "Empirical history stands no chance against Marxist dialecticians." He closed by wondering "how could such a book be produced by the hope-filled progeny
of a capitalist private university" (emphasis added), after opening by bemoaning the fact
that a Marxist, namely, I, had made it to "Princeton, the renowned university on the east
coast." Feldman's own comment on this review, when I asked him about it, at the time,
was that he would have been even harder, "while simply paying no attention to my
"Marxism." (Feldman to author, April 29, 1983, author file). All three dangers were
thus indeed present.
names it my “complete insensitivity to and lack of interest in what actually took place in the past” (p. 168) and “indifference to what documents actually say and fanatical attachment to preconceived notions” (p. 170).

Although Feldman did in fact finally approve my manuscript, giving in to what he has described as his “weak-kneed liberalism,” the dangers voiced in that report are the claims raised here of “insensitivity,” “indifference,” “disdain,” “fanaticism,” and “contempt” for historical reality. And while here sometimes professing agnosticism as to the cause or intent, “whatever the motives, whatever the reasons” (p. 177), Feldman’s own conclusions present the third danger—that I hold businessmen responsible for what followed Weimar and therefore “exaggerate the evil role played by industry” and “overdramatize its power.” Nocken simply extends this same logic and same claim to write that I “tie German heavy industry to the National Socialists as closely as possible” (typescript p. 8, as distributed by Feldman); “discredit heavy industrialists”; portray them as “mighty wirepullers”; impute “dark influences” to trade associations; and, finally, present “without using the name” the “state monopoly capitalism theories” of “official East German theory.”

This position culminates with the charge that distills all three misunderstandings: “Abraham’s central thesis [is] that the NSDAP was brought to power through a joint decision of the dominant fractions.”

These misapprehensions all bear on the present charges. For the implied meanings for my “delinquencies” and “violation of trust” revolve around precisely this axis—ideologist with preconceived notions, sociologist of insensitivity, conspirator with dramatic intent—and it is from it that the phantasmagoria of my work has been projected.

Now, it might also be that I hold some antipathy toward capitalists, individually or collectively, that would lead me both to paint them as malevolent social actors and to exaggerate the degree to which they, as a class, engage in concerted action. That such is not the case is evident from the very core of my work, and it must be paradoxical for Feldman: in large measure, mine is a work of decomposition into industrial fractions and political blocs, one of whose consequences may be to make a goodly portion of Weimar industry look too progressive. In any event, such was not the pond in which I went fishing, or as T. W. Mason put it in his second AHR contribution, “the bulk of Abraham’s book represents a sustained (and, in my view, in good measure successful) effort to raise the argument to a higher plane of structural analysis,” one at which the motivations and choices of individuals are heavily determined. . . . in terms of his own analysis, these constraints and pressures help account for the collapse of the Weimar Republic but not for the construction of the Nazi dictatorship.” Hence too, there are no “Secret Cabinets” or the like in my work.

I think, rather, that it is the mistakes just discussed that lead Feldman to a series of judgments that include my being unable to “comprehend the most elementary laws of historical proof and evidence” (p. 160), as well as to seeing malevolent patterns where there are none, illustrations of general points as claimed proof of different specific ones, and, ultimately, the intent to fraud where there was nothing of the sort. Ideologies are “fanatic”; they operate with “preconceived notions”; they “exaggerate”; they use the word fraction without derivative quotation marks. Sociologists (unlike historians) focus on groups and constraints, rather than individuals and volition; they see evidence more as illustration than as proof of truth; they may also use the word fraction, and discuss history as something square rather than round. Work by scholars of these sorts may easily be confused with “economic determinism or dogmatic theory building.” It may posit something “immanent in the capitalist system,” a proposition Feldman rejects as an approach which only distracts our attention from the specific problems. . . . And here, he alleges, it may even lead to fraud.

The acknowledgement of error is the beginning, not the end of discussion—both for Feldman and for myself. On the basis of the errors he cites (most—though not all—of which are errors) Feldman would lead the reader to conclude (1) that “this profession needs to be protected against people like David Abraham,” (2) and (3) that their presence


20. Thus Feldman speaks of archives making materials available “for the discovery of historical truth,” whereas I think that, rather than containing the “truth,” they contain evidence and illustration for a range of arguments. And I have neither heard of nor experienced any “distrust.”

41. See Gerald Feldman, Von Weltruhm zur Wirtschaftskrise (Göttingen, 1954).

42. Ibid., pp. 221, 232.

43. Feldman circular, March 27, 1984; see note 15, above.
undercuts the particular arguments and overall thesis of my book. To
convict me of "systematic tendentiousness," or a "conscious deception
maneuver or, at the very least, cover up" (Nocken) he is compelled to
assert that what is involved is much more than random lapses—some of
which "help" my argument, and others of which "hurt" it. To sustain
his conspiratorial interpretation of my behavior he must try to show
that the errors are patterned so as systematically to mislead the reader
to conclusions which he or she would not reach on the basis of accurate
renderings. This, as I will shortly demonstrate, he cannot do. The errors
are not patterned as he alleges, and they do not have the consequences
he alleges. The arguments are in no way dependent on the errors; ac-
curate renderings would sustain my arguments as well as deficient ones.
In some cases, the arguments of the book stand, when the errors are
corrected, even more firmly than before. This does not make me any
less unhappy over the errors, but it does prove that they are not the
consequence of dishonesty. Any rational, non-conspiratorial hypothesis
of my dishonesty collapses, since the lapses were counterproductive for
establishing my case.44

Let us turn now to the "errors," "distortions," "inventions," (and/or)
"fabrications" alleged by Feldman. I will take up every one of his allega-
tions, arranging them in ascending order of seriousness: errors, "ten-
dentious distortions," and "tendentious inventions" or "fabrications,
climaxing with his centerpiece, the celebrated "Christmas exchange."

NON-ERRORS

Feldman charges that, contrary to my assertion, Bernhard Harms was
not the director of the Friedrich List Gesellschaft. He is quite mistaken:
Gesellschaft stationery from the year in question so identifies him.45
Feldman charges that it is illegitimate for me to characterize Otto Wolff
and Carl Friedrich von Siemens as "executives" of the Deutsche Bank
on the basis of their membership on its Supervisory Board of Directors
(Aufsichtsrat). "Officers" they were not, but he underestimates the role

44. Conspiratorial theories are notoriously impossible to refute, since they can be
"discredited" by the addition of ad hoc hypotheses of "incompetent dishonesty," "sup-
pository manipulation," etc. Ultimately they can only be rejected on grounds of
implausibility.

45. To be found, e.g., HA GHI 400 101 221/3. Even a source as authoritative as
Feldman's, Wenzel, can be wrong sometimes.

46. See, for example, Jürgen Kocka, "The Modern Industrial Enterprise in Germany,"
in Alfred D. Chandler and Herman Dangel, eds., Managerial Hierarchies (Cambridge,
1980), p. 91, where the "interlock" between banks and industries pivots around members
of these Aufsichtsräte.

47. Factoring out lignite, as Feldman seems implicitly to suggest I should have, would
strengthen my argument that mining as a whole was stagnant.
committee, an economic policy advisor to another "dynamic faction"—I. G. Farben—and, hence, the case for his linking the "dynamic forces of industry and RDI power positions remains.

Not all random errors are completely neutral in their consequences; inevitably some "help," others "hurt" the argument. Thus, I'm likely correct in his point 8 that my understanding of "Zentrum and the NSDAP" as including both ends as well as the middle was not justified; the German "zwischen" was more exclusionary sense rather than inclusionary sense. Feldman is right in saying that in Table 15 I mistakenly labelled as "iron and steel" was in fact "metal production." Here getting it wrong "helped" my argument. I sought to show that iron and steel were not dominant and had I factored out copper, tin etc., the iron and steel index have even lower, and hence better for my argument. Finally, there are two allegations of error by Feldman which I simply do not understand. What does it mean when he says here that I "round off" data in such a way as to produce false averages"? What are the "false averages"? I am charged with "failing to indicate that Hoffmann included work as well as industrial production in his figures," which I neither the construction of my Table 15. But the table lists production in all, entire branches and hence naturally includes the production of all kinds, craft as well as industrial, which is what Hoffmann said.

* * *

We now move on to the rather more serious charges of "tendentious distortion." In nearly all of these cases I am guilty of error of one kind or another; in none of them can the charge of "tendentiousness" or "distortion" be sustained. There are seven allegations of this kind lever by Feldman.

TENTENTIAL DISTORTIONS/MISCONSTRUCTIONS

Kastl's Leadership: "Exaggerated Evil"; "Overdramatized Power" (Feldman's Point 3)

Feldman alleges that "there is no evidence whatever that Reusch was launching a personal attack on Kastl." If I had written, as I should have, "Reusch wrote to Kastl that under his leadership 'industry has until now been too cowardly . . .'", a characterization fully supported by . . .

48. Weisbrod, p. 222, n. 15

document and others—the focus would have stayed where I put it. The policy issues. And the point is very important, the charge significant. For here we have an issue that goes straight to one of my more central arguments, namely that Reusch, along with other Ruhr industrialists, believed that under the liberal leadership Kastl represented, the RDI had been too indulgent toward Brüning and had shrunk from a necessary confrontation with the unions. Were that argument dependent on or enriched by my error, the problem would be serious. But it is not the case. The evidence for my argument, beginning with this point, is abundant and overwhelming. And it is not Kastl as a person or as Reusch felt about Kastl as a person (as it might be for Feldman's sake), but what Kastl represented that is at issue for me.

What did I write on pp. 168–69, and what does Reusch's letter to me say? I wrote: "The liberals were prepared for a compromise candidate to replace the retiring Duisberg. Without attacking the aged 'Iberville Professor' directly, representatives of the national fraction linked his policies [support of Brüning, considerable cooperation with the unions—DA], primarily by assailing the RDI's chief administrative officer, Kastl. Intimately involved in the export-labor coalition, he was now accused of 'cowardice' in the struggle with the unions. . . . Under your leadership industry has until now been too cowardly to take up the struggle with the unions in all its sharpness. That great ill from which we suffer is largely due to the unions . . . which have in fact governed from behind the scenes." Reusch to Kastl, 6 Sept. 1931, "GHII . . ." (pp. 168–69).

What does this letter from Reusch to Kastl say? Is my presentation exaggerated? does it "overdramatize the power and self-assurance" of industrialists and especially of Reusch? Is it wrong or tendentious about Kastl? The letter was written at a time, September 6, 1931, when the Reichsverband (RDI) under Duisberg and Kastl still supported Brüning and his dependence on the Social Democrats and trade unions but the Ruhr industrialists and their chief organization (the Langenamverein) had gone into opposition. Here is what Reusch wrote in this same letter talking "not in the abstract but rather of the last RDI president meeting": "I support the minority opinion [Kastl had gotten his way—no attack on Brüning], that Mr. Brüning, given that the expectations we had placed in him have not been fulfilled, and given that he does not have the courage to separate himself from the Social Democrats, must be opposed [bekämpft] in the sharpest manner possible by the leaders of the econ-
omy and by the Reichsverband [i.e., the Duisberg-Kastl leadership-DA] and that industry further must fully openly speak out its mistrust of him. . . . I am further of the view that we finally must change our tactics [largely developed by Kastl-DA] toward the unions. Industry has until now been too cowardly to take up the struggle with the unions in all its sharpness. That great ill from which we suffer is largely due to the unions—which all the governments since the days of the Revolution have to one extent or another let themselves be influenced by—and which have in fact governed from behind the scenes. . . . one must finally rise to take the bull by the horns and fully openly and head on declare battle against the unions” (emphasis added).

It is very clear here that, in this letter to him, Reusch assailed the policies pursued by the organization under Kastl’s leadership. I did not make enough of this!

Already six months earlier, March 10, 1931, Blank wrote his chief Reusch, in a letter headed “Persönlich”:

“Today I had the opportunity to check out the rumors of the last several days, according to which Kastl intends to leave his position at the Reichsverband. I have reliable word, that Kastl has received an offer from a firm headquartered in Berlin, which would pay him a higher salary. . . . It seems that—given his differences with certain circles of western [i.e., Ruhr heavy-DA] industry, which in the long run cannot be resolved—all sorts of people whom Kastl has been asking for their opinion, have urged him to take that offer” (emphasis added).

For his part, Schlenker felt it necessary already in late January 1930 to deny press reports that he intended to replace Kastl or have him replaced.50 Reinhard Neebe, who has pursued these matters more closely than I, has the following to say about Kastl’s leadership and policies: “The managing director and presidium member of the RDI, Kastl was not . . . completely in accord with this confrontation strategy [toward the left-DA] but he barely retained any support for his divergent view. . . . Not without reason did Gilsa complain, that Kastl called for maintaining the [Grand] Coalition [with the SPD] not only for tactical reasons. Taken to task for this by Reusch [zur Rede gestellt], who had already in December welcomed the DVP’s rejection of compromise, Kastl defended himself adroitly but could not prevent his increasing isolation. Press

51. Neebe, pp. 55, 56, emphasis added. Here too, I have not yet had the opportunity to examine all the documents Neebe cites, but he is fully persuasive.

52. Reichert to Schlenker, December 4, 1930, in Gerhard Schulz, Die Maurer, and Udo Wengst, eds., Politik und Wirtschaft in der Krise: Quellen zur Arz Brening (Düsseldorf, 1980), pp. 478–80; cited hereafter as SMH. Reichert raised the matter with Reusch the next day.
directly." 53 As for the dramatization of power and self-assurance, after recently seeing these letters from Reusch and from Reichert, I think I may have here initially vastly underestimated both.

Paul Silverberg: "Exaggerated Evil"; "Overdramatized Power" (Feldman's Point 6)

In the case of Paul Silverberg, Feldman's point of departure is the fact that I mistook a summary in my notes for a quotation. Feldman's translation is, with one striking exception, accurate. 54 For Feldman, mine is a "consistently tendentious presentation [which] serves to exaggerate the evil role played by industry." But there is nothing in the summary quoted by Feldman, or in any of my 18 substantive remarks about Silverberg, that would offer any basis whatsoever for such a characterization.

These 18 mentions are to be found on pp. 37, 134, 138, 154, 160, 161, 178, 189, 206, 208, 211, 220, 237, 267, 300, 313, 321, 322. The following four are representative: "Silverberg, a politically complex figure, sometimes a progressive initiator, sometimes a pessimistic bellwether" (p. 134). "Silverberg announced that industry now had an absolutely affirmative attitude toward the [republican] state and that no government was possible without labor... A 'social partnership' was the order of the day for the sake of the nation" (p. 138). "Silverberg chided the processing industries for a certain konjunkturpolitischen Opportunismus in their dickering with the political left and right" (p. 154). "In fact, neither Krupp nor Silverberg cared particularly for Hitler or the Nazis" (p. 321, n. 120). My other references show the same respect for balance and complexity in the "muddle" of history.

I was already aware of this error, and not only because it, like most of the others here, is taken over from the Nocken typescript. Rather, it was reported to me, as part of several pages of "suggestions and corrections" I solicited from Professor Knut Borchardt of Munich in 1982. For Borchardt, this error was no basis for claiming that I "exaggerated the evil" or "over-dramatized the power" of the protagonists. Rather, Borchardt—no radical!—wrote that, for me, the end of Weimar was "not the work of ideologically tainted reactionary plotters." For my analysis, "a fascist disposition on the part of industrialists—which in principle cannot be demonstrated—is not needed for the explanation of political transformation." My book contained "little if any moralizing or Beser-wissen." Further, this very list, subjected to Feldman and Nocken's alchemy, now forms a basis for charges of "conscious manipulation" as well as for Feldman's claim that he is "indebted" to "Professor Borchardt for a great deal of the information on which this article is based." Indeed he is, but not at all in the manner he suggests.

The Deparimentalization of Economic Affairs/Taking Advantage of the Unions (Feldman's Point 7)

Feldman makes two paired accusations of "tendentiousness"—not readily comprehensible—with respect to my treatment of the "deparimentalization of economic affairs." First, that RDI concern with "get[ting] out from under parliamentary bumbling" and "take[ing] advantage of the unions" were "not related to one another." But as I demonstrate on my pp. 271-74, the RDI saw an opportunity to circumvent parliamentary bumbling and the political logjam by dealing with the unions in their weakened condition. 55 Second, rather than, as Feldman would have it, the RDI's desire to negotiate with the unions contradicting an intention to "take advantage of the weakness of the unions," it was still very weakness ("their members are fleeing from them" ["ihre Mitglieder ilmen weglaufen"]) that offered them the leverage which made negotiation attractive.

Far from my "exaggerated[ly]" evil industrialists hardening toward talks with the unions, I quote from this same document exactly those lines Feldman suggests I suppress: "'Right now the unions are relatively reasonable, and something can be attained by dealing with them; the social democratic leaders, on the other hand, appear to be incorrigible.'" 56

Reusch, Schacht, Collaboration (Feldman's Point 10)

The Schacht Arbeitsstelle was discussed at length by Turner, Mason, and myself in the AHR exchange, and readers can turn to it for details and corrections of the deficiencies involved, including the correct dating...

55. "...die Gewerkschaften augenblicklich relativ verpflichtet sind und dass mit ihnen schon etwas zu erreichen sei, wohingegen die sozialdemokratischen Minister scheinbar unbelastbar seien." Abraham, p. 271, also SMW, p. 70.
of Reusch's sentence—not fabricated but misdated—about his "complete sympathy" for the National Socialists and their "tactlessness." Over a year and a half ago, Feldman himself asserted that "a correct rendering of the materials would not" affect the argument. Here, however, by injecting misleading and irrelevant terms like "enthusiastic" and "praiseworthy," he has raised error to the level of fanaticism. Given my argument (p. 320) that industrialist support for Nazi participation in government peaked in late autumn, a correct dating (September 21, 1932) would have been far better for my case than the incorrect date I provided for the second Reusch communication, March 20, 1932, italicized here by Feldman.

As for collaboration: if "collaboration" means shared labors, working jointly with others, then the policy was, with all due respect, (p. 172) a "collaborative" one. I do claim is rather less than Feldman's touchstone, Neece, who writes: "These efforts were clearly a continuation of the first tentative efforts of Heinrichs Baur [Christian 1930-DA] and Funk in 1931; through the establishment of a definite institutionalized framework and the inclusion of Hjalmar Schacht, however, they came to hold a qualitatively different significance. . . . Cooperation with Schacht—someone publicly known as a 'open advocate of Hitler at least since the Harzburg meetings [October 1931]—made clear the drawing near of the Reusch group to National Socialism."

Whether "praiseworthy" or not, I do not know: that it was "enthusiastic," I have not sought and would not seek to claim. Far from suggesting anything like enthusiasm, I indicated on the very same page that the Nazi economic program remained ambiguous and that, although these industrialists considered enlightening the Nazis a necessary and promising approach, there was no reason to assume it would bear fruit. Also on the same page, I proceeded to list the obstacles (the radicalism of some Nazi leaders and their mass following, the party's commitment to state interventionism in price setting, taxes, industrial administration, and trade, etc.). Nevertheless, Schacht wanted as far as possible to assure (sicherzustellen) that "the economic policy conceptions emerging from the Arbeitsstelle be in harmony [in Einklang sich befinden] with those views represented from the national socialist side" by Hitler's two ap-

57. Feldman adds nothing new, but Neece's new claim, p. 8, that the misdating serves my argument is completely illogical.
58. May 20, 1932, letter to Press with cc's.
59. Neece, pp. 7, 8, gets both dates wrong, writing, September 19 and April 19.
60. Neece, p. 122, emphasis added.

pointees. In the end, the practical support that rebounded to the Nazis from the actions of individual industrialists was considerable.51

I described the Schacht operation as one of the "industrialists'" efforts at "enlightenment" of the Nazis on economic issues—efforts that dated back to December 1930 as a matter of fact. Schacht's proposal to Reusch and others—to organize and finance a two-year commitment, in steady touch with Hitler's appointees, to formulate for the Nazis an economic policy in accord with their own—was a contribution to the increased power and legitimacy of the Nazi movement. That it was a self-interested undertaking, I take for granted. Whether or not it was the intended, conscious purpose of these industrialists, this action assisted the Nazis. Schacht's design was to organize and use cooperation in order to bring at least Hitler's wing of the NSDAP over to the economic conceptions of these Ruhr businessmen. There may have been more in this for the vain and self-serving Schacht than for the others: within a year he turned to organizing the Adolph Hitler Foundation of German Industry.

Whatever their intentions, the cumulative actions of Schacht and his collaborators did lend real and tangible aid and assistance to both Nazi legitimacy and success. The need to work together issued out of the necessity of stubbornly defined interests—especially heavy industry's determination to bar a return of a socialist-influenced Weimar welfare democracy—and in view of the Nazis' own popularity, what Feldman here chooses to call "in light of the election results," I agree!

It is, I think, Feldman who exaggerates the power and self-confidence of industrialists. By assuming that they believed they could turn the Nazi movement into something it wasn't, he ignores the fact that, in the end, leading industrial figures were prepared to accept or surrender themselves to the Nazis as they were. Only thus can Feldman find himself adopting the categories of analysis offered by Schacht (though obviously rejecting Schacht himself): "heavy industry was "clumsy" [and] there existed "hardly anything with less intelligence and character than German industrialists in politics.""62 This is a kind of analysis I have trouble understanding.

The 1925 RDI Elections (Feldman, p. 173)

Like much else here, the difference between Bernd Weisbrod's conclusions about the 1925 RDI election and my own will be old hat to

61. Neece, pp. 153-73, 196. Neece and I agree on a number of points here, including the preference for and continued support of Schleicher by liberal, export-oriented groups like the DHFT.
numerous readers, Feldman adds nothing here, and I appreciate Weisbrod's work at least as much as he does. Although votes were cast, there was no electoral competition for the post of RDI president (although there was for other positions). What Feldman means by "truly contested," "victors and vanquished" I do not know; it is not my contention nor are these terms I would choose to use in describing politics within an organization such as this. But here again, it is Nocken who leads the way, this time writing that I portrayed the changes in the Reichsverband as the "result of a victorious power struggle" ["Result des siegreichen Machtkampfes"] of the dynamic industries—a rather unequal match for my statement that the RDI changes "reflected the ascent" (p. 132) of the dynamic industries. My term "chosen over" to describe the presidency was perhaps ambiguous, but the 1925 elections, undemocratic as they might have been, did result in a virtual clean sweep for the dynamic-export branches. Scholars, including myself, agree that the Reichsverband was run along the lines of a men's club rather than as a democratic polity. Indeed, clubs are not democratic, but they do have their cliques, and some of these become more influential than others. Club officers usually protest their lack of time and their reluctance to assume leadership positions. This does not preclude intense politicking behind the scenes, such as that described in Weisbrod's discussion of how Duisberg came to accept. Some people's protestations are accepted at face value (e.g., Borsig's) while some others (e.g., Duisberg) become subjected to entreaties from prominent members. This was my contention in regard to the Vorsitz (chairmanship), nothing about "a truly contested election" or "victors and vanquished." That language is Feldman's, not mine. I in no way disregard Weisbrod's facts: indeed they are essential to my analysis of changes that began when Duisberg rather than others was chosen to head the organization.

64. My own assessment of the significance of these changes is based overwhelmingly on Weisbrod's data. We know we disagree about their significance, and have discussed it as reasonable people can. What I see taking place in the 1925 RDI elections is a very important change of guard at the leading organization of German industrialists—this in contrast to the results of the preceding elections in 1919 and the succeeding ones in 1930. Paul Reusch, viewing matters from the perspective of the other faction of industry, saw it coming too—and wrote a confidant that "we have to reckon with the possibility, that for the time being leadership of the Reichsverband will pass to someone who thinks about protection of national production [i.e., commercial and social matters] differently from us." Reusch to v. Wilnowsky, December 31, 1924, HA GHH 100 101 290/39. It is

Feldman on Feldman: the Kapp Putsch (Feldman, p. 172)

It is, perhaps, appropriate to close this section on alleged tendentiousness and distortion with Feldman's charge that I have abused his work. Feldman is certainly entitled to change his mind about the Kapp Putsch on the basis of new research. But when he reads my one-sentence reference to business and the Kapp Putsch in 1979, Feldman in his reader's report voiced no objection: he wrote only, "Stinnes' support of Kapp is complicated, as is Vögler's. You must mention Kirdorf [as the third name listed—DA], not Kardorff." And contrary to what Feldman writes here in selectively excerpting his 1971 article, he went considerably further than he suggests. Of Stinnes he said, "It is possible that Stinnes was enthused by the Kapp Putsch at the very beginning." It is disingenuous of Feldman to imply that he "do[es] not discuss the attitude of Vögler and Kirdorf toward the Putsch itself" (my emphasis). For he explicitly wrote that they resisted pressure and allowed the putsch to go forward for four days before cutting off coal supplies from the Kappists, a measure that would have aborted the putsch. Further, they were pressed to "dispel the general impression that the employers had supported

Weisbrod who argues that the more conservative and protectionist Ruhr steel and mining figures, in good part through their other organizations, retained "veto power" within industry generally. And it is I who argued that the ascent of the more dynamic export liberals was rather more real. Weisbrod shows, on the pages of his book I cited (Weisbrod, pp. 220–26) how, under Duisberg, the key RDI organs were all expanded and the previous preponderance of heavy industry reversed in favor of the dynamic-export fraction. As Weisbrod shows, only 5 of 20 members of the Beirat (council), only 4 of 20 members of the economic policy committee, only 8 to 10 of the 31 members of the presidium, and only 21 of the 100 members of the Vorstand (governing board) represented the interests of heavy industry as opposed to those of the dynamic fraction. Not only do his quantitative indicators stand in distinct contrast to the situation before Duisberg's election and again after the next election, but the leading men and officers of the RDI (Kael, Büchter, Herle, Lammers, Baumer, for example) were of a distinctly liberal and dynamic bent when compared to either their predecessors or successors.

65. This claim is one of the four in this entire article that does not come straight from the Nocken typescript.
66. That sentence, p. 126, reads: "The number of actual supporters of the Kapp Putsch, for example, was quite small."
representatives, and some of the economic bases for it, will be familiar to readers of my book. Feldman claims here that his new research shows such a division to be false. But in his 1984 collection, Feldman provides further substantiation of the division I highlighted. Writing of attempts after 1930 to resuscitate industry-labor cooperation in the face of agrarian and Ruhr opposition, he writes that it was "surely no coincidence, that exactly those same people played a role here, who a decade earlier" i.e., at the time of the Kapp events, had worked for industry-labor cooperation: Hans Raumer of the electro-technical industry, Silverberg of the lignite industry, Kraemer of the printing industry, and Duisberg and Carl Bosch of the chemical industry. This is precisely the "up" side of which I make much in my book. At the same time, however, there was a "down" side, whose parallelism with responses to the Kapp Putsch events must also be noted. Thus, one of Reusch’s in-house advisors could respond to an attack on Brüning in early January 1932 by writing that Brüning was "a man who for years had been making pacts with Marxism [the SPD and unions] while treating the national freedom movement [the Nazis and/or other right radicals]... the same as those who consciously followed Moscow [the Communists] and stood outside the national community."71 Shades of 1920.

The question Feldman has posed is "what Abraham was up to," and his answer is delinquencies "no less serious than plagiarism" (p. 161). The most serious of these would indeed be fabrication, so let us turn now to those cases where that is the real allegation.

**Fabrication and Invention (Mostly with Tendentious Intent)**

*Fact and Fiction on the Stahlhelm (Feldman’s Point 9)*

Of those cases where Feldman’s real charge is fabrication, the worst allegation by far is his point 9 where I am alleged outrightly "to produce pure fiction with respect to the Stahlhelm" (my emphasis). And, although he writes that he has "personally read every document or source" he criticizes me on (n. 2), Feldman argues that *all* that Reusch could have recalled of the Stahlhelm during the 1928 lockouts was *only* "their sup-

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69. Ibid.
70. Feldman here, n. 27.
72. Ibid., p. 128, n. 37, emphasis added.
73. Feldman, Vom Welteris, p. 228, emphasis added.
74. Intra-office memo to Reusch, January 6, 1932, HA GHH 400 101 203/12.
plying—in opposition to the interests of the employers—their locked
out members with food.”

In the passage in question, I wrote that “Reusch recalled that during
the 1928 Ruhr lockouts ‘they [the Stahlhelm] performed very good
services’ as strikebreakers and thugs and should be kept active, not only
for these purposes but as an alternative to the unions for propaganda
and organization.” And in the footnote I cited a communication to Reusch
from his Berlin chief Blank (p. 319).75 Now, my description “strike-
breakers” is by definition wrong, but substitute for it “union busters,”
and we arrive at the essence of the matter. In this letter, Blank suggested,
in conjunction with both Heinrichs and Reusch’s Langenmärk
deputy, Springorum, that Ruhr industrialists deal directly with the local
Stahlhelm leadership rather than with Berlin headquarters. Why?

The document tells us something very close to what I cited and differ-
ent from what Feldman claims. According to the Stahlhelm, it had:
- rendered the employers “comradely help which worked well” [my
  “performed very good services”]
- it was a matter of the defense of the employers against the unions
- the help of the Stahlhelm negated the efforts of the unions to com-
pel unorganized workers into their ranks
- in the long run it would be desirable [my “should be kept active”]
to organize workers into an alternative “national vocational or-
ganization, which down the line and extending across the whole Reich
would struggle against the monopoly of the unions” [my “as an
alternative to the unions for propaganda and organization”].76

The imprecision in my quotation is inexcusable, but the distance from
that to the “fabrication” of “pure fiction,” is, I think, immense. Feld-

75. I think it fair to assume that Reusch recalled the matters mentioned in Blank’s letter
to him of December 17, 1928, because two days later he replied to Blank, writing, “I hold
the suggestions made to be right.” Blank to Reusch, December 19, 1928, Reusch to
Blank, December 19, 1928—both HA GHH 409/101 2024/4. In my footnote, I neglected
to provide the date of the letter.
76. Anlage 1 to Blank’s letter of December 17, 1928. This file has so deteriorated that it
is no longer accessible to scholars. Beyond my own notes, I have in rechecking my quota-
tion relied here on the German notes of another scholar, the first portion of which is in the
indirect discourse subjunctive: “die kameradschaftliche Hilfeleistung genommen haben; es habe
sich ja noch um eine Abwehr der Arbeiter gegen die Gewerkschaften gehandelt; die Stahlhelmhilfe
habe die Gewerkschaftshilfe durchkreuzt, die unorganisierte Arbeiter in ihre Reihen zu
ziehen. Beinäher möchten wir den deutschen Arbeiter zu einer nationalen Berufsorganisation
zusammenzuschliessen, die auf der ganzen Linie und auf das ganze Reich ausgreifend die Monopol
Gewerkschaften beherrschten.”

77. This entire charge, like the preceding and succeeding ones, is taken over from the
Nocken typescript.
78. “Es aufgehoben werden müssen, dass mit der Hilfe der Industrie keine zerstörende
Wirkung auf die Industrie ausgeübt wird und ungezogen.”
79. Alfred Sohn-Rethel, Ökonomie und Klassenstruktur des deutschen Fabrikations
(Frankfurt, 1973), p. 87: “Expansion of the domestic market into Mitteleuropa was from the
outset part of the program for agricultural cartelization. The first cautious announce-
ment of this wide-ranging project came in two articles in Rubin und Rohm in September
1932 (signed by Dr. Max Schleiner and Herr von Knebel, written by Dr. Max Hahn
and Herrn von Flugge).” These two men were colleagues of Sohn-Rethel and all three
were assistants to and ghost writers for Schleiner.
80. Knebel-Döberitz was, incidentally, no rustic gentleman but rather a man who, a
year earlier, had been proposed to Brüning as one of four or five men to staff an “eco-
nomic dictatorship” council consisting also of other of Schleiner’s colleagues. See Neebe,
p. 124. Several of the others had close contacts to IG Farben.
proclaim that what I wrote could not possibly be Schlenker’s position because he, like others in heavy industry, voiced support for a “free economy” (freie Wirtschaft), they are, I fear, mistaken. Manchesterites were nearly extinct species—what they understood by “free economy” was neither the absence of a strong state (which they wanted), nor the absence of regulation (they had pioneered cartels, for example), but rather the absence of socialistic influence, plain and simple. They wanted the removal of the impediments Weimar had created. Their free economy would restore the autonomy of economic elites and put an end to the need for democratic legitimation in favor of a strong authoritarian state—while they organized the economy on their own! As I pointed out in my book, “Some liberal figures in industry, such as Lange and the leaders of the Hansa Bund, spoke of the need for a ‘free economy in a strong state,’ but here the Langgässerverein [Schlenker’s organization—DA] was less ambiguous and more typical with its 1932 call for a ‘healthy economy in a strong state.’ In either version, however, organized labor and political democracy were [speaking of 1932—DA] the culprits . . . .” (p. 178).

Indeed, here again I understated my case. The industrialist newsletter for which Schlenker’s office was largely responsible, wrote of Schlenker’s and Bosch’s plans (discussed by me on the same page) that they “open up new and broad perspectives and point to a thoroughly organized German agriculture, operating with cartels and price supports. Such a partially controlled economy may not appeal to some, who still push [emphasis for the free, private economy. But one has to pose the problem, free of fruitless dogmatism and the fetishism of principles [Prinzipienfechteri], how otherwise order and new life is to be brought to German agriculture . . . .”82 A rather large step, if not a leap.

Western Industry and the Chancellor’s Petulance (Feldman’s Point 4)

The next case of alleged fabrication is Feldman’s fourth point, a “totally invented quotation.” Now, it is bad enough to have jumbled this quotation, but even Feldman seems hard put to indicate any consequence beyond its testifying to my “complete insensitivity to and lack of interest in what actually took place in the past.” In a sense, it was my attempt to set this document in context—precisely my sensitivity to all that this document said—that led me to the compression. And examination of the document at issue shows that a more precise rendering would not only be commensurate with my argument but would strengthen it. In discussing the deterioration of relations between Brüning and Ruhr industry (but not all of industry), I wrote that “Brüning’s government did not enjoy a popular base substantial enough to divide and conquer the industrialists” and their parliamentary representatives and friends. Blank begins this report to Reusch83 by indicating that Brüning had saved himself from a no-confidence vote thanks only to a great speech and an unusual spiritedness. This would ultimately not prove enough, thought Blank, to turn aside Ruhr-based pressure on DVP members to cooperate with others in toppling him. Blank portrayed Brüning as near the end of his tether: “Various people, who heard the speech, are nevertheless certain . . . that it was a totally battle-weary, tired man using his last energies who spoke, and a man not free of a certain hysteria.”

His own exhaustion and lack of a substantial enough popular base was driving Brüning into the arms of western heavy industry’s adversaries and turning him personally against western industry and its representatives.84 Blank continued that “The Chancellor has now definitively sold himself out to the Left. It is even being contended, that . . . he cannot become ‘true’ to Social Democracy, which has facilitated the work of governing for him. It is utterly regrettable, that the Chancellor has turned so sharply against the Right, without even a single expression of understanding for the patriotic motives of the best sort on the part of those circles gathered in Harzburg [the Hitler-Hingenberg et al., gathering of the National Opposition—DA] . . . . One can only form the impression that the Chancellor formulated his speech under the motto . . . . The enemy is to the Right!” Now, however complex or “muddled” one wants to call all this, my “complete insensitivity . . . .” led me, this far into the document, to underestimate significantly the manifest ill will that developed between Brüning and big western industry at this time.

82. Deutsche Fabrikbücher, November 35, 1932, emphasis added.
83. Blank to Reusch, October 14, 1931, HA GHH 3401 102 102 4/8; now in SMH, pp. 1944 ff.
84. Three weeks earlier, with western big industry rejecting his call for a lowering of cartel prices, Brüning made his well-known complaint to Catholic trade unions that: “Brüning-Westphalen [Ruhr] industry has for the last thirty years done nothing but cause every Chancellor difficulties.” ZSA, BLB Pressbuch, 11/10, p. 27, 24 September 1931.
The quotation I then proceeded to present is, indeed, poorly rendered; my note-taking was inadequate here.85

The first of the two sentences should read: "The special emphasis on the needed credits for middle and small undertakings demonstrates once again, that the Chancellor has an outright big industry complex... that perhaps in the near future all kinds of things will happen... to make real the threats against western big industry." I don't know how "credits" became "praise" and "special emphasis" became "always"; likewise how a "big-industry complex" combined with "threats against western big industry" became "petulance toward western big industry." This quotation is indeed off—though not in the service of any argument or to exaggerate the evil role played by industry. In fact, my rendering emphasizes that the chief organization of German industry, the RDI (Reichswerkband), and especially Kastl, continued to support Brüning. That, unlike much of what has been alleged, is one of my arguments. And Blank in this document expressed this contrast to Reusch: "Kastl, with whom I spoke about the talk, explained to me that he was satisfied with the Chancellor's economic remarks and that, in his opinion, the Reichswerkband need not concern itself, as to whether the Chancellor proceeds with the Right, with the Left, or with the middle. Indeed, on the Right [Kastl thinks] there is no suitable person in sight, and consequently Brüning must stay at the helm—regardless of whom he draws on for support."

No system to the error; nothing gained from it; nothing at all lost by its correction.

Government Inaction and Recalling Parliament (Feldman's Point 2)

In his point 2 Feldman alleges invented words and, worse than tendentiousness, that my interpretive sentence "makes no sense" without the invention. Quite the contrary. On page 167 I did indeed do something inexcusable: I accidentally allowed the implication of the document to become part of the quotation from it. Even if correct, an inference is an inference and absolutely not part of a quotation. Whether "the interpretive sentence makes no sense" without the italicized words we shall have to see.

What the author of the document wrote was that Brüning feared an upcoming meeting of the Ruhr industrialists' association which would "pursue the purpose of toppling the cabinet and especially him, i.e., the Reich Chancellor, through sharp attacks against the policies of the Reich government." Brüning feared, "they wanted to topple him from the West [i.e., through the influence of Ruhr industry (West), especially on the DVP-DA]" because "Western industry was no longer prepared to tolerate cabinet [government] inaction."86

Toppling Brüning would mean calling parliament back into session. The same Blank writing to the same Reusch (under somewhat changed conditions) less than a month later, reported Brüning's eagerness to avoid reconvening the Reichstag, while measures demanded by Ruhr industry and its friends, even those short of toppling him, would require it.87 As to whether the interpretive sentence, regardless of the ebbing of Ruhr industrialist anxieties over the Nazi Reichstag delegation, makes sense, here is what Neebe, Feldman's preferred source, has to say: "After obtaining Paul Reusch's assurances, Springorum [the Chairman of the Ruhr industrialists' association—DA] indicated to Sogemeier [of the Northwest German Economic Organization—DA], Blank [Reusch's man in Berlin—DA] and Heinrichsbaner [whom we will get to know below—DA], the contact man to the 'National Opposition' and the NSDAP, that he was determined 'in the appropriate form to go into opposition to the Brüning government, and... [from the platform of a Langnauverein meeting] to protest the policies of the government in every form and with the fullest determination.' This would only be possible... if ahead of time consensus could be formed among the leading men of the Ruhr as to the appropriateness of such an action."88 This latter set of developments dates from the last week of April 1931, i.e., almost three weeks past the letter I cited. Not only were these men presumably aware of the size of the Nazi delegation, but one of the three men to whom the news was first broken was heavy industry's chief contact man to the NSDAP. There is no fraud, the interpretive sentence makes sense, and it may even represent an understatement of the position of the protagonists.89

88. Neebe, p. 90 and Chapter 5 of Turner, "Big Business Responds to the Depression and the Nazi Breakthrough," where my interpretive sentence will be backed up and developed.
Harms, et al. (Feldman's Point 1)

The "final" charge of fabrication is levelled by Feldman in point 1, where "most of this quotation [is] invention." In addition, there are charges, incorrect, of misidentification and of "gross distortion."

In the passage in question on page 91, I wrote: "With the exceptions noted earlier, the agricultural specialists in the scientific-academic community continued after 1928 to support the agrarian ideology of a protected and united rural sector. Like Max Sering, Edgar Salin, and others, Bernard Harms, director of the Friedrich List Gesellschaft, reflected the views of that community when he wrote, ..." (italicized words elided here by Feldman). "Bernard Harms, ed., Strukturanalyse der deutschen Volkswirtschaft (Berlin, 1929), 1:23-24. See also Edgar Salin, 'Am Wendepunkt der deutschen Wirtschaftspolitik,' in Friedrich List Gesellschaft, Agrarpolitik 2:684-733."

I have dealt above with my (incorrect) identification of Harms as the Friedrich List Gesellschaft's Vorsitzender, so let us proceed. Sering and Salin were at the core of the national school (Sering was its "dean"), Harms somewhat less so. Few scholars—not Harms, Salin, or Sering—denied Germany's dependence on industrial exports, but that by no means excluded supporting a protected and united rural sector, even for Harms. My point was one of illustration, qualified in the manner indicated by precisely those names and phrases Feldman has elided. The latter portion of my quotation, following the break, is poorly rendered but nevertheless from the second source cited, deficiently, in the footnote. Salin. The illustration does not depend on the error—Salin is, after all, named in the text as holding this view as well as in the footnote, and correction would only yield language stronger than that there. There is nothing systematic about the error, and correction would not weaken the point made.

"Simply Cannot Be Found"

There is, in fact, one last charge of fabrication, though Feldman only suggests it: "The statistics Abraham gives for lignite production simply cannot be found in Hoffmann" (p. 173). There are, to be sure, statistical errors in my book, but what lies beneath this accusation of making up these figures? Reproduced below are Hoffmann's lignite figures from the table I cited and my own figures from the table in question.

<table>
<thead>
<tr>
<th>Production Index (1913 = 100)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1925</td>
</tr>
<tr>
<td>Hoffmann (pp. 342, 3)</td>
</tr>
<tr>
<td>Lignite (column 2)</td>
</tr>
<tr>
<td>[Braunkohle]</td>
</tr>
<tr>
<td>Lignite Briquettes (column 13)</td>
</tr>
<tr>
<td>Abraham (p. 144, Table 15)</td>
</tr>
</tbody>
</table>

Lignite

Imperfect, unweighted averaging of lumps and briquettes, but is insinuating fabrication warranted? (Again, too, the higher figures would have been "better" for my argument.)

* * *

CHRISTMAS 1930

Only omniscience could explain some of the claims Feldman makes in regard to the Christmas 1930 documents and my supposed "modus operandi" in dealing with them, and, indeed, "the character of Abraham's self defense". My modus operandi for Feldman is simply to lie. Here Feldman writes that he neither knows nor cares what my motive for repeatedly lying might have been, but it is fair to assume that both his endorsement of Nocken here (n. 1) and his earlier mass distribution of that typescript suggest that he agrees that this was for "the systematic purpose of portraying industrialists as closer to, friendlier toward, and more supportive of the Nazis than they really were. Or, as Feldman puts it here: "to portray Reisch as an early example of industrialists' support of the Nazis" (p. 166). Feldman has presumed to raise a syllogistic structure of premises, implications, and conclusions about the materials, the charges, and my actions, which he concludes were a condemnable "cover up" (p. 170). It is false. Working one's way through his three-page web of "had he's, "would have's," "so as to's," "instead of's,

89. Stated here pp. 161ff. and elsewhere numerous times. Most directly--"I submit Abraham knowingly lied"; Feldman Rundschreiben of February 26, 1984 accompanying his distribution of the Nocken typescript, from which all of the charges here come.
writes of “misidentifications, false datings, false attributions, and false connections” (p. 165), as well as of “misquotation, tendentious misconstrual, outright invention of archival citations, and outright invention of the content of documents” (p. 165). For Feldman these passages of 20 and 7 lines represent not a statistical frequency of error but an archetype, an emblem of the mendacity and tendentiousness of the author and worthlessness of the book.

I shall examine the points being made in my passages, the texts themselves, the errors made, and the consequences of correction. But first, the cast of characters in the Christmas 1930 drama:

Paul Reusch: A leading, politically and organizationally active Ruhr industrialist (headquartered in Oberhausen). Recipient between December 9 and December 23, 1930 of two reports on the current political situation and the National Opposition—particularly as they concerned right-wing political options for business. Author of two letters to Edgar Jung, December 29, 1930 and January 2, 1931 discussing one of the reports (possibly also a draft version) and creation and funding of a “movement” aimed at creating one unified “national right.” Already funding other similar efforts.

Martin Blank: Reusch’s Berlin bureau chief and a leading political advisor and contact man for Reusch. Possibly the most frequent of correspondents in the file. Works together on numerous occasions for a period of several years beginning no later than 1928 with August Heinrichsbaumer and shares his office with Erich von Gilsa.

August Heinrichsbaumer: Chief “contact man” between Ruhr heavy industry, the National Opposition and the Nazis. Heavy industry’s leading publicist, heads a press and research bureau funded by Reusch concerns, including Reusch’s. Works with Martin Blank on numerous projects involving groups on far-right: his memorandum frequently reach Reusch via Blank. Probable author of the anonymous “Dez 1930” report (dated in newest scholarship between the 9th and mid-December). No evidence of direct correspondence between Heinrichsbaumer and Reusch. Heinrichsbaumer aware and supportive of the activities of Edgar Jung.

Edgar Jung: Based in Munich, author of far-right books and speeches subsidized by Ruhr industrialists including Reusch. A paid political consultant to Reusch and political organizer. Author of a report on meetings of December 10, drafts or versions of which were sent to Reusch between about December 23 and December 29, advocating building movement of the unified Right, the precise constituents of which are unclear. This report probably no longer preserved, but Reusch’s letters of December 29 and January 2 addressing it/them remain extant.

from its economic utopias, and analogizing between NSDAP and Mussolini's responsibilities once he was in power, and letter of January 31, 1931 calling for Rensch and other industrialists to use "all resources at their disposal" to bring about a unified national trade including the Stahlhelm, Hilter, Hugenberg and others.

Report 51: "Dez 1930," anonymous, no covering letter of any sort preserved, lodged in chronologically ordered bound volume following material of December 19, 1939 and immediately preceding Rensch's letters to Jung. Written from big businessmen's point of view, it reports on a series of talks with National Socialist leaders, advocates an end to half-way politics, recommends working more closely with rapidly growing and popular National Opposition, which Nazis are a part. Concludes that working with National Opposition is best way for entrepreneurs to "recover lost political leadership." The report is red-pencilled in the manner often done by Blenk for Rensch and contains few marginalia.

Report 52: No letter preserved, authored by Jung: one or more drafts/copies received by Rensch between December 23 and 26. Contents can only be inferred from Rensch's letters to Jung, but report(?) may discuss business's role in creating and helping to finance "one national Right free of particularisms."


What Point Was I Making

To begin with: What I thought I had come upon in an anonymous "Dez 1930" report in Rensch's files was a rather unexceptional illustration of two widely accepted points—(1) growing impatience and disappointment with Chancellor Brüning among significant Ruhr industrialists, and (2) emerging interest in the possibility of taking the distrustful but not unprepossessing Nazi mass movement through linking it to or submerging it in other existing conservative, "national" groups mentioned often by Rensch, like the National Opposition with the anti-Weimar German Nationalists (DNVP) or with Hans Luther's anti-parliamentary and anti-socialist League for the Renewal of the Reich. 93

In this document, located immediately after an irrelevant note of December 19 in a chronologically ordered bound volume, and in the two letters immediately following it and that I took to be discussing it.

93. It did not occur to me that I might have been one of the very first scholars to see this report. As I learned only this year, eight years after my research. Nebe describes it in his monograph, "Führer unternehmer gebildet"—p. 117. I missed a scoop! The documents referred to in this discussion, unless otherwise noted, are located in HA GHI 149/725 29, 1931.

I saw further confirmation of an unremarkable and not particularly original position: "that the split between the liberal (pre-Brüning) and national (pro-Hitler-Hugenberg) factions of industry occurred almost as soon as the outlines of Brüning's program became clear" in early 1931. 93 That was my first conclusion, on page 165.

My second conclusion, on page 316, added that Rensch (1) had begun to despair of a bourgeois concentration as early as Christmas 1930 and suggested instead off-the-record funding directed toward "crystallizing the bourgeois right and the NSDAP into one." That is, that an all-bürgerliche concentration (Sammlung) was impossible without some role for the National Socialists and that, (2) the key goal of "unifying the Bürgertum rightward" would be to "bring the NSDAP away from its economic utopias and make it capable of governing" in conjunction with established conservative forces. I reached these conclusions on the basis of the same documents along with other materials from the same period in another folder to which I referred in the relevant footnote (but not reproduced here by Feldman). 94 My second conclusion also did not startle me as particularly new, except perhaps that it suggested a somewhat earlier starting point for these developments than I had previously thought on the basis of the literature. When all is said and done, and with a precise identification and reading of these documents, these conclusions will stand with only one possible revision: Rensch, as opposed to some of his leading advisors, may have held out hope for an all-


94. See Abraham, p. 315, n. 113. The additional materials are from the von Gilsa file. 4b. These materials make it clear that a role for the NSDAP was definitely intended, at least by Rensch's advisor von Gilsa, and financial resources marshaled.
bourgeois concentration a bit longer than I thought. This makes a
difference and necessitates reconsideration of this piece of evidence. But
whether he did or not, in no way does any of what I wrote approach a
portrayal of “Reusch as an early example of industrialists’ support of the
Nazis” (Feldman, p. 166). I wrote of a united Right which could include
the Nazis, not of the Nazis.95

In sum, I thought there was barely anything novel in what I concluded from
these illustrations.

Identifying the “Report” and Its Author

In the AHR96 I identified Edgar Jung as the likely author of the anonym-
ous December 1930 report in place of Martin Blank. Both of these
identifications were wrong. I am now (November 1984) reasonably
persuaded that there were two discussions, “Besprechungen” or actually
sets of “Besprechungen,” which issued in reports and became the object
of Reusch’s interest at almost exactly the same time in December 1930.
One of these “Besprechungen” involved Nazi leaders and others and
was reported on by Heinrichsauer on December 9, according to Neebe,
or around mid-December, according to Grüber.97 A second set of
“Besprechungen” took place the next day, December 10, probably in-
volving some leading industrialists and political figures, and these “Bes-
prechungen” were perhaps organized by but certainly reported on to
Reusch by Jung.98

I seem to have mixed up or conflated these two reports, taking #1 to
be a version or draft of #2, the report discussed in Reusch’s letters to
Jung. The internal references in these letters are very unclear and, given
the similarity of the topics and the absence of any materials pertaining
to #1, it was only in 1984 that I came to realize that they were separate
entities. In my original research and notes I held Blank and Jung to be
correspondence for the anonymous report, but since Jung made no other
appearance whatsoever in my work, I ascribed authorship to Blank
alone. Over seven years later, in 1983, this was no longer clear in my
memory, and, returning to the materials, the Jung identification imme-
diately seemed clear and unproblematical.

As noted, however, the internal references are complicated and have
apparently confused Feldman as well. Thus, in his February 26, 1984
circular introducing the Nocken typescript, Feldman wrote that the
“letter of January 2 makes specific reference to a document sent by Jung on
December 31 which was being returned with the suggestion that it be
destroyed . . .” (emphasis added). Now, Reusch’s letter to Jung of
January 2, 1931 is indeed in response to what he there calls Jung’s
“Schreiben” of December 31, 1930. But the immediate object of discuss-
ion in the January 2 letter cannot be and is not, as Feldman claims, the
“Schreiben” of the 31st itself, but rather one or another version or copy
of another document, which Reusch refers to in that letter as the “Denk-
scrift.” That Reusch could write to Jung several days earlier, December
29, 1930, already referring to some first version of “the” document (“Ausarbeiten,” “Entwurf” are the words Reusch uses) indicates that
some form of it was already in Reusch’s possession before the date Feld-
man claimed, December 31. To correct his own earlier error or omission,
Feldman here adds (p. 163) “and to another document sent on or about
December 23” (emphasis added). What Feldman has had to do to make
sense of his understanding of the exchange is precisely what I had to
do to make sense of it—conclude that “the report probably arrived in the
week preceding”99 our original datings. Indeed, Feldman’s recognition of
the necessary existence of another document from about a week
earlier is relatively recent. His first version of this article contained no
mention of it.100

Further, whereas in February Feldman claimed that there was a spec-
ific “formulation” referred to in the letter but absent from the report,
he now writes that there was general reference “to the content” of a
subsection (IV-3) of the document of December 31 [sic]. I took that
reference to be to an issue—gathering in the politically homeless—that
I saw alluded to in point 3 on page 3 of the anonymous report. Given
that more than one version had been sent to Reusch, I did not find the
absence of Roman numerals significant. Perhaps if I had seen the report
and January letter in isolation from the other letters, as Feldman and now

95. The difference between Hitler or the Nazis as such and the National Opposition
(consisting of Hugenberg’s established DNVP, the Stahlhelm, and the Landvolk as well
as the NSDAP) is so well established. I cannot understand why Feldman ignores it. The
same holds for Nocken’s version of this charge: my claimed “proof of the early emer-
gence of a pro-Hitler faction” (p. 3).
97. Neebe, p. 251, n. 9; Grüber, p. 422, n. 804.
98. Details in the Krupp Archiv, IVE, 152, 776. See also Grüber, pp. 444–25. I have
not been to the Krupp archive.
100. I owe one of the German recipients of that draft thanks for bringing this discrep-
ancy to my attention.
others have, I might have been more quizzical. Having been accused of
making up a document, I was relieved to find it.

Reusch’s Response to the Report

Reading Reusch’s January 2 letter as a reply to report #1, I assessed
his response to be both direct and indirect. On the indirect level, there
are underlinings under and in the margins next to particular statements
in the report; these were applied in heavy red pencil in Reusch’s office—
a practice familiar to users of these files (and described by his Berlin
man Martin Blank)—as were marginalia, like “wichtig.” Some of these
heavily underlined phrases were quoted from by me, such as “The
time for a so-called bourgeois concentration is past” [“Die Zeit der sog. bür-
gerlichen Sammlung ist vorbei”]; “What is decisive, is much more the
concentration of the national opposition, to which I reckon above all the
NSDAP, German Nationals, Stahlhelm and Landvolk” [“Entscheidend
ist vielmehr die Sammlung der nationalen Opposition, zu der ich vor allem
rechne die NSDAP, Deutschnationale, Stahlhelm und Landvolk”]; and “The
essential goal must be maintaining and building the unity of the national
opposition” [“Das wesentliche Ziel muss sein die Aufrechterhaltung und der
Ausbau der Einheitlichkeit der nationalen Opposition”]—translated by me as
“We must maintain and build the unity of the Hitler-Hugenberg oppo-
sition” and italicized here by Feldman]. 101 This presumably because of
the intention to “pick leaders for the masses from among those people
already available” [“den Massen aus den bereits vorhandenen Leuten heraus
die Führer zu geben”]—translated by me as “pick a leader from it” and
italicized here by Feldman.] The comments by Goebbels and Strasser
were not underlined, only their names were and that by typewriter.
Hence some of my inferences about what Reusch was referring to and
commenting on in what I took to be his return letter may have been
thin, though plausible. My comment that Reusch was “particularly
impressed” by what Goebbels had to say was not justified. We cannot
know, and I apologize to the reader.

In Reusch’s letter to Jung of January 2, 1934, Reusch instructs Jung to
gether in the various copies of the current version of an otherwise
unidentified report from its other recipients and to destroy all of them,
“wenn Sie von den einzelnen Persönlichkeiten zurückhalten haben, restlos
zu vernichten.” While Reusch does not otherwise identify the report, for

the reasons indicated above, I took it to be the “December 1936” report.
There is no “ludicrous” or “dramatic presentation of Reusch’s effort
to have all the copies destroyed (Feldman, p. 166): on the contrary, not
wanting to risk overestimating Reusch’s support for a report whose cur-
crent version he certainly did ask to have destroyed, and allowing for the
possibility that he was simply flattering its author when he praised the
“many good ideas” [vielen guten Gedanken] it contained and which
should be retained in a future version, I did not attribute to Reusch
endorsement of the report’s conclusions. I did, however, infer that the
points that drew his attention in the copy he had marked for him and
apparently kept in his files were related to those on which he compli-
mented the author. At that, my conclusion was rather minimal.

“Nicht”

What about the missing “nicht” in Reusch’s response to Jung? What
Feldman called misquotation “in the most serious manner imaginable.” 102
Yes, in my original [notes I did omit the word “nicht.” In view of all the
charges levelled here, the reader may be incredulous, but that omission
was simple and innocent. It did not strike me along the way as incon-
grous however, because, without the not, the sentence makes sense in
its own context.

How?? The sentence in which I missed the “nicht” reads: “I do not
see the task of the movement as being such, that we should fit a point of
crystalization between the bourgeois Right and National Socialism.”
And the very next words read: “The movement should much more
have in mind the creation of one national Right, which is free of the
representation of petty interest standpoints, as is now by and large the
case with the bourgeois parties.” [“Ich sehe die Aufgabe der Bewegung
nicht so an, dass wir zwischen der bürgerlichen Rechten und dem National-
socialismus einen Kristallisationspunkt einbauen sollen. Der Bewegung soll
vielmehr die Schaffung einer nationalen Rechten vorschreiben, die frei ist von
der Vertretung der kleinsten Interessens-Standpunktes, wie dies jetzt bei
den bürgerlichen Parteien im grossen ganzen der Fall ist.”] Thus, it seemed
to me that what Reusch wanted to do was bring together, into one, the
various elements of the “national Right.” This error, in no way delib-
erate, shifted the meaning of Reusch’s position—this is the difference
noted above—but hardly qualifies for the claim Feldman levels: that

101. The next sentence identifies the National Opposition’s leaders: “hauptsächlich
führenden Persönlichkeiten (Hugenberg, Hitler, Seldte).”

102. February 26, 1934 circular letter.
Reusch adopted "a negative position which Abraham converts into a positive one" (Feldman, p. 165). I thought there was further confirmation for my reading in Reusch's observation to Jung that the "Bewegung" could only be successful if the industrial "was not to be involved for now" but that the movement instead "proceeded along the Berlin-Munich line," Berlin being the seat of the bourgeois parties and Munich the seat of the NSDAP. Hence it was reasonable to think that the latter was somehow to be included in the "Bewegung's" goal of a united "national Right." Now, when I discovered in July 1983 that I had missed the "not" and the between/together distinction, I pointed this out in a letter to Turner, a fact of which Feldman was unaware. 103

Off-the-Record Funding

Feldman has also italicized the phrase appearing on my p. 316 pertaining to what I called "off-the-record funding," suggesting that it is either wrong, tendentious, or an invention. The letter of January 2 makes clear that Reusch and Jung shared an interest in organizing this "Bewegung"—a "Bewegung" I erroneously identified with that discussed in the other December report—and both Reusch and Jung were aware that this would take money (and that the financial role of Ruhr industry should not be overt—the industrial "Wesen zuletzt nicht in die Erscheinung treten"). Reusch wrote Jung, first, "I do not believe that the movement, either sooner or later, will be able to support itself financially through the masses . . ." (Actually it seems to me now that this sentence might have alerted me to the possibility that Jung's "Bewegung" was not the "Bewegung" of the December report.) Reusch then continued, "In regard to the question of financing, you will want to consider, that it is at this moment out of the question, to raise the types of sums that you and—as I must openly agree—also I have in mind for the future. One has to start small . . ."

Bringing the NSDAP From Its Utopias

For whatever reasons, Feldman has deleted several key words from his reproduction of my text and footnotes. Thus, he ends my p. 316, n.

103. I take this opportunity to correct the other errors and inexactitudes in my rendering of the December report and the Jung-Reusch letter. My corrections also indicate that neither the report nor the letter were written in the first person. ("Der Abenteurer[(sic)] und der Staat[(sic)]") (p. 341), but his American translator Feldman certainly was, since he was a recipient of my letter to Turner (Nov. 18, 1983) and he responded to it.

113 (p. 165) with ellipses, whereas my footnote continues: "On later warning the Nazis, Gilsa to Reusch, ibid., [i.e., 190 101 293] ab." Here, Reusch's confident and former Reichstag delegate, Erich von Gilsa, 104 a man who shared Reusch's Berlin office, wrote to Reusch, also in January 1934, urging Reusch and other economic leaders to use "all reserves at their disposal,"—(underscored in the original) "[all die ihnen zur Verfüen stehende Mittel"]—"to help bring about a united national Right consisting of the Stahlhelm, Hitler, Hugenberg, and other cooperative members of the bourgeois Right. 105 Although Reusch may not always have entirely shared von Gilsa's opinions, he was informed and affected by them. In sum, it seemed to me that Reusch's remarks to Jung, together with the other materials cited lend substantiality to my argument.

Further, it is that file, which provides further evidence not only for "off-the-record funding to crystallize [sic, joint connect] the bourgeois line down and the NSDAP into one" but also for my flawed but not invented rendering that "unifying the Bürgertum rightward would be to bring the NSDAP away from its economic utopia and make it capable of governing." Thus, this same Gilsa, again in the file I cited in the footnote in question, wrote to Reusch already on October 20, 1930 expressing anxiety over a "left turn" on the part of the Catholic Center Party and the Brinling government. To prevent the left from short-circuiting the "strong movement to the Right running through our nation" and "manifest in the last [September] elections"—in which the NSDAP accomplished its major breakthrough—he proposed that: (a) the Bürgertum must now adopt, without wavering, an unequivocal attitude of struggle [Kampfstellung] against socialism; (b) Hugenberg must be ready to cooperate on this basis; and (c) the National Socialists must bring themselves away from their economic and fiscal utopias. This turn is not to be ruled out, despite the radical agitation of the National Socialists. The Fascists in Italy in 1922/23 also raised rather similar demands, which were then thrown overboard when Mussolini came to power and had them assume responsibility. . . . The education [Erziehung] of the National Socialists to sensible economic thinking will proceed all the more quickly, the stronger a truly right-oriented and active Bürgertum is on the scene.

104. See Weidbrod, pp. 467-74; Abraham, p. 472.

105. This letter dates from January 21, 1934, among others, all along the same lines, are those of July 16 and August 12, 1934 and others.
next to them.”106 Sympathetic tutelage, rearing, if not actual crystal-
ization. Von-Gilsa’s letters suggest that in the course of the coming
months, and by no later than October 1931, he had won Reusch’s active
assistance, also financial.

What Did Goebbels Say?

I wrote that Goebbels said “wage cuts for the sake of reparations and
the current system are unacceptable, but in a national system they would
be acceptable.” The December 1930 report reads, “He [Goebbels] sees
relations to the economy, to the entrepreneurial class purely politically.
In the event that the entrepreneurial class goes along with the national
opposition, he is glad to recognize and promote it; should the
entrepreneurial class however stand up for support of the current sys-
tem, he will fight it mightily. Goebbels said, that it is unacceptable to
put through wage cuts for the sake of the current domestic system and
reparations; he must unconditionally reject support of such measures.
On the other hand, he would put himself completely at the disposal of
the entrepreneurial class, if it for example declared, that it refused all
financing of the current system (also by tax payments) and turned with
the workers together against continuation of the bleeding dry of the
economy.”107

Comparing the two, one sees that the first part of my quotation is
skeletal, though with no change in meaning. The second part of the
quotation is not a direct quotation, but is it a distortion, one that helps
my argument? I do not think there is any exaggeration of what it was
reported Goebbels said he was prepared to offer in exchange for entre-

presurial cooperation in a national system.108 Strasser, following Goeb-
bells in the report, hammered home the latter’s point: “If it is for the
sake of freedom for the German economy,” — the lack of which was the
constant entrepreneurial complaint — “then no measure (also in the area
of cost-cutting) is unpopular enough for the NSDAP.”

Edgar Jung

Who was Edgar Jung, the man whom I mistakenly identified in the
AHR as the author of the anonymous report? And what is the con-
sequence of correcting the identification? Edgar Jung was a Munich-based
lawyer, lobbyist, author of books such as, The Domination of the Inferior:
Its Decomposition and Replacement by a New Reich (1927, 1930) and
The Meaning of the German Revolution (1933) and, for a while, a speechwriter
for Baron von Papen. A proto-Nazi radical conservative national intellect-
ual with an interest in racial questions, social Darwinism, and Italian
fascism, this “precursor of a terror regime” enjoyed a brief but lively
career as far-right political organizer and consultant. By 1930 he was
taken very seriously by serious and important politicians and business-
men, for whom he sometimes worked. Reusch cannot be immunized
here. After January 1933 Jung seems to have run afoul of certain impor-
tant people, for he was shot during the purges of June 1934.

In my AHR reply, I referred to Jung as an “adjunct employee of
Reusch’s in Munich.” Feldman here complains that this description
“awakens a false impression of a formal tie between the two men.”109 I
based my description on the following kinds of information: On Janu-
ary 21, 1931, Fritz Springer, first Reusch’s deputy and then his suc-
cessor as chairman of the Ruhr industrialists’ organization (Association

106. Von Gilsa to Reusch, October 30, 1930, ibid.; now also in SAM: pp. 437, 458. My
emphasis. Gilsa observed that there were “many who believe” that they could help move
the NSDAP in the right direction by “joining the party and to a certain extent transform-
ing it from within.” This idea has something to be said for itself.” But Gilsa nevertheless
rejected it on the grounds that there were not enough people with the right perspective
around to pull it off.

politisch. Für den Fall, dass das Unternehmen mit der nationalen Opposition geht, ist er
zu seiner Anerkennung und Förderung bereit, setzt es sich des Unternehmen jedoch für die
Unterstützung der jetzigen Systeme ein, wird es auf das Schwere belassen. Goebbels sagte,
dass es nicht zu versuchen sei, Lohntarifzügezüge des jetzigen inpolitischen Sys-
tems und schwinden einer Reparationsausführung durchzuführen, eine Unterstützung derartiger
Maßnahmen muss er unbedingt ablehnen. Der betrieblich verantwortliche Unternehmer
steht zur Verfügung, falls dieses beispielweise erklärt, dass er jede Finanzierung der jetzigen Systems
(schließlich für die wirtschaftliche Entwicklung) ablehnen und sich mit der Arbeiterschaft gemeinsam gegen die Fort-
setzung der Aktivitäten der Wirtschaft wehren.”

108. On the later course of wages, national income, and worker consumption, see R. J.
Ovsey, The Nazi Economic Recovery (London, 1982), pp. 37, 64; Gerhard Bry, Wages in

109. P. 164 above, my emphasis. In his February 26 circular, Feldman claims simply, “I
don’t know what an ‘adjunct employee’ is” and leaves it to the accompanying Nocken
text to elaborate. Thus, (Nocken, p. 4): “Verwundbar nur weil Jung einige Anschläge von der
Reichskanzlei bewirkt hat, finden sie in der Hauptsache durch die Notwendigkeit finanzielle
Unterstützung für seine politischen Projekte gesucht hat, wird er von der Gesellschaft der
GHH-Konzerns bezeichnet” (emphasis added). Now, I did not identify Jung as a toady or
any other kind of employee of the Reichsfinanzministerium, the core of Reusch headed.
And my identification of Jung had nothing to do with the newspaper in which he pub-
lished, as Nocken “suspects.” Feldman’s source refers to Jung as “Verführer einer Terrors-
regime.” See Bernhard Jeschke, Zur Kritik der konser vativ-revolutionären Ideologie der
for Furthering the Joint Economic Interests of the Rhineland and Westphalia, Langenauverein) and of the Northwest [Ruhr] Group of the Association of German Iron and Steel Industrialists, wrote to Jung committting himself to continued funding for Jung's lectures and writings. Not only did Reusch receive a copy of this letter, but it also speaks of the participation of a close associate of Heinrichsauer. On June 11, Reusch confirmed this connection. On October 19, Jung wrote to Reusch apologizing for his recent silence (blamed on his father-in-law's death and children's illnesses) and enclosing his formal work report ["heiligender Täglicheschatz"] based largely on conversations and travels, including to the Bad Harzburg meeting of Hitler, Hugenberg, and the rest of the National Opposition front. The tie between Jung and Reusch was real and formal. There is no false impression awakened, and there was no assertion on my part akin to that invented, that I made Jung into an employee of Reusch's firm.

My Jung inference was plausible and reasonable, but it was wrong. That means that Reusch's correspondence with Jung was not about the anonymous report and that there is less basis than I thought on which to assess what Jung might have thought of Goebbels's and Strasser's remarks. Jung himself plays absolutely no role in my narrative or analysis, and his loss as illustration is fully insubstantial. But what of the actual author of the December 1930 report, August Heinrichsauer? That is a different story.

August Heinrichsauer and His Relationship to Reusch, et al.

If this anonymous "December 1930" report, written according to Neebe and now Grubler, by Heinrichsauer and probably red-pencilled and circulated by Reusch's Berlin representative Martin Blank, was not what Reusch wanted destroyed but rather sent on for the consideration of others, it might suggest considerable interest in the National Opposition already by the end of 1930. Emphasizing the Reusch-Heinrichsauer connection would do far more to portray Reusch as an early example of industrialists' support for the Nazis" (Feldman). I still have no real interest in doing this, but, in researching this problem, archival materials I have seen recently indicate that this connection, via Blank, Springorum, and Max Schlenker was considerably tighter than I thought. Heinrichsauer was much more than Feldman's "right-wing, pro-industrialist journalist" (Feldman, p. 164), as the sources he cites make clear—beginning with Neebe's description of Heinrichsauer as industry's "liaison to the National Opposition and NSDAP," the man responsible for the "surprisingly positive evaluation of Gregor Strasser in the ranks of Ruhr industry" and "mediator between heavy industry and National Socialism." Heinrichsauer worked for, represented the Mining Association and other heavy industries in important ways, and possessed a

"absolutely reliable man," Heinrichsauer. And this key man, this absolutely reliable man, appears exactly once in my entire book and that in a footnote. Were I interested in either tarring Reusch or unmasking "the early emergence of a pro-Hitler faction" in industry, then Heinrichsauer would have been my man.

113. Pp. 320-21, n. 127. My text and note read as follows: "its [the NSDAP's] economic platform remained ambiguous. One should not, therefore, assume that the industrialists' efforts at "enlightenment" were bound to bear fruit; the point is that they considered this the necessary and most promising avenue." 114. Nurnberg, "Faschismus," pp. 12, 40 ff., 62, 78. On the Nazi side, Walther Funk was particularly receptive, and Hans Reulke's pamphlets sought to demonstrate the complete compatibility of National Socialism and private property of whatever size. August Heinrichsauer also worked at harmonizing the economic views of the Nazis and Ruhr heavy industry. One culmination of this effort was Hitler's speech to the Düsseldorf Industry Club of 27 January 1932, see Bracher, p. 365. Crichlow, p. 27, Krohn, pp. 127 ff.

115. On the regularity with which Blank read and red pencilled materials for Reusch, especially those originating in Berlin, as Heinrichsauer's reports did, see, for example: Blank to Reusch, January 27, 1931, HA GHH 400 101 202/485; and Blank to Reusch, April 14, 1931.

116. See Neebe, pp. 93, 145, and 250, n. 5 respectively. Also, pp. 95, 107, 117-19, 122; and Grubler, pp. 202, 422.
considerable budget, part of which did come from Reusch’s firm.116
More importantly, through Reusch’s assistants, deputies, and associates, Blank, Springorum, and Schlenker, Heinrichsbaier enjoyed regular contact with Reusch, who, for his part, was rather aware of Heinrichsbaier’s activities beginning no later than early 1929. There was more here than I had imagined. For example, already in June 1929, Blank and Springorum worked with the Stahlhelm (to which Reusch was later proposed for honorary membership) in opposing the Young Plan.117 In the six months following Christmas 1930, Heinrichsbaier’s activities with Reusch’s assistants, deputies, and associates and on Reusch’s behalf accelerated. They included: still in January 1931, consideration of how to topple Brüning and with whom or what he might be replaced;118 observation of activities very much like those of Jung;119 a close watch on the development of relations between Hugenberg’s DNVP and the NSDAP;120 immediate notification of Heinrichsbaier of the April decision to oppose Brüning;121 Blank’s initiation of contacts in June with Hitler’s new economics advisor and the NSDAP press bureau;122 the rapid extension of those contacts to the head of the NSDAP economic policy division for the purpose of formulating an economic program for the party and a German Economic Manifesto;123 and, also, planning for the October meeting of the combined National Opposition.124
These are some of the developments in the thickening of relations between some of Reusch’s agents and associates on the one hand and the NSDAP portion of the National Opposition on the other that took place within six months or so of Christmas 1930 and which are documented in one single archival file. In many cases, we do not know how Reusch responded. More research could fill in some of the gaps. What we do know is that, during the critical second half of 1931, increasingly divergent views came to manifest themselves within industry. As I made very clear, on the one side, Carl Duisberg, Ludwig Kastl, and the former Reichsverband der Deutschen Industrie leadership, as well as groups still under the sway of the exporters and dynamic fraction of industry, continued to support Brüning and his policies. On the other side, Reusch, the Langnamverein and Ruhr heavy industry generally moved into both severe opposition to Brüning and increased support, of different sorts, for the National Opposition.125
Heavy industry's move toward the National Opposition, what Nebe calls a "schrittweise Annäherung von Grossindustrie und Nationalsozialis-
mus" developed after Christmas 1930 in numerous ways, including: indirect and direct support of the Harzburg gathering of the National Opposition (Hitler, Hugenberg, et al.) in October 1931; Reusch's Au-
gust 1931 counsel to the chairman of the Bavarian People's Party strongly discouraging disagreement between that party and the NSDAP on the grounds that "differences within the national circles" had to be "avoided"; Reusch's toleration, at the very least, of von Gilsa's efforts, also beginning in October 1931, to purge the DVP of Brüning sup-
porters and to use financial leverage to merge its rump into the National Opposition with Hugenberg and Hitler; Hitler's successful speech to the Düsseldorf Industry Clubs on January 26, 1932; Reusch's directives in the Winter and Spring of 1932 to newspapers of which he was part-owner to desist from criticism of Hitler or the National Socialists; and Reusch's joining with Schacht in the latter's "Arbeitsstelle."

Much thus happened in the months following Christmas 1930, and all of these developments culminated in a manner perhaps best described by Nebe: "Of decisive importance in the reorientation process of Ruhr industry was Paul Reusch's change of position, assuming an oppositional stance toward the Brüning cabinet in August/September 1931. [I initially dated this change from January 1931; Weisbrod dates it Spring 1931–DA.] On the occasion of the presidential election of Spring 1932, marked by the choice between Hindenburg and Hitler, Reusch opted for the side of the 'National Opposition'. . . . Within western industry, Reusch's change of position was of decisive importance in the Ruhr captains' [Ruhrhade] failure to provide any funds for the reelection of Hindenburg. . . . Seen against this background, the first meeting be-
tween the leading authority of the west [Ruhr], Paul Reusch, and Adolf Hitler in Munich on 19 March 1932 . . . played a signal role." 128

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reader turning to Geary's article would be hard put to recognize it or its treatment of my work from Feldman's reference. And his quotation on p. 97, taken over from me, is correct.

126. Nebe, p. 159.
127. See v. Gilsa to Reusch, October 22, 1931, December 3, 1931, December 18, 1931 in HA GILH 400 101 293/4 where these plans are put forward in detail.
128. Nebe, p. 120.

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To Conclude

In conclusion, I must stress that both my original and second descriptions of the materials contained an error of attribution and association and that my original rendering of the texts was not without flaws. Among these, there is an error that "helped" my case (taking Reusch's remarks in his letter of January 2, 1931 to have addressed the anonymous December 1930 report). There is an error that superficially appeared to "help" but did not really (the entire "crystallization into one"/"not fitting a crystallization point between"/"creating one national Right"/loop—including the "nicht" question). There is an error, ultimately the key error, which actually "hurt" my case (not identifying Heinrichsbauer as the author of the anonymous December 1930 report). And there are errors that neither help nor hurt in their consequences for meaning (in the rendering of Goebbels's and von Gilsa's remarks).

But there was no deliberate or substantial misquotation or misconstrual; no reversal of meaning; no fabrication or invention, "outright" or otherwise, of either archival citations or the content of documents. There was no false or miscalculation of the ties among the correspondents. And there neither was nor has been any deception or lying.

(See Appendix.)

Analysis of the materials I cited, further archival materials, and the findings of other scholars have I believe shown that Feldman is wrong both in his parody of what I argued—"to portray Reusch as an early example of industrialists' support of the Nazis" (Feldman, p. 166, my emphasis)—and in his conclusions, "The documents in question, however, prove nothing of the sort and could be used to prove the reverse" (ibid.). The evidence for what I did argue—rapidly growing disenchantment among Ruhr industrialists with Chancellor Brüning of the National Opposition, increasing despair over the non-realization of a bourgeois concentration, and rising readiness to assume a role (also financial) in linking the National Opposition, which included the Nazis, to a functioning popular Right capable of governing—is overwhelming and widely accepted.

Finally, with full acknowledgment that factual errors and inaccurate renderings are always deplorable, how would the passages be affected by correction? Let us conclude this 12-day Christmas by comparing the passages as they originally appeared with how they will look completely corrected:
Reusch’s Berlin representative, Martin Blank, reported in December 1930 that “the time for a so-called bourgeois concentration is past. . . . We must maintain and build the unity of the Hitler-Hugenberg opposition and pick a leader from it. We must be much more cautious in supporting Brüning and his half-measures.” For his part, however, Reusch preferred an attempt to “crystallize the bourgeois right and the NSDAP into one.” He and several colleagues were particularly impressed with Goebbels’s statement that “wage cuts for the sake of reparations and the current system are unacceptable, but in a national system they would be acceptable.” Reusch was further encouraged by Strauss’s remark that “industry has exerted itself too much for exports; the economy must be switched over from the world to the domestic market. That would mean the reinvigoration of both agriculture and industry. That means a feasible form of autarky.” It would be too much to infer from this a switch to a strategy based on the Nazis; Reusch considered the report so controversial that he asked Blank to destroy all his copies. Nevertheless, it does help to demonstrate that the split between the liberal (pro-Brüning) and national (pro-Hitler-Hugenberg) factions of industry began almost as soon as the outlines of Brüning’s program became clear.

Those who were initially not of the Weimar system would save those whose dominance the system had threatened. As noted earlier, Reusch had begun to despair of a bourgeois concentration as early as Christmas 1930 and suggested instead off-the-record funding directed toward “crystallizing the bourgeois right and the NSDAP into one.” The primary function of unifying the Bürgertum rightward would be to “bring the NSDAP away from their economic utopias and make it capable of governing.” At that time he was in a minority; a year later that was no longer so.

The Christmas 1930 exchange between Blank and Reusch in HA GH/H/Nachlass Reusch 400 101 293/11. On later revealing the Nazis, Gilb to Reusch, ibid., 46. Jones, “Sammlung oder Zersplitterung,” makes it clear that increasing impotence and malevolence marked the efforts of the kept politicians trying to form a Bürgertum-like concentration.

The second sentence of this footnote is omitted in Feldman’s rendering.

Those who were initially not of the Weimar system would save those whose dominance the system had threatened. As noted earlier, Reusch had begun to despair of a bourgeois concentration as early as Christmas 1930 and suggested instead off-the-record funding directed toward “creating one national right,” also out of “the Stalllhelm, Hugenberg, Hitler, and cooperative members of the bourgeois Right.” A primary function of unifying the Bürgertum rightward would be to help the NSDAP move away from its economic utopias. That could happen, as with the Fascis “very similar demands, which were then thrown overboard when Mussolini came to power and had to assume responsibility.” The NSDAP might also be made capable of governing. At that time these were minority views: a year later that was no longer so.

Reusch’s readiness to fund efforts aimed at “creating one national right” is expressed in his letters to Jung of 29 Dec. 1930 and 2 Jan. 1931. In the former he writes, without naming groups, that the “movement must proceed along the line Berlin-Munich,” Berlin being the seat of the bourgeois parties, Munich the seat of the NSDAP—HA GH/H/Nachlass Reusch 400 101 293/11. Erich von Gilb, Reusch’s confidant and the man who shared his Berlin office with Martin Blank, repeated the call for “creating one broad national Right” and named who could be included in a letter to Reusch, 31 Jan. 1931. In the same letter, he urged Reusch and other industrialists to use “all resources at their disposal” to bring this about. Earlier, 30 Oct. 1930, he wrote Reusch of seeking the NSDAP “from its economic utopias” and recalled Mussolini’s transformation.—Ibid., 46. . . .
IN CLOSING

In a far-ranging discussion such as this, it may be useful, for the last time, to sort out what has and has not been said. As I have said repeatedly, I deeply regret the errors in my book and apologize to my readers for them. At the same time, however, I emphatically reject any charges or implications that these errors represent egregiousness, "tendentious misconstruals," or "outright inventions." There is no difference between Feldman and myself on the importance of the norms of accuracy. But, as I have demonstrated here, neither are there any boxes of tendentiousness or fabrication nesting inside my errors. After three years of sustained discussion of my book, I remain committed to this type of structural analysis of Weimar's decline and the results it yields. The confrontation with my critics, which has led to repeated return to the evidence, has not brought me to alter my interpretation or my fundamental arguments. Indeed, many even stand more strongly than before. In this respect, when all is said and done, the whole of the errors amounts to less than the sum of the parts. Feldman's repeated contention that, "If these quotations and attributions were correctly presented, they would completely reverse much of the argument," is itself completely false. Further, I continue in my conviction that my theoretical starting point can do much to order historical understanding. As I continue to work on a new project—the evolution of German social democracy in the years after 1945—my research procedures are improved, but my theoretical and conceptual commitments are in no way diminished.

One closing word. Scholars, it is said, cooperate by disagreeing. I am sure that Professor Feldman and I will continue to disagree on many issues. I hope that, in the future, disagreement will be carried on in ways that advance rather than poison our discourse.

APPENDIX: "If Abraham had..."
(pp. 164-65)

- "If Abraham had admitted in the AHR that the citations questioned by Turner", Turner did not question citations; he questioned the existence of documents containing certain texts and which he identified differently from the way I had. (As the Nocken typescript circulated by Feldman put it, "Nachdem Henry A. Turner die Existenz dieses Dokuments [the December report] in einem Brief an die AHR angezweifelt hat" [p. 4]).

- "misattributed ... and misdated so as to present ... a non-existent exchange": A peculiar phrase "so as to"; it suggests intentionality but evades declaring it. Any implication of intentional deception is completely false. I called it as I saw it, and abusive language does not make it otherwise.

- "would have exonerated himself of the charge of inventing the documents themselves": The documents were and are there, and there is no need for such exonerations. Turner himself has recognized this.1

- "why it was so difficult to find them": It was not and is not at all difficult to locate in the file cited a longish report filed after December 19 and Reusch letters dated December 29 and January 2. In the presence of the archivist it took me a few seconds once we did not focus on Martin Blank as addressee. And the "Dez 1930" report sticks out very clearly because it is the only longish document anywhere in that part of the file. None of these documents was or is hard to find.

- "why the archivist had been unable to find" the documents at Turner's request: For some reason Turner asked the archivist to search only for a personal (and presumably signed) "letter to Blank," when what I referred to was a "report." This was a point the archivist made to me as he expressed his own puzzlement over Turner's procedures.

1. Albeit in somewhat peculiar language: In an unsolicited letter to the outgoing German historian at Catholic University, February 29, 1984 (cced to me and in author file), Turner wrote that "Abraham, upon discovering on his return to the archive that the Blank-Reusch exchange about which I inquired ['inquired?'—DA] did not in fact exist chose to solve that problem by simply substituting two clearly unrelated documents he found in the volume he had cited in the footnote in question" (emphasis added). Apparently first there was a citation, and then only later documents! This is a doctrine of substitution as curious as Feldman's doctrine of invention: from the outset, we have had the same two real, existing documents and texts.

129. For example, circular letter of February 26, 1984; author file.
I must add here that the "sowing of distrust among archivists" Feldman threatens (p. 177)—something which, if true, should alarm historians—was and is in no way in evidence in my dealings with any archivists, including the archivist in question. Quite the contrary!

"Instead of following that course, Abraham": It is generous of Feldman to suggest an escape route, but I was in no need of one.

"misidentifies Edgar Jung ... as an 'adjunct employee' of Reusch, a mischaracterization that awakens a false impression of a formal tie between the two men": There was a formal tie between the two men. Jung did political work for Reusch; he was summoned to him; he submitted work reports to him ("Tätigkeitsberichte"); he was paid by him for his efforts (see discussion above).

"difficult to see why he had to spin a tale about 'scholarly disagreement' connected with the authorship of this report": It is difficult to see why simply because I did not. I contacted and spoke about the matter in the summer of 1983 with a German scholar, but our remarks were inconclusive, and Neebe's work did not come up at all.

... that he may have misdated it, originally claiming that it had been sent on December 29, but now arguing in the AHR that it might have been sent a week earlier": Closer scrutiny and an attempt to follow the "cross references" led me to conclude that it was sent about a week earlier. As noted above, Feldman has now also shifted the date of the document discussed in the letter of January 2 from his earlier bald and certain dating of December 31 to a here more tentative "on or about December 23," that is, a week earlier.

"Reusch sent the document to Reichsbank President Hans Luther on December 9": Neebe does, in fact, maintain this, and I am prepared to believe it. Feldman, however, originally made a far different claim on this matter. In his February 26, 1984, cover letter to the Nocken typescript, he announced of the anonymous report that it "has a covering letter dated December 9" (emphasis added). This was a coup de grace, proof that I "knowingly lied." One assumes that cover letters identify the author of that which they accompany or cover. But when I asked Feldman about and obtained a copy of this "cover letter," it turned out that:

a) this "cover letter" is lodged in an altogether different file series, and does not accompany or "cover" anything;

b) this letter is from Reusch, whom all agree was the recipient of the report(s) in question, to Hans Luther, whom no one claims to have been either the author or the initial recipient;

c) this letter describes the author of a report on "talks with leaders of the national opposition" in the following terms only: "an absolutely reliable man." The entire text reads as follows: "Zu Ihren persönlichen und vertraulichen Kenntnisannahmen übersehe ich Ihnen in der Anlage den Bericht eines absolut zuverlässigen Mannes über Unterredungen, die er mit Führern der nationalen Opposition gehabt hat. Ich wäre dankbar, wenn Sie mir das Schriftstück nach Kenntnisannahme wieder zugehen lassen würden. Mit herzlichen Grüssen/Ihr [signed] Reusch"—HA GHH 400 101 296/29b.

"Abraham reviewed the Neebe book in the December 1982 AHR" That is certainly true, but when I wrote my review of Neebe in January 1982, none of this flap had yet occurred, and thus the "problem" of this report had not occurred to me either. I read the book and appreciated but did not focus on its rich documentation, and, finally, I skimmed this section whose title "August Heinrichsauer und der Strasser-Flügel der NSDAP" seemed far from my central interests.

"[Abraham] cites one of Neebe's footnotes in his October 1983 AHR letter which is only nine pages away from the footnotes dealing with the Heinrichsauer report": I came to this particular footnote via the index, and, indeed, had I read backwards 181 footnotes, I would have come to the relevant ones. A bit strained, I think.

"I find it impossible to believe that Abraham was unaware of Neebe's..."
A Response to David Abraham's "Reply"

GERALD D. FELDMAN

REGRET that David Abraham's lengthy, bizarre, obfuscatory, and often mendacious reply to my charges requires me to impose on the generosity of CEH once again, but readers will understand why I insisted that Abraham not be allowed to have the kind of "last word" granted him in the AHR. In my article, I deliberately refrained from a discussion of the various circular letters sent around by Abraham, myself, and others for the simple reason that most readers would have no direct means of checking the evidence for themselves, that the whole affair had been utterly confusing, and that it was most appropriate in this journal to concentrate on what had appeared in print. This is not a tabloid, but a scholarly journal, and I find the airing of Abraham's personal grievances against me inappropriate in this forum. The way in which I treated Abraham can have no bearing whatever on the reliability of his book or other writings. Although I initially contemplated writing a detailed reply to Abraham's plaints, I have concluded that this would be an abuse of CEH's hospitality. Since this is a historical journal, however, falsifications of the historical record and certain charges should not go totally without comment, and I will make a number of brief responses to certain of Abraham's charges for that purpose.

1. There was nothing "abusive" about my correspondence with Abraham in the spring of 1983, and I did not circulate my letter to the Princeton University Press of May 29, 1983. In my letters to Abraham of April and May 1983, I made clear to him that I took the evidence I had received from Henry Turner about the shortcomings of the book to be extremely serious since I had read the manuscript of his book with trust in its scholarly integrity. I criticized projected corrections of documentary renditions for the planned second printing, which happily never took place, and I made absolutely clear to Abraham that he could not count on my support if the evidence against his scholarship was
true. This was what I told the Princeton University Press on May 20 as well. I did not prejudge the anticipated AHR exchange. I can read, and the documents placed in my possession did not correspond to what appeared on Abraham's printed pages.

2. David Abraham simply is not telling the whole truth when he implies that it was the AHR exchange which triggered my open letter of November 28, 1983. The latter, the first of my circular letters, which began with the words, "It is with the greatest regret that I involve myself directly in the Abraham-Turner dispute" (a regret I could not have expressed if I had already begun circulating my views back in May), was a direct response to a twenty-two-page letter from Abraham to Turner of November 18, 1983. Abraham sent this letter to me and numerous other scholars with the object of refuting charges Turner had raised against his scholarship in a letter of June 27. I found Abraham's response to Turner unsatisfactory and unconvincing both in his AHR response and in his massive circular of November 18, and I said so. When I received Ulrich Nocken's manuscript and realized the full gravity of Abraham's lapses, I circulated that document with a covering letter of my own on February 26, 1984. Since the AHR had a policy prohibiting another exchange and since other journals I contacted were unwilling to allow discussions of a book already reviewed, I had a choice between permitting Abraham's circular letter and mendacious AHR response to stand unanswered for as long as it has taken to get these lines published or to try to make as many of my colleagues as possible aware of the truth. I chose the latter course and believe I acted correctly in doing so.

3. The truth of charges such as those that have been levelled against Abraham's work in Nocken's article are not a function of their appearing in print. I had no intention of misleading anyone, in circulating Nocken's piece, and by March 1984 there was an absolute guarantee that the Nocken article would appear in print in at least one major German journal. It has been accepted by the _VSHG_ and will appear in somewhat shortened form.

4. David Abraham seems to forget that the manuscript of his book was published with my imprimatur in the mistaken belief that its scholarship was sound and that his book constitutes the chief of his scholarly credentials when applying for positions. I did my duty in informing the institutions in question that my views had drastically changed, whether this was at their request or by my initiative. In the three cases where I was aware that he was a job candidate, I made it clear that everything I said and sent should be made available to Abraham. I fulfilled my professional and ethical responsibility and will continue to do so.

5. It is absolutely correct that I said that I would move to rescind the Ph.D. of any student of mine who did a dissertation that turned out to have the number and kind of errors contained in Abraham's work. Research practices such as those confessed to by Abraham are, indeed, indefensible. It is indefensible to present a dissertation based on such techniques to a dissertation committee, and it is indefensible to present them to manuscript readers for a distinguished press.

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Abraham's astonishing—and self-admiring—discussion of "How did Abraham come to his book and what is it about?" is irrelevant to the charges I have made against him. He has not spent the past eighteen months trying to defend his allegedly bold and daring arguments but rather in trying to explain away the extraordinary amount of misinformation his book contains. One of the most distressing aspects of his self-defence has been the manner in which he has piled new misinformation upon old. I must protest against the tendentious misrepresentation of my views through the misconstruction of what I have said in unpublished and published work. Let me provide three illustrations from the conclusion of Abraham's Ode to Himself:

1. David Abraham seems to forget that I recommended his book for publication, and I could not have done so if I had had the boundless hostility to structural and marxist arguments he attributes to me. The passage

2. I did not know that Abraham was a candidate for a job at Tel-Aviv when we visited Israel in early January 1984—not February or March—and I only learned this in April. Had I known he was a candidate, I would have been much more active in informing the department there but would also have asked them to inform Abraham about my actions. His presentation of the number of phone calls I made to Catholic University and Santa Cruz is exaggerated and thus tendentious. On December 20, 1982, the Council of the American Historical Association refused to consider the complaints made about my disclosures.

quoted from my reader’s report had the purpose of pushing the author to expunge some of the more esoteric passages contained in the first version and clarify his argument. Abraham did, in fact, follow my advice, and perhaps it helped to make the book more intelligible to allegedly enthusiastic reviewers in five countries. To take these remarks out of context, however, and present them as an illustration of views which I do not hold and have never in my life held, namely, that “all marxism” is the “same” is mendacious.4

2. The list of names on page 172 of my article, as any sentient reader certainly must recognize, is meant to make the point that all historical actors, whether they are heroes or villains, must be quoted correctly. There was absolutely no reason to infer from that list any view about any of those individuals or about their connection to Abraham’s work.5

3. My latest book has been out for less than half a year, but Abraham has already managed to quote it improperly and tendentiously. In the paragraph that concludes on page 197, Abraham manages to combine a few words in the preface with a passage from its concluding pages to reach a conclusion that has nothing to do with anything I have said. The book is a collection of some of my essays. In my preface, I state that “The basic goal is . . . to illuminate the socioeconomic determinants of Germany’s development . . . in the twentieth century. That this goal is in no way to be confused with economic determinism or dogmatic theory building should be clear from the content.”6 The passage from which Abraham takes his next two quotations pertains to a critical comment I made about a specific argument of Michael Schneider, whose work I respect very much, that the root of industry’s posture toward state intervention in Weimar lay in something “immanent in the capitalist system” (Schneider’s words) and that there is continuity between industrialist attitudes in Weimar and Bonn. It is in the context of this argument that I state that “such an approach distracts our attention from the specific problems which arose between 1930 and 1932 through the juncture of two independent but at the same time interdependent phenomena, namely, the temporal coincidence of the crisis of the political system with the crisis of the economic system. Both developments had as their background the experiences of industry in the period from 1914 to 1929. The entire problem of state intervention should be understood in this context.”7 Nowhere and in no place do I ever or have I ever suggested that the approaches I criticize, explicitly or implicitly, “may even lead to fraud.”8 Whatever methodology or whatever theory one holds, the empirical evidence should be clean in its substance and honest in its presentation. David Abraham has not fulfilled this requirement.

Can a person who works the way Abraham does be expected to write a fair and competent review of the work of others? Let us consider a recent example. Already under a cloud because of the criticisms of his work, Abraham wrote a review of Christoph Graf’s book on the political police in the Weimar Republic for the AHR in which he gratuitously remarked that “Graf’s presentation makes clear just how mistaken H. Turner’s argument is that Papen’s government represented an attempt ‘to stave off the Nazis.’”9 This piece of tendentious misquotation did not come from any published work of Turner’s to which a reader might turn to see if Turner really had argued anything so dubious. It came from Turner’s letter to Abraham of June 27, 1983, widely but not universally circulated, in which there appears the sentence: “In actuality, however, the industrialists at that meeting, their best attended high-level conference of the entire Weimar period, unanimously agreed to raise two million marks to help the Papen cabinet stave off the Nazis.”10 The industrialists, not Turner, thought Papen offered an opportunity to stave off the Nazis. Another innocent mistake?

What is David Abraham’s real conception of the writing of history? A great deal can be learned from his own words, not those contained in his Ode to Himself, but those stuck way back in his appendix where he tries to answer my charge that Abraham should have known that Heinrichsbauser was the author of the December 1930 memorandum because he had read and reviewed Neebe’s book: “I read the book and appreciated but did not focus on its rich documentation, and, finally, I skimmed this section whose title ‘August Heinrichsbauser und der Strasser-Flügel der NSDAP’ seemed far from my central interests.”11 Here

4. Ibid., p. 192.
5. Ibid.
7. Ibid., pp. 221-22.
is how David Abraham works as an historian. Since when does one skim over a book one has agreed to review where the matters discussed are not of central interest to the reviewer? Since when do historians disregard paying close attention to the evidence presented in support of an author's arguments? How can a person working on the collapse of the Weimar Republic not be interested in industry's dealings with the Strasser wing of the NSDAP?

This illustrates the way Abraham deals with other people's work and the way he deals with historical materials. What I tried to show in my article is that he does not seek really to understand what other historians are trying to do any more than he really cares about the full complexity of the documents through which he rummages to find support for his ideas and to buttress his structures. It is hopeless to try to define when Abraham is quoting documents and presenting facts tendentiously and when he does not because he works tendentiously. One might argue that no person could have made all the mistakes that he has made intentionally and that, since some of the errors accomplish nothing while others actually run against his contentions, he is really an innocent if "sloppy" fellow. Abraham uses this argument here, but it is inadequate because it suffers from that "elementary fallacy of composition" concerning which Abraham has seen fit to instruct me.12 What most of Abraham's mistakes reflect is a considerable indifference to precision and accuracy, to the faithful rendering of what he has before him, and to the historian's obligation to wrestle with his materials and try to empathize with the persons and situations about which he is writing. Abraham sees that which he wishes to see, but when the going gets rough, he goes well beyond this as the above illustrations, which do not come from his book, have shown. I am not a psychologist or a mind reader, and I have no idea whether this is intentional in most instances or not, but there is a pattern of tendentious misconstrual, not to support the argument, but rather to support whatever is the argument at hand. With this in mind, we may turn to the other aspects of Abraham's self-defence.

* * *

I shall take the liberty of returning to my original organization in responding to Abraham's response, but I shall make one change by reversing the position of my discussion of the Christmas 1930 exchange and place it at the end of this section. I shall not answer every point he has made for what I trust are understandable reasons, one of which includes documents which I have never seen and are not referred to in his book, but I think I shall say enough to provide readers with a sufficient basis for judgement.

The discussion might helpfully be prefaced, however, by a general consideration that relates to the first of my numbered examples, the case of Harms and Salin and the alleged misidentification of the former. In my first circular letter of November 28, 1983, I attacked what I described as Abraham's "method" in dealing with his errors: "the errors are dismissed as insignificant and then presented as a factual foundation for Abraham's conclusions."13 What do I mean by this? Let us take as an example my error about Bernhard Harms, who was in fact the Chairman of the Friedrich List Gesellschaft, as Abraham claimed. As Abraham notes, my source did not list this office, and the error was innocent. The matter could be left there, but I could also seek to justify my error, as Abraham so often does, by explaining why I made it. I knew Harms was the head of the Kiel Institute and an economic liberal, and I knew that Salin was closely involved with the List Gesellschaft and that he was a protectionist. In this context, I could argue, my error really reflected my deeper understanding of the differences between Harms and Salin. It was not only a natural one for me to make, but there was actually a touch of true knowledge and insight tucked away in it. This is especially the case because, as I shall show, the discovery of the error only helps to strengthen my argument against Abraham's misquotation.

The only trouble with this kind of explanation, which can appear very plausible, is that it would be so much bunk. Whatever forms of "sensitivity" to the issues I may have had, Abraham's point revealed to me that I did not know as much about the Friedrich List Gesellschaft as I thought I did. I remembered that Salin and that organization were discussed in a book by Herman Lebovics, and in that book I found that Harms was the Chairman of the List Gesellschaft, and that Salin was very frustrated by the inability of his social conservative economic group to dominate the society. While I might have been very happy to have confirmed that one of the big differences between Harms and Salin was that Harms was "an advocate of free trade,"14 my mistake reflected, if I

12. Ibid., p. 199.
wished to be honest with myself and everyone else, not knowledge but ignorance about the actual politics of the List Society arising from my having forgotten what I had read. I have decided to forgive myself for this lapse, but it serves as a reminder, for me at least, of how every generalization, analytical argument, and structural construct should be linked as closely as possible to the empirical evidence being used to support it and how this can only be done through immersion in the evidence and careful attention to details.

1. The Harms-Salin Quotation (Abraham’s reply, p. 218): It is illegitimate to put the words of two people into one quotation even if they are Siamese twins. The linking of the two in this case is particularly serious because it seeks to demonstrate unanimity about a point on which the two men disagreed. Harms did not reflect the views of the “agricultural community.” Abraham’s point is indeed not “weakened.” It is demolished by the correction.

2. Government Inaction and Recalling Parliament (Abraham’s reply, pp. 216–17): I agree that it is inexcusable to allow “the implication of a document to become part of the quotation,” and it is this which makes the quotation an invention. It is no less inexcusable, however, to declare that the interpretive sentence makes sense because other quotations and sources that are not to be found in the book allegedly support it. In fact, they do not support it. The leading Ruhr industrialists decided to put more pressure on Brüning but to refrain from overthrowing him. It was the DVP deputies allied to western industry who, much to the latter’s irritation, almost carried things too far.15 The interpretive sentence is inaccurate and the reader has not received an accurate account even now.

3. Kastl (Abraham’s reply, pp. 200–4): Once again, invented words in a quotation are passed off as insignificant because other documents suggest that they could serve as a basis for a valid interpretation anyway. In this instance, the delinquency involved is compounded by Abraham’s statement that how Reusch felt about Kastl might be important for my work but not for his, as if readers are not entitled to accurate information.

4. “Western Industry and the Chancellor’s Retaliation” (Abraham’s reply pp. 214–16): Readers really should look at this part of Abraham’s reply again. Having totally “jumbled” the quotation, Abraham then tells us that “In a sense, it was my attempt to set this document in context—precisely my sensitivity to all that this document said—that led me to the compression.” Is the source of the misquotation “sensitivity” or is it the “inadequate note-taking” discussed a few lines down? I would not accept this from an undergraduate, and neither would I accept Abraham’s statement in his new discussion of the document that Blank thought that Brüning’s recent vote of confidence “would ultimately not prove enough . . . to turn aside Ruhr-based pressure on DVP members to cooperate with others in toppling him.” Blank points out that even some of Brüning’s DVP opponents had voted for him and, in the resigned tone that characterizes the entire document, indicates that he has no doubt that Brüning will stay in power.17 The document says not a word about differences between Kastl and heavy industry, and it is obfuscating to claim that nothing is gained with the correction of a misquotation containing a tendentious misconstrual that turns the original words into language that supports Abraham’s overblown presentation of the conflict between Kastl and certain heavy industrialists. That con-
flict reached its high point in the last months of 1930 and then dwindled into insignificance until 1933. It is the task of the historian to get these things straight, if not the first time, then at least the second.

5. Stehauer, "von Kinkel-Dörflitz" (Abraham's reply, pp. 213-14): Once again, we have quotations from two people rolled into one, but to Abraham it does not matter because Kinkel-Dörflitz was "a dummy voice" for Schlenker anyway, albeit also an important enough person to have been considered for membership on a small "economic dictatorship" council. Needless to say, for Abraham, the correct version of the last sentence in the "quotation" does not really matter much either from the standpoint of substance and argument. I disagree. I think it makes a major difference because the correct version of the sentence has the implicit qualification that continuously destroyed the chances for industry-agriculture collaboration before and after this time, namely, that the help for agriculture had to be compatible with industrial interests and vice versa. Just for the record, the Deutsche Führerbriefe did not come from Schlenker's office.

6. "Paul Silverberg" (Abraham's reply, pp. 204-5): My "point of departure" is in no way that Abraham "mistook a summary in my [Abraham's GDF] notes for a quotation." How on earth should I or anyone else know where this "quotation" comes from? All I know is that I did not find it in the printed source I consulted. The point I am making has nothing to do with Abraham's numerous treatments of Silverberg, but with the fact that the quotation is a tendentious misconstrual of the actual text because Abraham is giving the impression that

industry is dictating to the government in an arrogant and self-confident manner when the document can be interpreted quite differently.

7. Departmentarism of Economic Affairs (Abraham's reply, p. 205): The illustration was meant to show how Abraham works with documents. The misquotation oversimplifies the document and the situation.

8. I am not "likely" correct. I am correct about an extraordinary misconstrual in which Reusch is presented as supporting a coalition that will include the Center Party and the Nazis.

9. The Stahlhelm (Abraham's reply, pp. 211-13): The insinuation that I did not read the document in question personally is deplorable. It is notecard No. 789 of the over 800 pages of notes I took when working at the Gutehoffnungshütte in 1966/67. I have also written on the Ruhr Lockout. If Abraham had worked that way, we wouldn't be engaging in these exercises.

Any words that vaguely or more or less resemble the words in a document will not do. There are labor historians and others interested in the problems of yellow unionism who will read Abraham's quotation and description and pick up misinformation. The quotation, which Abraham admits to be inexcusably imprecise, cannot be excused in this manner, and the new mistakes he is making must be corrected also. On December 17, 1928, Blank sent Reusch a letter with two appended documents, the first a letter from Dr. Brauweiler, the head of the Political Section of the Stahlhelm in Berlin, to Heinrichs, the second an expose by the leader of the Stahlhelm in the industrial area, Harlan-Hagen. Reusch was not recalling anything, Blank was pointing out that he and Heinrichs thought that they should work directly with the Berlin and local offices of the Stahlhelm rather than funnel support through the South German branch as heretofore.

Blank, in other words, did not suggest cutting out the Berlin headquarters, as Abraham claims, because the two offices in question had done a good job of supplying their locked out members with food. In so doing, and more generally, as Brauweiler pointed out in his letter, the Stahlhelm had undermined union efforts to win over more workers during the lockout. This "comradely help" was given to the Stahlhelms, not the employers. The argument was that the employers benefited from it. Brauweiler rejected the idea that the Stahlhelm could do more than this since "the worker comrades must recognize that it is

their own task to create an organization through their own efforts which is strong enough to counter the politics and terror of the unions. That is not a matter for the Stahlhelm..."22 The terms “union busters” and “thugs” are tendentious and inaccurate. The Stahlhelmers were not Pinkerton men.

10. *Reusch and Schacht* (Abraham’s reply, pp. 205–7): Once again we have Abraham’s classic response. Yes, there are errors, but they are not really very serious and, when you think about them, the argument is really understated. In fact, the critics do not really understand the issues as well as Abraham. Not quite. The context of Abraham’s tendentious paragraphs from the Schacht-Reusch correspondence is the preceding eleven-line text which discusses how “the leading figures in the now decisive fraction of industry concluded that Nazi participation in or control of the government would provide the best way out of the political crisis while providing auspicious possibilities for a profitable economic recovery.”23 The Reusch-Schacht exchange was supposed to be “characteristic” of this. When I wrote on May 20, 1983, in a letter which, once again, I did not circulate, that a correct reading of the documents would not affect the argument, I did not know that the Springorum letter quoted on the following page referred to the need to include the NSDAP in the Prussian, not the Reich Government.24 Indeed, I did not know most of what I now know about the book.

Finally, I recognize that Abraham, from his Olympian perspective, has trouble understanding the kind of analysis contained in my new book, but just in case some readers don’t understand what he says, the context of what he cites is: “The leading industrialists sang their old lament about economic freedom and the important role of individual personalities in the economy all the louder, the more they were dependent upon the state and the less they could solve the dilemma in which they had landed through their political incapacity and lack of any concept. If Schacht actually had said that there existed ‘hardly anything with less intelligence and character than the German industrialists in politics’ and spoke of the ‘clumsiness’ of heavy industry, then he knew whereof he spoke, even if he hardly was the right man to make judge-

24. This is discussed in Nocken’s forthcoming article and in Turner’s forthcoming book.

ments about the character of others.”25 Some people might prefer this to empty boxes of the kind Abraham confuses with the presentation of historical reality.

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**The Kapp Putsch** (Abraham’s reply, pp. 209–11)

Abraham’s treatment of my article26 in this reply contains three absolutely incredible tendentious misconstruals, which are all the more astonishing since the article was published in this journal and is easily accessible to its readers.

1. The sentence quoted by Abraham from note 14 on page 108 of my article omits the words I now italicize: “It is possible that Stinnes was enthused by the Kapp Putsch at the beginning, but as what follows will show, he had become quite cautious by the time he reached Essen on March 14.” The absence of an ellipsis in the context of the argument being made makes the misquotation fraudulent.

2. On page 123 of my article, I did not write “explicitly” that Vögler and Kirdorf “resisted pressure and allowed the putsch to go forward for four days before cutting off coal supplies from the Kappists, a measure that would have aborted the putsch.” This can only be a deliberate invention, but it is beyond my comprehension how Abraham thinks he can get away with it. What I said was that Vögler and Stinnes were told by the labor leaders that it would have taken almost two weeks for a coal blockade to be effective and would thus have had the same effect as a general strike in the end. The context was a discussion of which method would have been best for ending the putsch.

3. The fact that the trade unions were pressuring the industrialists to compensate the workers for days lost during the general strike by telling them that this was a good way to dispel the impression that they had supported Kapp hardly constitutes evidence that the industrialists had supported Kapp.

The advice I gave Abraham in my reader’s report to be careful about saying that Stinnes supported Kapp was well-taken. Apparently I forgot whether or not I had said anything about Kirdorf in my article, but it was Abraham’s responsibility not to say that I had when I had not. Wieland was a member of the DDP and much more liberal than most of

26. All references are to the 1971 article in this journal cited in note 27 of my "A Collapse in Weimar Scholarship."
his fellow industrialists. He was not representing any group. The chemical industrialists were sharply criticized for supporting the general strike by their colleagues outside Berlin. No other peak association from any industry, stagnant or dynamic, heavy or light, is mentioned as supporting the general strike. If there was any basic division over the question of payment for the strike, it was between the representatives of industry in Berlin and their colleagues in the provinces. It is extremely irritating to have my work tendentiously misconstrued for the second time.

The 1923 RDI Elections and Related Matters (Abraham’s reply, pp. 207–8)

David Abraham cannot pass off my criticism quite so quickly as he does here as “old hat.” One of the chief points of my first circular letter of November 28, 1983 was that Abraham had falsified and misconstrued the arguments of Bernd Weisbrod, and I offered five hundred dollars to anyone who could demonstrate that Weisbrod said certain things Abraham claimed he had said. No one, including Weisbrod or Abraham, has sought to claim that reward. I can well understand Abraham’s desire to get away from this issue since it touches directly upon one of the most dubious of the central arguments of the book, namely, that the “export-dynamic fraction” gained ascendancy over the heavy industrial group in the 1925–29 period and that this was a period of significant cooperation between the “dynamic fraction” and labor. This argument is supported by none of the serious literature dealing with this period and is nowhere properly demonstrated in Abraham’s book.27 The tendentious presentation of the RDI elections and total disregard of the arguments of those who have researched these problems are particularly harmful to serious scholarship at a time when a major debate is going on over the economic and political consequences of wage levels and social expenditures in the Weimar Republic.

Supervisory Boards (Abraham’s reply, pp. 198–99)

I neither underestimate nor overestimate the role of supervisory board members of German enterprises, and after twenty years of working on German business history, I do not need Abraham’s instruction concerning these matters. Abraham supplies not a shred of evidence to demonstrate the actual influence of the persons he names on Deutsche Bank policies, and he has not answered my point asking why Otto Wolff, a heavy industrialist member of the Deutsche Bank Supervisory Board, supported the policies of the “dynamic export fraction.”

Statistics (Abraham’s reply, pp. 199–200, 218–19)

I do not wish to be pedantic, but Abraham has thirty-four statistical tables in his book, which means that he has approximately 10.5 pages containing such tables for every 100 pages. This is a fair amount, and users of the book might reasonably expect that they can rely on the book for such information. Furthermore, when they use the book, they are entitled to expect that the statistics replicate the figures in the sources cited. When this is not the case, they are entitled to know how the figures are calculated. I only provided a sample of Abraham’s errors. The column “real hour rates” in Table 3 on page 30 also does not conform to the figures given in the source cited.25 The averaging of lignite and lignite briquettes on page 144, Table 15, is more than just a matter of “imperfect averaging.”29 There is no perfect way of averaging a set with a subset because such an average is utter nonsense by definition. This is not the example of Abraham’s employment of peculiar statistical methods without enlightening the reader as to what he is doing. In Table 17 on page 144, he produces “composite” wages for the textile and clothing industry without weighting them. Such simple averages can be very misleading. Perhaps Abraham thinks that there is a Methodenstreit lurking here too.

It will not do simply to pass off the confusion of the indices for textiles and clothing with those of leather working and clothing as an error.30 In the context of Abraham’s argument, it is a significant error because


30. Ibid., p. 199.
Abraham Frowein, the most important representative of the textile industry in the RDL, is constantly portrayed as an ally of the "dynamic export fraction." If so, Frowein was not behaving properly because the real index for textile and clothing makes that branch a candidate for the "tramp" (e.g., coal) rather than the "borderline" category in which he places it on the basis of the figures for leather and clothing on page 114, Table 15.2

<table>
<thead>
<tr>
<th>Year</th>
<th>Coal</th>
<th>Textile and Clothing</th>
<th>Leather and Clothing</th>
</tr>
</thead>
<tbody>
<tr>
<td>1925</td>
<td>70</td>
<td>81.3</td>
<td>96</td>
</tr>
<tr>
<td>1926</td>
<td>76</td>
<td>70.3</td>
<td>80</td>
</tr>
<tr>
<td>1927</td>
<td>81</td>
<td>160.5</td>
<td>117</td>
</tr>
<tr>
<td>1928</td>
<td>79</td>
<td>87.8</td>
<td>98</td>
</tr>
<tr>
<td>1929</td>
<td>86</td>
<td>80.1</td>
<td>89</td>
</tr>
<tr>
<td>1930</td>
<td>73</td>
<td>69.7</td>
<td>83</td>
</tr>
<tr>
<td>1931</td>
<td>62</td>
<td>63.2</td>
<td>77</td>
</tr>
<tr>
<td>1932</td>
<td>55</td>
<td>64.9</td>
<td>82</td>
</tr>
</tbody>
</table>

Perhaps Frowein, like Otto Wolff, should have been in another industry.

Christmas 1930 (Abraham's reply, pp. 210-37)

I am not in the least bit gratified to learn that David Abraham, after an entire year, has come to the grand conclusion that the December memorandum was written by Heinrich Bauer and has figured out who wrote what and when to whom. He seems to suffer from a certain confusion about what our respective roles are supposed to be. It is the height of presumption to say that "What Feldhahn has had to do to make sense of his understanding of the exchange is precisely what I had to do to make sense of it. . ." He seems to have forgotten that it was he who wrote the book and that it is customary for historians, not their readers, to make sense of their materials before they publish, it is an outrage that Turner, Nocken, Maier, I, and others have had to spend weeks away from our own work wading through the jungle of error Abraham had created. To then provide a tedious description of how he managed to create this mess, faced with criticism of his critics is mind-boggling. The significance of my being aware that he had told Turner that he left out the "night" of the Reusch letter escapes me. The others who purchased the book or read it were not privy to this noble confession. I persist in my belief that "not" means "not." I find it very peculiar that Abraham claims in the New York Times that he cannot reconstruct how he made his errors a decade ago and then provides elaborate reconstructions to suit his purposes here (Abraham's reply, pp. 224-26).

But why make things worse? Why do what Abraham has done in the section "Bringing the NSDAP from Its Utopias" and handle the Gilsa letter in a way that can only serve further to discredit Abraham's honesty as well as his scholarship? I don't know, but the matter must be taken up. I have four points to make on this matter:

1. Abraham is fully aware why I "deleted" the reference to the undated Gilsa document from my quotation. The text makes reference only to Reusch, and only a mind reader would know that the last quotation on that page, which improperly follows note 113, came from another document. My task was to figure out where Abraham found the words "bring the NSDAP away from its economic utopias and make it capable of governing" (italics mine). Given his text, the only logical answer seemed to be the sentence in Reusch's January 2 letter to Jung dealing with the need to make Nazi economic ideas more rational. Even when Abraham's translations in the book are accurate, they are often faulty, as is in fact his original translation of the corresponding sentence in Gilsa's letter. In any case, I gave him credit for all the words up to the crucial ones, "make it capable of governing." These words are not in the Gilsa passage either, and they are an invention added to the poorly translated passage from Gilsa's letter to Reusch of October 30, 1930. What Abraham has proven in short, is that he has misquoted two documents on page 316 of his book instead of only one.

2. Gilsa is not Reusch.

26. The explanation was sent to Abraham by me on November 1, 1939, with a correction of the text to account for the document in question. Since Abraham was so far along with his piece that it would have showed things down to have my change put in and for him to make the corresponding changes, I withdrew my change in order to leave it for the response. Abraham certainly could have changed the opening sentence, however, which suggests I may have had ulterior motives.
3. The invented words cannot be excused by the Mussolini example employed by Gilsa because of what certainly in this case can only be called the deliberately tendentious manner in which the Gilsa document is quoted in Abraham's self-exculpation. Let's rerun the passage, this time omitting Abraham's italics and putting in italics for the words left out: "This turn is not to be ruled out, despite the radical agitation of the National Socialists. The Fascists in Italy in 1922/23 also raised rather similar demands, which were then thrown overboard when Mussolini came to power and had then to assume responsibility. Nevertheless one would well to reckon that this self-seeking of the National Socialists will not come all too quickly. But that would again mean that we will still have to fight too long against a Socialist from left and right, which in turn would bring about the collapse." Gilsa then goes on in this rich and fascinating document to discuss the fact that many believed that the Nazis could be cured by their entering the NSDAP themselves and thereby overwhelming the radicals. While granting that they might have a point, Gilsa remained convinced that middle-class irritation with the Nazis after the election was too great for sufficient numbers to be willing to join the party. He went on: "One must therefore choose another way and not place everything on the National Socialist card. The education of the National Socialists to sensible economic thinking will proceed all the more quickly, the stronger a truly right-oriented and active Bürgertum is on the scene next to them. Nothing would be more dangerous, therefore, than that industry [Die Wirtschaft], out of understandable irritation at the previous watering pot of many of the middle parties, would turn away from the latter..." If Gilsa had any tactical goal in the rest of this complex discussion, it was to find a way of drawing middle-class defectors back from the Nazis to a strong rightist Bürgertum Sammlung.

In the light of the full reading of the Gilsa document, I find no excuse for the invented words in Abraham's book and even less for the character of his explanation here.

4. What Gilsa said in January 1931 is not what he said or necessarily thought in October 1929. This was a great crisis, and it is in the nature of a great crisis that people are constantly responding to immediate circumstances. When one writes about specific people, they cannot be boxed into Abraham's "blocs."

Little more need be said about Abraham's self-defence regarding the by-now infamous Christmas exchange. The excuse in diction applied to underlinings is anything but productive. No one questioned that Jung was a publicist who received financial support from Reusch. Neebe tells us what we need to know about Heinrichsauer. Why repeat it here? Perhaps because Jung did not appear once in Abraham's book, while the major figure of Heinrichsauer was relegated to an insignificant footnote on page 321 and Abraham was uninterested when he read about Heinrichsauer in Neebe until he was prodled to embark on this great voyage of discovery.

What might get lost in Abraham's description of what he found in each little inlet is that there are no grounds to argue, as he does argue in his renditions on pages 165-66 and 316 of his book, that Paul Reusch was willing to support the inclusion of the Nazis in a government or a coalition of parties at the end of 1930. Similarly, there is no basis whatever to interpret Goebbels's remarks beyond what the text allows, which is that Goebbels would be willing to support industry if it turned against the government in collaboration with the workers. The latter would not have supported wage cuts, and it is impossible to write about a document like this without paying attention to the various currents and tactics of the Nazis. The new versions of pages 165-66 and 316 are nothing but patchwork for an argument that has fallen apart. The first change talks too much of what may have been the case rather than what people actually were doing. The second change speaks of Reusch beginning to despair of bourgeois concentration at a time when there is no evidence for this and misconstrues tendentiously what Gilsa said. While there is no evidence that Abraham is a better historian than he was when he did his original research, what I find most deplorable is that he is so oblivious to the incredible discrepancy between the two versions presented by him as to suggest that they are not of great moment. Has it not occurred to Abraham that, aside from the improperly cited and undated letter from Gilsa, there is nothing, absolutely nothing, about the identification of the authors of the documents in question and the reporting of what they said that is correct in the first version? How
can anyone who has created such a mess be trusted to do responsible scholarship, especially in the light of the new errors, misconstruals, and fabrications reported here?

Finally, and most painful of all, was Abraham telling the truth in the AHR about the Christmas exchange? Obfuscation will work no longer. There are three points that should suffice to tear apart Abraham's elaborate smokescreen. Let us take up from Abraham's renewed defence against the charge that he invented the documents of the Christmas exchange in his lengthy note 91.

1. On June 7, 1982, Henry Turner wrote to the archivist of the Gutschornungshütte specifically inquiring as to the existence of a "letter" (Brief) and a "report" (Bericht) written by Blank to Reusch.43 The archivist could not find them, not because Turner spoke of a "letter" when he should have spoken of a report, as Abraham claims, but because Herzog could find neither a letter from Blank to Reusch nor a report from Blank to Reusch. On page 165 of Abraham's book reference is made to a letter from Blank to Reusch of December 29, 1910, and comments by Reusch on Goebbels and Strasser and instructions to Blank of January 2, 1931. This citation is a complete fabrication because no such documents exist. The same holds true for the Christmas 1930 exchange between Blank and Reusch cited in note 113 on page 316. In both his letter to Abraham of June 27, 1983, and in the October 1983 AHR exchange, Turner calls upon Abraham to prove the existence of the particular documents cited.

2. When Abraham refers to the Turner letter of June 27 in note 91, however, he does his case great damage because he was fully aware of what Turner meant, since Turner specifically argued that Heinrichs-\-bauer was the author of the December report and that Jung was the recipient of Reusch's "instructions" of January 2. Turner's point was not lost on Abraham because the latter took up the question in November 18, 1983, circular letter,44 where he addressed himself to the charge that he had fabricated the Christmas exchange and made mention of the fact that another historian had identified Heinrichs Bauer as the author of the report. There, Abraham continued to contest Heinrichs Bauer's authorship without ever saying why. The important point, however, is that Abraham was, at the very least, quite conscious of the fact that Heinrichs Bauer might have been the author of the report. Why did he fail to mention this possibility in the AHR?

3. I am not the only person who has been puzzled by this disregard of the evidence supporting Heinrichs Bauer's authorship of the document. In his appendix on this matter, Abraham claims that he did not "spin a tale" about a scholarly disagreement concerning the authorship of the Heinrichs Bauer report in the AHR and goes on to say, "I contacted and spoke about the matter in the summer of 1983 with a German scholar, but our remarks were inconclusive, and Neebe's work did not come up at all."45 As, on August 17, 1984, a German scholar, who I have every reason to believe is the same person to whom Abraham is referring, wrote to me: "I find it much more difficult to find an explanation as to why Abraham fell back [in the AHR—GDF] upon the Jung-version in the question of the Christmas exchange after I—in independence of Neebe—pointed to the authorship of Heinrichs Bauer."46

It is really not so difficult to explain. If Heinrichs Bauer wrote the report, then the entire Christmas exchange was fictitious and the discussions of the documents on pages 165—66 and 316 of the book became utter nonsense. It was very much to Abraham's interest to concentrate on the sheer existence of the documents which he had misidentified and misquoted and suppress Heinrichs Bauer's authorship in an AHR exchange which he mistakenly presumed would mark the end of the public debate about this issue. It took Nocken and myself to force him at last to admit that Heinrichs Bauer wrote the December 1930 report.47

43. Turner to Herzog, June 7, 1982. A copy of this letter has been provided to me by Turner. Since Turner was writing his book at the time, Abraham's sources were terribly important, especially since Turner could not find anything like them in his own notes and materials. How are scholars supposed to work effectively when they come up against work of the sort done by Abraham?

44. This letter, formally addressed to Turner and the cause of my open intervention against Abraham, is on file with this journal.

45. Ibid., Appendix, p. 242.


47. He has not, however, given up spinning tales. He now provides us with interpretations of the underlinings of the Heinrichs Bauer document and tedious and irrelevant details trying to suggest that Heinrichs Bauer's authorship strengthens his overall case because of the latter's association with Reusch. Since Heinrichs Bauer was the editor of an industrialist newspaper, he obviously had connections with Reusch, which does not make him Reusch's agent. There is no basis for making strained connections between the stream of independently inspired reports coming into Reusch's office. What is true is that important changes in industrialist attitudes were triggered by the banking crisis of the spring of 1931, but the second half of 1931 is not the second half of 1930.
CONCLUSION

There are worse things than consciously suppressing the entire truth when one is under enormous pressure, and that is doing something immortalized by a recent ex-president, namely, suggesting that one’s faults and lapses are universal. Note how he qualifies his confession of “errors”: “Whether my book contains more lapses of this sort than comparable works that have not been subjected to the ardent line-by-line critical attention that mine has is a moot point. Not everyone goes to a book in order to evaluate its particular arguments and overall thesis; historians (and others) often use a book for particular items of information: quotations, citations, etc. And here there are simply too many errors in my work.” This is an insult to our profession. Anyone who reads the articles of Nocken and myself will recognize that Abraham’s book contains dozens upon dozens of errors spread over a large portion of the book, and this does not include the errors found in most of the quotations in one of his published articles and what I have exposed about his reply to me here. There is an acid test of the accuracy of quotations, paraphrases and other renderings of material in a book like Abraham’s, and that is translation into German. While it would be quite possible to translate Abraham’s book directly into a host of languages, this cannot be done in the case of German without returning to the archives and secondary sources and literally redoing the research. Apparently this is what Abraham is doing, but does he really mean to suggest that all of us who have had or are having our books and articles translated into German face the same problem?

I am sorry to say that I do not find anything I can respect in David Abraham’s reply. It is a tortured and strained effort at self-exculpation that loses all my sympathy because it persistently fails truly to respect the scholarly standards of this profession and the significance of maintaining the trust upon which it must be based. David Abraham was not a callow youth when he undertook his year and a half of doctoral dissertation research in Germany. He was thirty years old when he returned with his notes, which had been taken in a “hasty and niggardly” manner. He was thirty-one when he received a Ph.D. with Honors for writing a dissertation based on those notes, and thirty-three when his manuscript arrived in my mail from the Princeton University Press for consideration. I can find no excuse for being, in effect, defrauded into accepting a manuscript based on such research, and I can never believe for one second that Abraham was not aware that his notes were an impossible basis on which to write a historical work and that there was something problematic about the quotations he constructed from those notes and from secondary sources which could have easily been checked again. No one who can obfuscate with the kind of consistency shown here and elsewhere could have been unaware of what he was doing.

Just as he sought to deflect attention from the fabrication of the Christmas exchange by concentrating on the charge that he had invented documents, so now he seeks to deflect attention from his tendentious way of doing history by arguing that there was no “pattern” of misconstrual or invention, as if the reality would not exist if it could not be put into some kind of rigid and mechanistic structure. People do not function that way, and neither does Abraham. He is indifferent to what does not interest him or serve his purpose and tendentious when he finds or feels it necessary. For him, his numerous violations of scholarly standards do not add up to anything serious at all in the end. He has, indeed, added a host of new ones here and elsewhere. What does it really matter if summaries of documents are presented as quotations, the words of two persons are presented in one quotation, and words, phrases, and exchanges are invented? Indeed, many of these delinquencies are claimed to be closer to the truth than the truth itself. If we accept this, we have ceased to be a profession.

48. Abraham’s Reply, p. 184. As the New York Times reported: “Dr. Abraham said recently, in one of several long interviews, that ‘there are errors in every work’ and that no one knows if there are more in his book than in other histories. ‘No work that I know of in recent years has been subjected to that kind of scrutiny,’ he said. New York Times, Dec. 21, 1984, p. 19.
49. David Abraham, “Corporatist Compromise and the Re-Emergence of the Labor/Capital Conflict in Weimar Germany,” Political Power and Social Theory 2 (1981), pp. 59-199. Ulrich Nocken has been asked by the editor of that journal to write a comment on the errors in this article which will appear shortly. It should be noted that no one has yet checked Abraham’s use of the materials he found in the East German archives. I have no intention of taking the trouble, but what he is likely to have done with those documents should be obvious from his record.
On Professor Feldman’s Insistence:
Some Closing Remarks
DAVID ABRAHAM

In opening this exchange, Gerald Feldman accused me of “egregious
errors,” systematically “tendentious misconstruals,” “outrageous in-
ventions,” and “fabrications.” In my reply, I made what I believe
to have been a good faith effort to address and evaluate these charges.
Because an understanding of the arguments in my book was essential to
that evaluation, I first restated those arguments, and my overall thesis.
In the face of a fantastic portrayal of business-Nazi conspiracy, it was
necessary to set the record straight. Making repeated reference to what
I actually said in my book, as opposed to what Feldman intimates that
I said, the substance of my reply was this: First, I openly acknowledged
my commission of error. I did not trivialize or minimize my mistakes.
I apologized for them, and said they were “inexcusable.” Second, and
not inconsistently, I argued that Feldman was nonetheless wrong to
claim that the errors undermined the fundamental arguments of my
book. Third, I showed that there was no consistent pattern to the errors
—the correction of some hurt the argument; the correction of many
others strengthened it—and that there was, therefore, no systematic
tendentiousness evident in my mistakes. Fourth and finally, I refuted the
many charges of mendacity, invention, fabrication, and fraud, and pro-
vided a variety of sorts of evidence to show that these charges—surely
the most serious that Feldman made—were utterly baseless.

What is Feldman’s response here?

First, he retreats from many of his initial charges, including many of
the charges of fabrication, without being moveh enough to withdraw
them explicitly.

Second, he does not engage the substantive question of whether,
overall, the correction of the errors in my book renders its argument
infirm. Indeed, after insisting that correction of the errors would “com-
pletely reverse much of the argument” in my book, Feldman now labels
my pursuit of this question mere “obfuscation.”

Third, unable to sustain the original charge of a systematically ten-
dentious misconstrual of evidence, he resorts to almost purely ad homine
attacks on my intellectual makeup and “way” of doing history. He
claims now that I suffer from a sort of chronic and apparently incurable
tendentiousness. Its presenting symptoms can appear, without warning,
at any time in any place in any work I do.

Fourth, having abandoned many of his previous charges without any
mention that he ever made them, let alone any admission that they were
false, Feldman renews other charges that were false to begin with, and
introduces new mistakes and false accusations.

Fifth and finally, having announced a year ago his decision not to
treat me as a colleague,1 Feldman makes some of the consequences of
that decision evident here. He uses words like “fabrication,” “inven-
tion,” “mendacity,” so loosely as to destroy the boundaries of common
meaning and discussion. A veteran of over “twenty years of working on
German business history,” he ignores or forgives his own errors while
attacking me for acknowledging mine. Feldman insists on the impor-
tance of appreciating the “rich,” “fascinating,” and “full” complexity
of historical sources. When I discuss such materials, however, some-
times reaching conclusions different from his own, he evinces only con-
tempt—and such words as “tedious,” “obfuscating,” “bizarre,” “con-
templible,” “self-admiring,” “annoying,” etc.—for the effort. At least
for Feldman, this is a discussion no longer constrained by the demands
of scholarly criticism or even civility.

What does all this boil down to? A senior historian makes the most
gruesome of charges against a junior colleague and has been shown wrong.
He nonetheless refuses to acknowledge the falsity and gross inappro-
priateness of those charges. At best, what was claimed as evidentiary proof
of fraud ends up as inference about my intellectual character: I am too
clever to have made mistakes other than intentionally (“Response,”
p. 267). At worst, accusations are simply soft-peddled or dropped,
withdrawn without mention, sometimes only to be replaced by further
false accusations. This is a confused and miserable state of affairs, but it
cannot be ignored.

1. “I no longer regard him as one [a colleague], and I do not intend to treat him as
In replying to his "Response," I will treat Feldman's claims in the order of their appearance.

First Series: Correspondence

1. Feldman says now that his letters to me in Spring 1983 were not "abusive," but actually "friendly." I suppose that is a matter of judgment. I would be glad to provide copies to anyone interested. He says too that he did not circulate his letter of May 20, 1983, addressed to the Princeton University Press, withdrawing his reader's endorsement of the book. Yet I never said Feldman himself circulah the letter beyond his listed cc's. What I wrote was that the letter was in fact circulated, meaning that it was distributed to and read by colleagues other than those to whom it was immediately addressed or cc'd. It was. Feldman also writes now that a second printing of my book "happily" never took place. He is wrong; there was a second printing before the need for corrections came to my attention. The next edition will mark a third printing. He says he "did not prejudge the anticipated AHR exchange," but that of course was not the point. He did prejudge me, that is, he did find me guilty of the crimes Turner accused me of before seeing my reply to Turner's charges. And he did make his prejudgment widely known in his travels during the summer of 1983, as well as through the fact of his prior withdrawal of his reader's endorsement in his letter to my publisher.

2. Feldman writes that I am "not telling the whole truth" when I imply that his circular letter of November 28, 1983, was "triggered" by the appearance of the AHR exchange. In my "Reply" I simply stated what is true, namely that "With the publication of the AHR exchange ... Feldman's engagement in my case took on a qualitatively new dimension" (p. 178). His circular letters were that new dimension; they started right after the AHR exchange appeared. But there is very little implication here about "triggers." Feldman himself wrote that he began his campaign of circular letters because he did not like the response of our colleagues to the exchange initiated by Turner. He did not like the profession's response to the published portion in the AHR, and he did not like the profession's response to what Turner had made a very public annex to it.

3. It was then that Feldman enlarged his efforts to make them what he calls "public" and "directly."

4. I doubt that Feldman can find anyone who believed that his February 1984 descriptions of the unfinished, unsubmitted Nocken typescript as "forthcoming" in the VSWG meant something other than just that. How does a powerful senior figure deal with this deception? By vaguely and indirectly informing us now that by sometime the next month he was persuaded that it "would appear in print in at least one major German journal" (my emphasis). Perhaps in several, but not the VSWG and not then. At the very least, he owes its editors as well as the historians at Catholic, Santa Cruz, and Tel Aviv universities an apology.

4. Characteristically, Feldman now claims that my description of his telephone calls to Catholic and Santa Cruz universities as a "series" was "exaggerated and thus tendentious" ("Response," n.2). It was neither: Catholic University's search committee has in print reported "four or five" calls, some of which "threatened"; Santa Cruz about as many, construed by nearly the entire department there as "an effort to intimidate colleagues." This was certainly quite a "series" of calls. Feldman's objection reveals not my exaggeration or tendentiousness but a certain recklessness in making charges and readiness to hector those who might want to make up their own minds. Feldman came to visit in Tel Aviv in January, not February. My error.

Second Series: "Any Sentient Reader" (Feldman)

1. Feldman claims that his remarks about Hari Krishna chanters and the liturgical quality of my analysis were good-natured attempts to prod

3. Feldman justified his Rundschreiben of November 28, 1983 thusly: "I am particularly moved to take a public stand by comments made to me that Abraham has satisfactorily responded to Turner in his AHR letter and especially in his 22-page circular letter [sic] of November 18." Feldman's qualitifiers, "public," and "directly" ("involve myself directly") serve to trivialize his prior activities. My lengthy letter to Turner (November 18, 1983), was sent to those individuals listed as cc's in Turner's earlier letter to me. I closed that letter by asking Turner to supply me with the names of the unlisted recipients of his own. He did not answer, but I was subsequently contacted by unlised recipients of his letter and supplied them and some few others with my reply to a letter that Turner had effectively made a public annex to his letter to the AHR.

4. For Catholic University, see The Nation, February 16, 1985, p. 181; for Santa Cruz, Daily Californian, p. 10. Feldman's letters were subdistributed for him in Israel. At two of the three universities at which he admits intervening (Catholic University, Santa Cruz), his interventions were uninvited. What then does it mean for Feldman to say now that he "made it clear that everything [he] said and sent should be made available to" me? Why? Is it their fault? Are those who receive unsolicited criticisms of a third party called upon to transmit them to that third party?
me into clarifying my arguments, and that he does not have the “bound-
less” hostility to structural Marxism attributed to him by me. Well and
good. Let us say his hostility is merely immense. He also claims credit
for making possible the “allegedly enthusiastic” reviews garnered by the
book he now denounces. This is indeed dialectical.

2. The “sentient” reader might wonder if Feldman is being disingen-
uous. Why of “all historical actors, whether they are heroes or villains,”
did Feldman light upon Marcuse and Stalin? His choice of references
may have been random. But if Feldman wants to take his revenge on the
“class of ’68,” he (or Turner, Necken, Torquemada, Norman Pod-
horetz, or “any other historical actor, thinker, or even historian”) has
available any number of suitable journals.

3a. Feldman’s more extensive quotation from his new collection of
essays does nothing to alter what I said he wrote about his position on
these matters or Michael Schneider’s view of immanent tendencies in
capitalism. It is false to claim that I quoted him “improperly.” The reader
can check: the footnotes are in my text; Feldman’s book is available.

3b. Feldman claims now that I quoted Turner “gratuitously” and
“tendentiously” in a review I wrote and that I cannot, in fact, write
“fair and competent” reviews. This last claim does not merit a response.
Still, Feldman asks if my characterization of Turner’s view of the Papen
bureau is “Another innocent mistake?” No, it was neither innocent nor
a mistake. Turner wrote that the industrialists “agreed to raise two
million marks to help the Papen cabinet stay off the Nazis.” Turner’s

5. To continue with the Preface to his new collection where he leaves off immediately after
asserting that his goal is “in no way to be confused with economic determinism or dogmatic
theory building,” he asserts that “Much more it reflects the pragmatic and eclectic traditions
of American historiography.” Now, that American tradition usually espouses tolerance and the
benefit of the doubt, but Feldman is sometimes short on those qualities. Further, “It is just as
important to counter certain contemporary historiographical tendencies—such as general
attempts to turn the clock back—what might be even worse, to consider history from a purely
7, 8, respectively. These remarks mirror certain “enemies to the left—enemies to the right”
themes abounding some liberal German historians and enunciated at the 1984 German
Historikertag.

6. If an author writes, “the Committee on the Present Danger agreed to raise two
million dollars to help the contras stave off totalitarianism in Nicaragua,” it is more than
reasonable to conclude that the author is not only reporting the Committee’s intent but
himself believes that the contras are acting to stave off totalitarianism in Nicaragua. Okay?

7. My review of Christoph Graf, Politische Polizei zwischen Demokratie und Diktatur
unity behind the Papen cabinet and its purposes appears in German Big Business and the
works as an historian” (“Response,” p. 249f.) is but a diversion, a search for a dark, aberrant character trait. Feldman’s claim also argues against what he says my book is about.8 I take my responsibilities as a reviewer seriously.

Third Series: Former Charges

There is something less than exemplary about Feldman’s way of dealing with his own errors and with charges that prove false: First, ignore what is discomfiting; “I shall not answer every point he has made for what I trust are understandable reasons…” Second, forgive one’s own mistakes: “I have decided to forgive myself for this lapse…” This threatens to turn debate into a monologue. Since Feldman has now revised nearly all of his charges, I shall simply treat them in his order.

1. Harms and Salin: There are problems with my treatment here, since Harms and Salin did not hold fully identical views, but Feldman has not clarified matters. Initially he charged that “most of this quotation [is] an invention.” That charge has proven false. Now he misquotes me. Neither in the passage in my book (p. 91) nor in my “Reply” (p. 218) do I refer to the views of the “agricultural community” (Feldman); I refer instead to those of “the agricultural specialists in the scientific academic community,” to which, one can safely assume, very few farmers belonged.

2. Recalling Parliament: Feldman argues now that my “interpretive sentence is inaccurate”: the original charge was that my “interpretive sentence makes no sense.” Both charges are inaccurate. In a loud footnote, he ignores the documents I quoted—and which he undoubtedly checked—and refers to the same pages of Neebe I quoted. However, he does not quote from them. If he did, one would find, in addition to the striking evidence I presented above (“Reply,” p. 217), the following statement on the subsequent role of Ruhr (“western”) industry in threats to recall parliament: “Western [Ruhr] industry was not without a role insofar as the DVP deputies supported by it, basing themselves on the Langenauverein [Ruhr heavy industry association—DA] meeting of 5 June 1931, insisted until the end on calling parliament back into session.”10 Feldman now additionally inaccurately claims that this was “much to the latter’s [leading Ruhr industrialists’] irritation” but the same source, on the same page, writes not that Reusch and Springorum expressed “irritation,” but rather that they refused the opportunity to object when it was offered to them (by one of their “dynamic fraction” colleagues).11

3. Kastl’s Leadership: Here again the original charge—“there is no evidence whatever”—simply disappears without comment. The new charge is that I “passed off [error] as insignificant.” This is not so. Rather, I have examined the consequences of correction and let the chips fall where they may. In discussion of the opposition to Kastl’s liberal leadership, I cited Reusch, Blank, Schlenker, and Reichert—all respected men at the very center of the heavy industry fraction. Feldman comes back by contrasting Reusch with Thyssen.12 Nowhere in this discussion did I mention that extremist and atypical figure. Of course it is useful for Feldman to cite Thyssen if the goal is to create the totally false impression that only oddball extremists took positions which were, as I have shown, in fact taken by mainline heavy industrialists.

I am sorry that so accomplished and prolific a figure as Feldman feels he is being treated as if he “dig[s] potatoes” while I “mine gold.” This is a needless worry and an absurd distinction.

4. Western Industry, Kastl, and Petulance: Here what started out as a “totally invented quotation” is now characterized as “misquotation.” Neither is good, but invention is different from error, and Feldman should recognize and acknowledge that difference. He does not. Feldman now labels my reading of the document “tendentious.” I reject Feldman’s characterizations of this document: Kastl’s favorable attitude toward Brüning and his speech stands in clear contrast to what the rapporteur, Blank, and the man to whom he was reporting, Reusch, felt. Blank found significant parts of the speech “highly regrettable,” and was not at all pleased that Brüning would for the time being retain a small majority and “continue in the same way as hitherto” with his policies, including those hostile to western big industry but readily accepted by Kastl.14

5. Schlenker’s Major Step: Once more, and without notice, Feldman

8. That Heinrichs-bauer-Strasser relations discussed in these three pages were not at the center of my interests really only provides further evidence that Feldman’s implicit and Neebe’s explicit portrayal of my thesis—as being about business support of the Nazis—are way off the mark. Feldman cannot have it both ways.
11. Ibid., “Silverb erg forderte im m achinen eindeutige Konsequenzen gegenüber diesen Parlamentariern, konnte damit aber bei Springorum und Reusch nicht durchhängen.”
12. Actually the charge first appeared in Feldman’s “Dear Colleagues” letter of March 20, 1984, where he alleged that the error was “designed to back up an interpretation which would otherwise make no sense.” That charge disappeared already before Feldman went into print.
retreats from his original charges of an "invented sentence" and a "fabricated statement." A decent person would acknowledge that the earlier charges were false and defamatory. Feldman instead seeks refuge in a sarcastic footnote reminding readers that Berlin and Düsseldorf (of which, in discussion of Feldman's charge, I made no mention) are different places. But Schlenker most certainly did have an office in Berlin, at Schönberger Ufer 39, to be precise. Headed by Dr. Max Hahn, it was shared by the Central European Economic Congress (taken over by Schlenker's organization in August 1931) and the Deutsche Führerbriege, publisher of the second major statement in question—which Feldman unfortunately ignores. In his first essay, Feldman thought it "might be a reasonable question" whether the character of Schlenker's step remained major. I answered that question for him, but he has chosen to ignore my response.

6. Paul Silverberg: There is no question, as I acknowledged in my "Reply," that my original rendering of the quotation was badly jumbled. And nowhere did I aim to excise the error by explaining how it was made. That said, it does seem to me important that Feldman adduced the error as an example of my "consistently tendentious presentation [which] serves to exaggerate the evil role played by industry, to overdramatize its power..." (" Collapse," p. 169). It was this charge that I attempted to meet in my reply. I did so by considering this quotation in the context of my treatment of this very key industrialist, and found the charge unsubstantiated. I still do.

7. Deparlimentarization of Economic Affairs: Here Feldman charged that two intimately related issues were not related. Rather than admitting the error of that charge, he simply shifts to claiming the situation is "oversimplified," something I and many others doubt but would certainly be prepared to discuss.

8. Zwischen: As I acknowledged before, the word "zwischen" was indeed meant in the exclusionary rather than inclusionary sense, and Feldman is thus entirely correct about that.

9. The Stahlhelm's Help: I am not in a position to know whether Feldman did or did not read this document before he embarked on his attack or how many notecards or pages of notes he took in 1966-67. What I do know is that he wrote that I "produce[d] pure fiction" because Reusch "could only have [remembered]" the Stahlhelm's "sup-

plying—in opposition to the interests of the employers—they locked out members with food" (my emphasis). And I do know that his original citation was to the wrong file. Now he admits that these Stahlhelm activities unquestionably benefited the employers, but claims instead that Reusch recalled none of it. If Feldman read the document, how could he possibly have made the first claim? (Feldman's reminder, "I have also written on the Ruhr Lockout," only renders this more troubling.) Why does Feldman insist that Reusch was aware of these activities when claiming that they were opposing employers ("Collapse," p. 171) but unaware of them when acknowledging that they were rather less savory? ("Response," p. 253) Obviously too, Reusch recalled all of this. After reviewing these materials, he wrote back to Blank two days later, "I hold the suggestions made to be right." 17

Rather than acknowledging this at least two-fold error, Feldman attacks with an irrelevant Pinkerton metaphor. Obviously the Stahlhelm was, as we know, a mass organization. But since when do union-busting activities, like those described and subscribed to in this document, require badges and contracts?

10. Reusch-Schacht Collaboration: Feldman's characterization of my reply does not correspond either to what I wrote or what he claimed. Nevertheless, his more extended quotation from his new book is useful, for it underscores precisely what I was asking about Feldman's position.

Kapp Putsch: Feldman on Feldman: Here Feldman does not retreat from his original charge of "distorted and abused" scholarship but rather selects new words, claiming my treatment of his essay contains unbelievable "tendentious misconstruals," a fraudulent misquotation, and a "de liberate invention... beyond my comprehension how Abraham thinks he can get away with it." These charges are all unfounded. First, I wrote in my "Reply" that I was struck by Feldman's assertion that Stinnes may have been "enthused by the Kapp Putsch at the very beginning." That he became "quite cautious" by March 14 is an interesting but surely different point, and it is a bit queer for Feldman to assert, on the basis of that point, that my reading was "fraudulent." If someone "enthuses" on Monday but cools on Tuesday afternoon, is it misleading, let alone fraudulent, to say he "enthused" on Monday? I think not.

How does Henry Turner, presumably free of my failings, understand

17. See my "Reply," pp. 214-13 and n. 75.
Feldman's meaning? Much as I did. Basing himself solely on this same Feldman article, he writes: "As long as the attempted overthrow of the republican government seemed to have a chance of success, the leading men of big business nevertheless maintained a posture they characterized as 'neutrality,' which amounted to according the new government [the Kappists] parity with the old." If putchists and a constitutional government are treated with "parity," is it too much to say the putchists are being supported? Surely not.

Second, does Feldman really mean to suggest now that these industrialists waited four days before cutting off coal supplies from the Kappists because they were cogitating over "which method would have been best" for aborting the putch, and that they were in the interim really considering the other alternative—supporting a general strike? That would be some finding indeed, especially since Feldman wrote that the leaders of the chemical industry were "reproached" by the RDI "for their early stand against Kapp." 20

Third, rather than complaining of his irritation, Feldman might more usefully have addressed the issues I raised on pp. 210-11 of my "Reply": They are based on his own new findings that "it was surely no coincidence that exactly those same people" who seemed to form a group in 1920 were still doing so in 1930. Perhaps Feldman found his argument here uncomfortably similar to my own. These pages of Feldman's work are also easily accessible. I would urge interested readers to consult them. 21

The 1920 RDI Elections, etc.: Gone without a trace is the claimed language of "victors and vanquished." Instead, Feldman now writes that my argument about the ascendency of the dynamic fraction of industry and its greater cooperation with labor is "supported by none of the serious literature" and stands in "total disregard of... those who have researched" it. This too is queer: at least if one does not accept Feldman's use of the word "serious" surreptitiously to exclude the work of numerous East Germans (whatever their deficiencies; after all, Feldman and I have both taken their work seriously enough to cite it) as well as the work of Hilferding's generation ("warmed over pseudomarxism"). Curiously, the case he offers consists of two recent articles by himself (with assistance), one eleven-page eleven-year-old article by his then-student Nocken, and an older article by Dirk Stegmann, whose position has since moved closer to my own. In any case, all of these articles, except for the two recent ones by Feldman, are, along with the Weisbrod book, addressed in my work. 22

Supervisory Boards: Three points. First, I can understand that Feldman resents my "instruction" on five points of industrial-financial organization. Equally I am less than taken by the schoolmasterly instruction on broad points of ethics, mentality, professionalism, and the demands of modern life. Second, it has been Feldman's own position that membership on the Supervisory Board of Directors (Aufsichtsrat) of firms and banks brings with it a voice in basic policy. Third, Feldman's reference to Otto Wolff was presented as an aside, not as a substantive point. But if he wishes a very brief substantive reply, I would say: a) No account of a particular social system can be expected to explain the behavior of all individuals within that system, nor must it for it to have genuine explanatory power; b) My sense of Wolff, as I already suggested in my book (p. 166), is that particularly as someone Catholic, left-Rhenish, and Francophile he was supportive of Brüning's government, which was favored by the dynamic-export fraction of industry.

Statistics: Here just one point. Readers looking at Feldman's source

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I made my differences with Weisbrod explicit in my book (e.g., p. 114, n. 61: p. 140, n. 70; p. 227, n. 60) but also clearly relied on him for other matters. This is normal. For example, see my "Reply," n. 64, above. Weisbrod rejects Feldman's claim that I have "falsified and misconstructed" his arguments. See also Dieter Peters, "Germany and the Great Depression," Journal of Contemporary History 4 (1969): 67-72; Walter Hornmann, Bandwisse und Zersplitterung zwischen Landwirtschaft und Industrie (Darmstadt, 1966); Dieter Schüller, Der DGB als politisches Forum der Weimarer Republik (Hamburg, 1960); Fritz Gaugler and Manfred Oelsen, "Der RDI, 1919-1933," in Dieter Frieko, ed., Die bürgerlichen Parteien in Deutschland (Berlin, Leipzig, 1970), pp. 180-190, and much else. Feldman's new claim appears in his "Reply," p. 258 and n. 27.
and mine for Feldman's elaborate "correction" of my "Textile and Clothing" production indices might conclude that Feldman (dare one say it?) fabricated a category. What he here labels "Textile and Clothing" is, in the source, only "Textil." It does not include clothing ("Bekleidung"), which is listed only in the category I mistakenly used. Yet I would prefer to think that the category he provides for the figures he takes from Hoffmann is an error rather than an invention.


1. Feldman may now find it the "height of presumption" and "mind-boggling" for me to point out that numerous charges he circulated in his missives, some of which he included here, are false. Unfortunately, that response evades something basic: Common decency demands that Feldman retract false charges he has made against me. It demands his open withdrawal, for example, of his December 31 dating of the Jung document, intended to show that I lied; his reference to a specific "formulation" actually absent from the report; his insistence on the existence of a "cover letter" to the anonymous report; his claim of no formal Jung-Reusch connection; his claim that there was no off-the-record funding for creating one national Right; his claim that I presented Reusch as an early example of industrialists' support of the Nazis; his outright insistence on a "negative position which Abraham converts into a positive one"; etc. Feldman has used these claims—all false—to try to discredit my scholarship and drive me from the profession. False accusations of such weight must be withdrawn. Does he intend to inform search committees and world-wide sub-distributors of his xeroxes and all those who trusted that Gerald Feldman would not make false charges that these charges too were in reality false?

2. That analysis of documents and sources which Feldman normally finds so "rich," "complex," and central to the labors of the historian suddenly becomes "tedious" and mere "divination" when its fruits fail to please.25

3. Feldman questions the significance of my informing Turner and him on November 18, 1983, that I discovered that I had missed the "nicht." But does it really escape him? He knows also that this and other errors were reported to my editor. They were to be included in an errata sheet, an idea Feldman later dismissed as "novel" and "ridiculous."26 He seemed to prefer indictment. In his February 26, 1984, circular, Feldman, knowing of the correction, wrote "Abraham has him [Reusch] saying the reverse by leaving out the 'not.'" Indeed, the Nocken typescript he first counseled and then distributed boldly and falsely claims that subsequent to revisiting the archive "[daraufhin] Abraham did not himself correct his grotesque distortion." Not content with the false premise, Nocken ventures to call this a "conscious deception or at the very least cover up."27 If Feldman was uninterested in correcting me, he could at least have corrected Nocken, for whose "service to the integrity of our profession" he has such high regard. The significance of my correction was that I made it, it was public, Feldman knew it, my publisher had it, and despite those facts Feldman and Nocken chose, months later, to proceed as they did!

4. Of course "not" means "not." And "reverse" means "reverse." Feldman's year-long charge has been that I fully "reverse" Reusch's meaning, and that charge has proven false.

5. I have my sense of what is key in the lengthy von Gilsa letters, and Feldman apparently has his. I leave it to the reader to judge.28

26. See my "Reply," pp. 227-28. I began preparation of an errata sheet for future sales as well as for broader distribution in Fall 1983. That was put in abeyance when I found myself occupied with a semi-public campaign whose central claims were those of fraud and mendacity. Feldman's characterization of an errata sheet appears in his February 26, 1984, circular, p. 1, line 1.
27. See my "Reply," n. 101; Nocken, p. 5.
28. But had I not quoted extensively from the Gilsa letter in my "Reply," here is what Feldman intended to present as his "rich and fascinating" understanding of the Mussolini reference: "Although Gilsa speculates about the relevance of the Italian experience, the entire letter is extremely pessimistic about the chances of taming the Nazis in the near future." Of course, this was not von Gilsa's mere "speculation" but the third point in a numbered three-point recommendation. Nor was he "extremely pessimistic." Feldman's construal is, at best, one-sided. This revision was sent to the editor and myself about three months after Feldman submitted the final draft of his essay. Feldman need not be a "mind reader." I had already indicated to Turner and Feldman that part of the footnote in
6. Again, the rich and complex process of analyzing sources ceases to interest Feldman when the results are disappointing. Why does his substantive nonresponse to my discussion of Jung, Heinrichsauer, their activities, and their connections to Reusch's assistants, deputys, and associates? Here too false charges have been leveled but are now simply ignored, and the label "patchwork" offered as a change of pace from "obfuscation." 29

7. To allege mendacity, Feldman ("Response," p. 261) proffers a confusion of two very basic and different issues. What I provided in my "Reply" was an examination of the meaning of documents. This has nothing to do with accounting for the genesis of any mistakes years ago in copying from them.

8. Feldman's claim that I "suggest" that there is no significant discrepancy between the original and corrected versions of the documents involved here is fully without foundation. I introduced the corrections by writing that my errors and inaccurate renderings were "deplorable," and they are. Is it not also deplorable for a man of Feldman's stature to claim "new errors, misconstruals, and fabrications" here when there are none?

This leaves the matter of the renewed charge of my lying in the AHR, "How Are Scholars Supposed to Work...?" (Feldman)

Let us look at Feldman's renewed and ever-more-violent claims of fraud ("tear apart Abraham's elaborate smokescreen"). In doing so, I shall ignore the considerable but acknowledged discrepancies that have emerged with each restatement of this claim over the past year. 30

1. Once again, in my book on pp. 345-65, I make reference to "the report" to Reusch and to "all his copies"—the report, of course, being half of what I mistakenly took to be the "exchange" cited there and again on p. 316. It is easily checked and simply not true, contrary to what Feldman insists here, that "reference is made to a letter from Blank to Reusch." This is all examined and reproduced in full detail in my "Reply," and Feldman's wanton use of the term "complete fabrication" only contributes to further degradation of debate. 31 But I am really stumped as to why Feldman again repeats Turner's oft-repeated error.

Of course, Feldman pooh-poohs this distinction, suggesting it is a diversion from my original sin in handling the document(s). That is not the case. I have made and repeat the distinction for two reasons: First, repeated identification of a document in a way the author did not identify it, especially when such contrary identification is coupled with repeated charges of authorial fabrication, seem to me at best misleading and at worst, evidence of bad faith. Second, after going to the archive and finding the document, I had a conversation with the archivist about it. He told me, as I remember it, that his difficulty in finding the document derived from Turner's mistaken identification of it as a personal letter rather than a report. Since Turner added the archivist's inability to find the document as evidence for its nonexistence, this mislabeling seemed to me highly relevant.

While on the subject, let me note as well how curious Turner's behavior was in the Spring of 1982. If it is the case, as Feldman claims here, 32 that Turner, working on his book in the Spring of 1982, could not find the documents I cited in his own notes, why did he not simply ask me about them then? Turner's first request to me came a year later, only after he thought he had proved their "nonexistence." This suggests a prior (or perhaps, a priori) animus toward my work and the seriousness with which I take collegial obligations, and describes a procedure less befitting a colleague than a presumed terrorist. Turner later claimed in the AHR—apparently only for rhetorical impact—that he "began considering a trip to Germany to see their full texts." 33 This too is very strange, especially since he had the Neebe book (see below), which it is now clear provided him with a rendering and attribution probably very close to his own notes. And Turner turned down my offer to procure for him "photocopies "of any of the sources cited in my book." 34

In any case, to me, the offer of a public apology in the AHR linked to my providing proof of the existence of something I never added or implied existed, seemed a stacked deck. And, if one includes that "offer," Turner asked me four times to prove the existence of a document I did not cite. 35 And when did you stop beating your wife, Professor Abraham?

2. In his June 27, 1984, letter, Turner then proceeded to identify the

34. I made the offer in my letter to Turner of June 16, 1982. He turned it down in his letter of June 27, 1983, p. 1; on file with the editor.
35. On May 11, 1983—a year after what Feldman reports here and, apparently only after he had convinced himself of my transgression—Turner wrote asking me for the
documents as Feldman claims he did. He did so on the basis of a "theory" attributed to "one of the other historians who has worked extensively in the Reusch papers, but without divulging his identity since he has not authorized me to do that." This seemed to be a bit bizarre. And it has proven to be even more bizarre than I thought. At the time, I thought that the unidentified historian was Turner himself. Recently, Turner informed a correspondent from the New York Times that the unnamed historian was Dr. Reinhard Neebe. This makes Turner's letter (and his letter to the AHR) stranger still since he could easily have written: "For the correct attribution and likely dating of the December report to Reusch you have misidentified" — Neebe, like me, calls it a report (Bericht) — "see Neebe's book, pp. 117ff., and the corresponding notes on p. 251." That would have been simple, straight, and the duty of a senior colleague. As it was, I did not know what to make of Turner's "theory," embedded as it was in so strange and hostile a communication. It was to this "reference" of Turner's that I "made mention" (as Feldman here calls it) in my letter to him of November 18, 1983.36

How would Feldman have had me cite that in the AHR: "Undated secret report to Prof. Turner from an unnamed historian?"

Nevertheless, I did in the AHR state the possibility that Jung was cited or text of "the two letters you quote on pp. 164 ff." of my book. I replied on May 21, 1983 (as discussed in my "Reply," n. 91). On May 28, 1983, Turner, a careful worker, submitted his letter to the editor of the AHR in which he wrote: "the use as evidence of nonexistent archival documents ranks, of course, among the gravest of scholarly offenses... I therefore call upon Abraham to divulge, in this professional forum, proof that the two documents quoted by him on pp. 164-65 and 116 of his book—a letter from Martin Blank to Paul Reusch of December 29, 1930, and 'instructions' from Reusch to Blank of January 2, 1931—exist now or ever existed." With minor changes, this letter, replete with its implications of purposeful falsification, then appeared in the October 1983 AHR. Again in his letter to me of June 27, 1983, a widely circulated annex to his letter to the AHR, Turner asked me "to clear up the matter of the quotations on pages 164-65 and 116 of your book which you attributed to a letter from Martin Blank to Paul Reusch of December 29, 1930, and to what you described as 'instructions' from Reusch to Blank dated January 2, 1931." And a few lines later, "I would be ready to extend you a public apology in the Review if I received from the chief archivist... a statement to the effect that you have succeeded in locating a letter from Blank to Reusch dated December 29, 1930 and 'instructions' from Reusch to Blank, dated January 2, 1931." Perhaps this was an innocent error, but repeated four times by Turner it could only generate confusion.

By the time of his unsolicited letter to Catholic University of February 29, 1984, Turner had, finally, cautiously abandoned his misdescriptions, writing, cryptically, of "an exchange of communications"; page 1, lines 8-9.


not the author of the anonymous December report. And I did so as follows: "I should note, however, that the authorship of this little-known December report is susceptible of several interpretations, and quite apart from the present exchange between Turner and myself, there is now some scholarly disagreement about it."37 This made explicit the possibility of a different authorship, and any other authorship whatsoever would obviously suffice to render the Christmas 1930 documents a non-exchange. But I certainly did not make this statement on the basis of Turner's letter. Rather, it was on the basis of discussion with the unnamed German historian Feldman and I both cite.

Silently abandoning his previous and widespread coup de grace claim of a "cover letter," Feldman now invokes a new one, about a colleague who set me straight. But there is no mystery here. He knows very well that we are referring to the same young German scholar because I told him of this scholar, his identity, and his view of the possible authorship of the report myself on July 10, 1983.38 It was on the basis of those inconclusive remarks, and my interlocutor's desire not to be cited, that I wrote what I did in the AHR. And, "alas," the letter of over a year later to which Feldman refers here neither takes the position Feldman suggests, nor says what he writes.39

Despite all this, Feldman claims that I sought to suppress from the public the possibility of Heinrichsbaumer's authorship and that "[i]t took Nocken and myself to force him at last to admit" it. This is simply not true. Nor is it true that correcting the identifications involved renders my discussion of the materials "utter nonsense," Feldman is wrong again. In January 1984, I submitted to my publisher a list of corrections for purposes of an errata sheet and any next printing. In addition to the other corrections involved, the corrected footnote, based on the information I had, concluded, "It is possible that the report in question was authored by Heinrichsbaumer and then annotated and forwarded to Reusch by his Berlin chief, Martin Blank." What is true is that in February and March 1984, my attention was drawn to the pages of Neebe I had not properly assimilated. Feldman could have ascertained from my publisher 37. AHR 88 (1983): 1146.

38. Abraham (Stiepel) to Feldman (Munich), July 10, 1983, late afternoon telephone conversation. On the "cover letter," see p. 2475 in the Appendix to my "Reply."

39. This letter of August 17, 1984 was a reply to a letter from Feldman in which the author rejected Feldman's charges of fraud, his moralizing, and his entire modus operandi. Feldman cannot be pleased with its contents, however he mistranslates the word verfallen: "to hit or chance upon," not "to fall back upon."
I provided the information in hand. There is no legitimate basis for the Feldman-Nocken charge of "suppressing the entire truth," nor for the charge here (entered on top of charges of incompetence and stupidity) that I am a clever devil.

The Christmas 1930 charges began with the Big Lie charge of document fabrication, forgery. They have from the outset been central to the attempt to invalidate my scholarly credentials. They are slanderous and the real blot on our profession. It is shameful that Feldman should offer them as the justification for his outside-of-print-campaign.

Feldman's "Conclusion"

This brings us to Feldman's Conclusion, a further repetition of his contention that I don't really care, have no commitment to getting things right, and suggest instead that everyone's work is sloppy. Feldman is wrong on all three counts, and what he, grandstanding, labels "an insult to our profession" is nothing of the sort. He wishes to support this claim by quoting from the second half of the paragraph on p. 185f. of my "Reply." But this cannot fly.

1. Readers looking back to my text will see that he ignores the first five sentences of that paragraph, including the topic sentence. "Yes there are mistakes in my book. There are far too many of them and there are more of them than I thought. . . . I am painfully aware of this, and it is inescusable. The mistakes are of various sorts. They include . . . ." etc.

2. The meaning of the conclusion to that paragraph is clear: Insofar as scholars, as I wrote, "use a book for particular items of information: quotations, citations, etc.," it becomes an archive annex. And insofar as my book is, in this aspect, not always as precise as it should have been, its value is correspondingly diminished, which I deeply regret.

3. I have obviously learned from this experience; archival research cannot be hurried and transcriptions must always be closely verified before translation. 41

40. Footnote 47 of Feldman's "Response" is really unworthy: 1) I did not describe Heinrichsauer as Bresch's agent, but rather showed, beyond doubt, that he worked regularly with those who were; 2) My discussion ("Reply," pp. 235-15) of events in the six months after Christmas 1930 becomes transformed into a comparison of "the second half of 1931" with the "second half of 1930."

41. Feldman ought also to improve his verification. He has my age wrong three times (for the record, I was 22, 30, and 32 respectively), and his new description of Nocken's achievements is sheer mischaracterization.

"errors." There is no qualification to my open admission of errors. But Feldman puts the word errors in scare quotes (""errors""). He means fraud, not error. 42 And that claim is false and foul. I reject it not with qualifications but absolutely and categorically.

The Bottom Line

I have never said or suggested that errors do not matter or that they are "closer to the truth than the truth itself." This is a complete invention on Feldman's part and a malicious one. Remarkably enough, Feldman and I, as I wrote in my "Reply," do agree on something: it is important to be accurate. But Feldman has accused me not of sloppiness but of fraud. He acknowledges that there is no pattern to or overarching purpose (e.g., impugning businessmen) served by the errors in my work: by any reasonable standard, this would refute the charge of fraud. Rather than simply acknowledging that fact, however, Feldman now charges me with two other flaws that are both unforgivable and barely susceptible to being overcome.

The first flaw is tendentiousness. I "work[s] tendentiously." Mine is a "tendentious way of doing history." This is complete nonsense. Feldman has never made clear what he takes to be the purpose of my arguments or "way of doing history." Without such a purpose, the charge itself literally makes no sense. But no matter —this does not faze Feldman: "it is hopeless," he claims, to try to determine when I do things right and when I do things wrong because it is really a matter of intellectual makeup. According to Feldman, I cannot be trusted because I can be dishonest or have the wrong opinions at any moment. This too is nonsense, a case of character assassination disguised as scholarly criticism.

The second, perhaps more fundamental, failing is my lack of empathy. "I do[es] not really seek to understand" either documents or the work of other historians. I am "indifferent to what does not interest [me] or serve [my] purpose." (Again we are left wondering what Feldman thinks my purpose really is.) I am indifferent to "the historian's obligation to wrestle with his materials and try to empathize with the persons and situations about which he is writing." 43

And what do I plead? It is true that my work is more structuralist

42. Feldman repeatedly uses the words "fraud," "indulgent," and "crime." See, most recently, Daily Californian, pp. 9, 10.

43. All from "Response," p. 510.
Some Closing Remarks

David Abraham

than narrative, and more marxist than liberal; my conclusions are harder on the role of most German industrialists at the end of Weimar than are those of some other historians. (Nor do I try to find scapegoats like Thyssen.) This does not make my work less open, honest, or serious. While we all try to empathize with everyone we study, both the awfulness of history and our own intellectual ambitions permit us to come to different conclusions. It is about time Feldman put this out on the table.

Unreally (Nearly) But Loud

Ultimately, all scholars should be able to agree on the raw facts. I have not attempted to mask errors in my own work, but have sought to correct those where I have found and those brought to my attention. Indeed, my lengthy "Reply" represents a sustained attempt to confront the lapses in my own scholarship and to come to terms with documentary evidence. But facts also speak in different ways and of different things and often they do not make sense of themselves. Consequently, within our traditional canon, we construct our evidence differently as well as—perhaps even before—differing in our interpretation of it. Such differences can and ought to be openly acknowledged and debated with mutual respect. That opportunity was not seized here by Feldman, who instead muttered about "empty boxes" and proclaimed instead that he "does not find anything [he] can respect in [my] reply."34

Feldman has curiously dismissed any suggestion that this might involve a Methodolatry. But his own repeated call for "empathy" as the first task of the historian itself recalls one of the eldest of such conflicts, namely that between Vichtl, grounded in "empathy" and Ehrlichman grounded in reason. Many historians believe that interpretive understanding is what the enterprise is about. For them, theoretically based examination of behavior and pattern consistency—the attempt to comprehend particular human associations as social systems—most often undertaken by marxists but not only by them, is unacceptable. Indeed, for Necker it is "long outdated," "East German textbook" dogma; for Feldman it is "fanatic," guided by "preconceived notions"; for Turner it is a method of "bias," "preconception," "impaired judgment," and "reductive assumption" by those other to "secular prophets of considerable renown," used for "corroboration of a body of doctrine" with "political usefulness... to undermine societies with capitalist economies and to legitimize repressive anti-capitalist regimes." In short, method

becomes a tool in a "crude against capitalism." They would try to distort my own approach into a form of dogma and impose their own on the profession.45

My differences with Feldman, Turner, and Necker extend beyond these questions of perspective and interpretation. Their call for "empathy" with Weimar businessmen has also been a call for sympathy with them. And Feldman, Turner, and Necker do express far greater sympathy for these men than I can. Yet nowhere does Feldman even attempt to show where my lesser sympathies obstruct my abilities as a far-minded interpreter of the political economy of Weimar Germany. Indeed, he has silently recognized that, as the Silverberg, Bosch, Lange, Kastl, and numerous other examples show, I am anything but an ungenerous critic of Weimar businessmen. Turner's "sympathy" goes even further, for he believes that most scholarship on late Weimar business and politics is deficient precisely because its practitioners have failed to sympathize with their subjects. And why have they like I failed to sympathize adequately? The answer is simple: in their political sympathies they have stood, despite their diversity, to one degree or another on the left, as do I. In short, my critics, in their division of labor, have offended themselves as unecize of empathy; Feldman's view seems to be that any historian who fails to muster the proper sympathy cannot be expected to write sound history, however many or few his errors.

The methodological and political issues at stake help provide a possible context for understanding my critics' false accusations and irregular actions. But this context does nothing to diminish the measure of Gerald Feldman's disgrace. He has located a number of errors in my book, itself a scholarly service. But he has misconstrued these errors as fraud and trumpeted a range of false charges far and wide. His blasts were framed in order to intimidate, not to inform, bidding other historians to a rash to judgment. Feldman began with a misapprehension: the treatment of errors as darkly motivated crime. Then, without passing to consider either the thesis or the arguments of the work he attacked, he sought to rationalize his own mistake through bluster and truculence. The admission of R. H. Tawney that Feldman cited in his declaration of war

45. Thus, Necker's essay (p. 25) by ruling out and excluding "again warned over, deficient" and "long since out of date and inapplicable," these, and others. This is barely disguised code. Turner's Causation, "Myths, Preconceptions, and the Making of History," pp. 349-50 there, pp. 350, 351, 354, 356, 357, rules out any possibility that structural analysis of Weimar can illuminate any historical truth, they only produce or perpetuate "myths."

Some Closing Remarks

against me was a valuable one. Perhaps Feldman should have quoted and construed it correctly. Or perhaps he should have recognized that a common scholarly effort unites us all. But he did neither. And it is only now, more than a year after the damage is done—and only thanks to the (often-taxed) patience and generosity of the editor of CEH—that Feldman has finally found it necessary to attempt substantiation of his charges of systematic tendentiousness and fraud. And these charges, it turns out, have proven shameful and without foundation.

From the Editor

This brings to an end the exchange in this issue between Professors Feldman and Abraham, which was intended to discuss as fully as necessary the allegations about Professor Abraham’s book made by Professor Feldman in the first of these articles. I think the facts of the dispute have now been amply laid out, and any further exchange would deal more with rhetoric than with substance. Professor Feldman fully agrees with this decision, but has asked me to state on his behalf that he stands by his charges, especially the most serious ones, and by his actions. There are, however, several issues raised by Professor Abraham’s remarks that call for editorial comment. (I will not comment here on such things as tone, motive, or the appropriateness of actions not related to this exchange, in the case of either author.)

1) The first three of these manuscripts received a good deal of editorial attention. As with any article, I conceived my role to be that of helping the author say what he really meant to say, and say it clearly; I also checked some quotations, especially from documents accessible to me but not to all readers. (The editorial work was complicated by the fact that copies of the unedited versions were circulating widely. I believe there may still be traces in Professor Abraham’s closing remarks of an earlier version of Professor Feldman’s response.)

When I received the draft of what had been intended as a final statement of five to seven hundred words from Professor Abraham, my first reaction was to say I would refuse to print it, not only because of its length but because of some of its contents. On further thought I decided, as I wrote Professor Abraham, that “it would not be useful for me to make further editorial suggestions about your answer. The features I found objectionable . . . are more pervasive than that, and to try to ‘edit’ them out would produce a document other than the one you

46. In his “Dear Colleague,” circular of February 26, 1984, p. 2, line 30ff. Feldman wrote: “I am in complete agreement with a statement made by R. H. Tawney that ‘an academic adversary is not an Amalekite, to be smitten hip and thigh.’” The actual quotation from Tawney reads “An erring colleague is not an Amalekite to be smitten hip and thigh.” Feldman has shown little appreciation for the historical difference between error and antagonism, but I would not think to claim that he has “invented” or “fabricated” the quotation. See J. M. Winter, ed., History and Society: Essays by R. H. Tawney (London, 1978), p. 126. This remark originally appeared in Economic History Review, 2d series, 7 (1954): 91–97 as part of a “Postscript” to Tawney’s 1941 article on “The Rise of the Gentry.”
From the Editor

Again having the "last word," Professor Abraham has again used it to raise "methodological and political issues" in order to attack not only Professor Feldman, but Professor Turner, who has not been involved in this exchange. There is obviously considerable ideological interest in this case; there are some who support Professor Abraham because he stands "on the left," others who support Professor Feldman because Professor Abraham stands on the left. If Professor Abraham had wished to discuss those issues and attack Professor Turner in this exchange, he had 107 manuscript pages in which to do so, at a time when a reply could be made; I find it inappropriate at this point.

In any case, it is obvious that there is ample space in other publications for discussion of ideological and other issues. I think enough information has been provided here to allow readers to make up their minds on the substantive issues raised by Professor Feldman at the beginning of this exchange.

DOUGLAS A. UNFUG