

# The Poverty of “Protest Research”

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GIESSELMANN, WERNER. “Die Manie der Revolte”. Protest unter der Französischen Julimonarchie (1830–1848). [Ancien Régime, Aufklärung und Revolution, nr. 25] R. Oldenbourg Verlag, Munich 1993. xv, 1086 pp. Maps. DM 378.00 (in 2 vols).

After nearly a ten-year delay, Werner Giesselmann’s 1984 Heidelberg habilitation thesis has finally been published with the support of the Deutsche Forschungsgemeinschaft. The work has been revised, greatly expanded, and updated to incorporate the most recent research literature. The publication features an unusually broad spectrum of source material and literature and is enhanced by an extensive amount of data presented in tables, graphs and charts. Giesselmann has divided his work into three sections: first, the presentation of “protest case studies”, then the “quantification of protest, based on French criminal statistics”, and finally the iteration of the causal “factors determining protest during the July monarchy”.

The first volume of the two-volume set is dedicated entirely to the “case studies”, for which the chief sources were contemporary press reports during the July monarchy and files of the *Archives nationales* that have long been known and well researched. The author is indeed aware of the drawbacks of these sources. Giesselmann has divided his “protest cases” into five “case categories”, each of which is featured in a chapter: socio-economic protest (under which “price riots”, “worker protest” and “agrarian protest” are subsumed), protest against fiscal policy, oppositional protest to the regime, clerical and anti-clerical protest, and youth protest. One look at such a classification approach is enough to see that it entails serious problems, since even Giesselmann must realize that this is a poor portrayal of reality in the period prior to 1848 and that it obliterates the context in which these events occurred.

The first part of the second volume features Giesselmann’s efforts to quantify protest during the July monarchy using French criminal statistics, which date back to 1827 and which in his opinion have been undervalued as source material in social history research. By using these, Giesselmann was able to increase the number of occurrences of protest (*Gesamtprotestvolumen*) to more than 110,000 cases, a figure far surpassing earlier surveys. Therefore it becomes rather apparent that it is not only necessary to scrutinize critically the sources – something Giesselmann does do, in fact – but especially to analyze critically the “concept of protest” which he uses, a concept which he has deduced from the mass of delicts compiled in the criminal statistics.

The major part of the second volume is focused on the author’s endeavors to explain systematically the findings presented in the first

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volume by weighing his explanations against other existing “theories of protest” (Marx, Tocqueville, T.R. Gurr, C. Tilly, L. Tilly, R. Tilly, E.P. Thompson) and compiling these into a “causal model” in which several “determining factors” are linked together. In this part of the book, which deals chiefly with developing the concept of “relative deprivation” into the most important cause of protest, Giesselmann relies heavily on existing literature and thus returns to a more conventional form of presenting his position.

Unquestionably, Giesselmann’s undertaking is an ambitious one. On the one hand, it quickly becomes evident that this is a very diligent, data-packed study, which attempts to make up for deficits in the German-speaking field of research on the July monarchy. On the other hand, the more than one thousand pages are full of far too many banalities, endless repetitions, and unnecessary concessions to the business of science. At some point in the long presentation the reader begins to ask him- or herself whether or not the author actually has made any real contribution to our knowledge of the July monarchy. To repeat an often raised objection to “protest research”: is it truly necessary to engage in extensive quantification and “bivariate correlation analyses” if the fruit borne from these efforts is merely the author’s compilation of previously known facts, namely that there are “three clear phases of crisis identifiable at the beginning, in the middle, and at the end of the regime” (1830–1832, 1839–1841, 1846–1847) (pp. 582ff., 586), that “the participants in protest were recruited primarily from the lower social classes” (p. 622), and that their chief motive was a “feeling of social discontent” (pp. 955, 962)?

Before I present the content of one such chapter and the results of Giesselmann’s work as a case in point, I would like to raise two fundamental objections. The first of these deals with so-called protest research, the realm in which the author works, while the second is addressed to Giesselmann’s “remarks concerning anti-Jewish protest and women’s protest”.

### THE POVERTY OF “PROTEST RESEARCH”

Ever since Charles, Louise and Richard Tilly published their studies on “social protest” in the 1970s, this topic has created its own comfortable niche in science: new literature continues to appear, and it is still suitable today as topic material for a habilitation thesis. The reason why I am by now so wary of the entire field of “protest research” is because it has been reproducing its systematic deficits for over two decades and because its most recent results demonstrate how immune the field appears to be to fundamental criticism.<sup>1</sup>

<sup>1</sup> In 1985 I attempted to formulate such a critique, if in a polemically exaggerated form and in a rather remote publication: “Massenarmut und Existenzrecht”, *Autonomie*, new series, 14 (Hamburg, 1985), especially pp. 86–88.

Here I intend to concentrate solely on those points that Giesselmann himself addresses. In his "prefacing remarks" the author discloses what motivated him to undertake this study: in his words, he hoped the "protest events from the past" would uncover "the key" to the living conditions, etc., of the "nameless and mute actors" and thereby enable him to "delve into" the socio-economic structures, etc. In addition, "the interest in present-day protest movements of one's own experience" was a guiding force in his research (p. xv). This agenda, characterized by the criminological vocabulary of hunt (*Fahndung*) and "capture" (*Erfassung*), has been prevalent in protest research since the very beginning. At the same time, the field depicts itself as siding with the common people, the nameless actors in history. It was the reverberations of the 1968 student revolts and the interest in Third World liberation movements (which actually developed quickly from "counter-insurgency-research to prophylaxis against revolution" (p. 778)), that acted as the initial catalyst for protest research in the early 1970s.

The field was able to establish itself in a certain sense as the historical arm of "conflict sociology". During its second developmental phase, protest research in the Federal Republic adopted several set pieces from English social historiography (Hobsbawm, Rudé, E.P. Thompson), without assuming the critical substance associated with it. All that happened instead was that the remaining remnants of Marxist conceptualization were relinquished in favor of a cultural-anthropological approach, or in Giesselmann's words, an "ethno-cultural [*volkskulturell*] approach" (pp. 69, 641). The application of electronic data-processing eventually led to the expansion of the field of protest research, particularly in Germany in the 1980s. However, the research concept remained nearly constant: once defined and analyzed within the framework of mass statistics, the event "protest" was to serve as a "tracer", Giesselmann reiterates, a "key to historical reality" that enables the historian to make "macro-level" evaluations of how conflictual the modernization process was and at the same time "to delve into the everyday world of the otherwise mute lower social classes", that is, to obtain "access to the subject matter" (pp. 17, 619):

This "tracer" is to be used to distinguish better several characteristics of the July monarchy in France. Yet it would certainly demonstrate a sort of "callosness" toward the subject if attention were to be devoted to it solely for the general benefit of obtaining findings. Protest research also legitimizes itself by its claim to be "social history from the bottom up". By decoding the "language of the mute," it strives to strip away the obscurity of the everyday life of the lower social classes. (p. 31, cf. p. 1041)<sup>2</sup>

<sup>2</sup> Giesselmann knows that "direct access to the subject matter [. . .] of a good 160,000 rebels" remains unattainable (p. 619), that "the rebellious subject with which we are dealing [. . .] also acts rebelliously in such a way that, for the most part, it evades the direct grasp [!] of the historian" (p. 654). Yet in the course of his research, Giesselmann must have delved so deeply into the world of the "nameless and mute" that he could

In defining his subject matter, Giesselmann naturally differs from the circle of authors associated with Charles Tilly in selecting criteria for registering “protest events”. Whereas Tilly and his associates apply the criteria of the collective use of violence and a minimum of twenty participants, Giesselmann introduces the criterion of “illegality” and defines “protest” as “individual and collective conflict behavior that violates existing law” and abandons the “legal and institutionalized avenues of settling conflict” (pp. 8, 12). Putting aside for the moment the difficulties of such supposedly operational definitions, which still always harbor normative assumptions, let us first focus on this “concept of protest”: here the objection must be raised that the selected criterion of “illegal behavior” is stipulated by the sources, or more specifically, is tailored to the ability to evaluate these sources.

The consequences of this become evident in the second volume. As mentioned above, one of the sources Giesselmann uses for his “empirical protest research” is the French criminal statistics.<sup>3</sup> This actually curtails the novelty of his work. Giesselmann’s study includes the impressive number of more than 110,000 analyzed “protest cases”, which – as he repeatedly emphasizes (pp. 35, 569) – greatly surpasses the “sample of a good 400 cases” that the Tilly group<sup>4</sup> collected from newspaper sources and used as the basis for their book, *Rebellious Century*. However, Giesselmann is only able to accumulate such an outstanding number of cases by including the multitude of everyday violations handled by the courts during this period (92,976 convictions for insulting and assaulting public officials, and for resisting arrest). Collective delicts in connection with subsistence disorders (*pillages de grains, entrave à la circulation des grains*) and worker strikes (*coalitions*) only add up to 0.3 per cent and 0.9 per cent of the convictions, respectively (p. 570), whereas “common criminality” among the lower social classes – be it theft, begging, arson and similar crimes – remains completely unaccounted for, even though Giesselmann initially praises the contribution of historical criminology to protest research.

This means that the extent of the statistical scope results from expanding the “concept of protest” beyond the criteria that were selected out of operational considerations and have been commonly used in research to date, namely collectiveness and violence. As questionable and scanty as these criteria were, they were at least practical enough to cover criminally relevant (meaning of interest to the police) “unrest”,

assume an intimate relationship with the perpetrators of protest: he calls them at different points “our machine stormers” (p. 127), “our workers” (p. 160) or “our striking workers” (pp. 164–165), “our accused” or simply “our rebels” (p. 605, etc.).

<sup>3</sup> The statistics in question are the *Compte général de l'administration de la Justice criminelle* (CJC) for 1827 and subsequent years.

<sup>4</sup> This is the wording introduced by Giesselmann.

"mutinies", and "revolts" occurring during the July monarchy and the *Vormärz*, the period directly preceding the 1848 revolution in Germany. Giesselmann, however, overlays such manifest events with a large layer of crimes labeled as "verbal or physical assaults by individuals or small groups against law enforcement officials" (p. 571). By doing so he hopes to find the "ordinariness of protest", meaning aspects of protest in everyday life. He enlarges the quantity of delicts involved with additional massive numbers of violations and misdemeanors and then uses it as "protest index P1" in most of his graphic diagrams and correlations.

The entire second volume of Giesselmann's work, in so far as he applies methods of quantitative analysis, is based on the otherwise unscrutinized assumption that "protest" during the July monarchy can be studied chiefly on the basis of a sample comprising "five classic violations of public order" (pp. 565, 572, 573ff., 658). The consequence of defining the "concept of protest" solely in terms of immediate friction with the regime is that the "deficits of social control in the Orléans regime" become a central focus for Giesselmann: "social control" is labeled as one of the "key factors of protest" (pp. 890, 892). By expanding the definition to include "daily occurrences" (p. 571), the "concept of protest" finally leads not only to the prestidigitation of everything known as class conflict in Marxist vocabulary, but also to the disappearance of historical content as such. Correspondingly, the material basis of social antagonism during the July monarchy is then reduced to the factor of "deprivation". I will return to this point later.

### ANTI-JEWISH PROTEST AND WOMEN'S PROTEST?

If the reader has not put Giesselmann's book down before reaching the sixth and last chapter of the first volume, then the impertinence of what follows will prompt him or her to do so: after 500 pages devoted lavishly to five "case studies of protest", the sixth chapter, entitled "Remarks concerning anti-Jewish protest and women's protest" is a full four pages long. Although it is very irritating to find these two topics lumped together in such an afterthought of a chapter, it is even more infuriating to read the few statements of any substance. Following a quote by the French minister of internal affairs, in which the anti-Jewish riots of 1832 in Alsace are attributed to the "usury practices" of Jews themselves, the author, who by this point in his work has amassed more than a thousand footnotes, maintains dogmatically and without offering any evidence to back up his claim:

It is beyond a doubt that the Jewish usurers mentioned here did actually exist and the exorbitant interest rates must have produced great bitterness among the indebted agrarian population especially in such extremely hard times. (p. 544)

This is “beyond a doubt”. Instead of furnishing sources on the economic function and situation of the Jewish population in Alsace, Giesselmann offers us the “scapegoat” hypothesis:

For the rural masses, tortured by hunger and also suffering from the cholera epidemic, the Jewish aliens [sic!] become a scapegoat, upon which responsibility for the perceived existential threat is projected and against which one delights [!] in working off pent-up frustrations. (p. 545)

Since I am not sure whether such quotes speak for themselves, let me take the precaution of suggesting that one refer to Hannah Arendt’s comments on this hypothesis of “healthy common sense”.<sup>5</sup>

The second half of this commentary pertains, in Giesselmann’s words, to “the other neglected ‘peripheral group’ – women”:

which will also not be treated specifically in a chapter dedicated solely to them. The reasons for this are basically twofold. First, the protest actions of women, which we have studied, cannot be attributed to their gender-specific situation during this period. On the contrary, their motives were identical to those of the rebelling men, with whom they shared a social existence characterized by misery, discrimination and repression. However, there exists a specific women’s protest in the form of emerging feminism [. . .], but this women’s protest only partially corresponds with the criteria of our definition. (p. 545)

What Giesselmann gives as the reason to exclude the study of women’s protest is precisely what should have been studied. After two decades of extensive research on women, which for good reason has repeatedly analyzed the presence of women in the revolts of the *Ancien Régime* and during the first half of the nineteenth century, Giesselmann does not even feel obliged to pose such a question by the facts evident from his own source material (see below). But unlike most of his fellow historians, who chose simply to ignore the challenges presented by women’s research, Giesselmann presents the argument, which one would have liked to think was obsolete, that “women” – always grouped as a historical entity – should only be given special consideration in historiography when they produce “women-specific” demands.

Then Giesselmann takes yet another step backwards by offering us his own assumptions on the repressed sex, clothed as scientific analysis of the motives for female protest. Edward P. Thompson once proposed that the role of women in the food riots could be explained by face-to-face marketing, which made them highly aware of the swings in bread prices.<sup>6</sup> I do not know whether this meant that public “protest behavior” of women was due to their status as consumers or to certain characteristics specific to their sex. In any case, Giesselmann argues the latter.

<sup>5</sup> Hannah Arendt, *Elemente und Ursprünge totaler Herrschaft* (Munich, 1986), pp. 28ff.

<sup>6</sup> Edward P. Thompson, “The Moral Economy of the English Crowd in the Eighteenth Century”, *Past and Present*, 50 (1971), p. 116.

At three points in his book; namely in the chapter on "price riots", in connection with his study of the "participants of protest", and while considering "specific conditions of socialization" as a "factor of protest", he happens to speak in passing of gender.

Contrary to his basic assumption that rebellion during the July monarchy was nearly always a "man's business", Giesselmann is first forced to admit that the percentage of women in the price riots was "relatively high": 24 per cent of those convicted for plundering and 35.6 per cent of those convicted for blocking the free trade in grain were women, according to his calculations (pp. 65, 602). He explains this as follows:

Significant in Table 18 are the evident differences in the percentage of women convicted for the various crimes. The lowest percentage of female convictions, namely less than 2.5 per cent, is for political crimes. The more highly developed, reflective and politicized forms of protest [?] are nearly exclusively a man's domain, as is politics itself. Correspondingly [?], the percentage of women is by far the greatest among the most elementary forms, those related to material existence such as grain plundering and blocking the free trade of grains – 24 per cent and 35.6 per cent, respectively – figures that are major exceptions to the rule. As consumers responsible for supplying the family with its immediate material provisions, they are confronted first and most drastically with the rise in food prices, and utter hardship may have been the primary motivation [?] for tearing them from their conformist [?] passive [?] behavior and to drive them to revolt. (p. 602, repeated verbatim p. 902; see also p. 65)

The only impression I can get from this is that the author did not become aware, in the course of his historical research, of the eminent political importance of "grain plundering" and the public articulation of the subsistence issue by women during the July monarchy. Instead, he appears to speculate about female characteristics and to link such speculations with a hierarchy of development for various "forms of protest", without having once bothered to study the family economy and the gender division of labor within the proletariat. It is thereby irrelevant that Giesselmann makes "traditional patriarchal structures" – which as far as I am aware were in the process of crumbling in the first half of the nineteenth century – responsible for the fact that "women were immunized against the spirit of the revolts" (p. 901). The exact pendant to such a type of support on the part of women is a cliché that Giesselmann is not afraid to repeat imperturbably sixteen years after the publication of Michelle Perrot's essay on *La femme populaire rebelle*.<sup>7</sup> He thinks he has discovered the "characteristics" necessary for protest, characteristics which "the women in this period" "lacked", namely "the awareness of one's own needs and interests as well as the ability to be able to articulate these emphatically and energetically"

<sup>7</sup> Michelle Perrot, "La femme populaire rebelle", in Christiane Dufrancatel *et al.*, *L'Histoire sans qualité* (Paris, 1979), pp. 123–156.

(p. 901). He then concludes that it was “the battle for survival” that could have been primarily responsible for having “driven women, who had been raised to be gentle and submissive, to aggressive acts” (p. 902).

### “PRICE RIOTS” – THE PERFECT EXAMPLE FOR PROTEST RESEARCH

Since the publication of the initial studies in this field by George Rudé, R.B. Rose and Edward P. Thompson, the food riots of the eighteenth and nineteenth centuries have become the most popular topic of “protest research”. Giesselmann also gives “price riots” priority. He groups them with several other “sub-classes” under the heading of “socio-economic protest” as the first of his five “case studies”. Indifferent to the problems inherent in his method – a method “which reduces the complexity of the total event to several types of classification” – the author insists, not the least due to pragmatic considerations, that it is necessary to establish criteria of classification “according to which protest events can be ordered” (pp. 49ff.). This reflects to some degree the overall poverty of “protest research”. The historical context is not studied; instead the method is designed from the start (even prior to any electronic data-processing or quantification) to dissect such a context into single, isolated events, thus destroying it. It is replaced by so-called “categories” of protest, set up along the lines of the conceptually barren, superficial criterion of “common causes, occasions or participants” (*ibid.*). Specific events are wrenched from their historical context to illustrate these categories and are often not even presented in chronological order.

The fact that an entire cycle of social struggle can remain in the dark in such a study became particularly clear to me with regard to the subsistence riots of 1846–1847. Although Giesselmann refers to these in connection with several specific cases, he never discusses the overall context of these riots. He neither asks more comprehensive questions, such as how these revolts spread and how they were linked to strikes and wage battles, or what effect they had on the 1848 revolution. Nor does he sufficiently discuss the specific meaning of the subsistence riots during the July monarchy – as I will show shortly. He never addresses these questions because they are labeled “price riots” and are interpreted instead within the framework stipulated by this label, namely that of “hunger and revolts”, an approach that E.P. Thompson and Louise A. Tilly have been correct to question. According to Giesselmann, what is being studied are the “traditions of protest”, which “testify to [. . .] the continuity of the hunger problem that spans the centuries” (p. 51). Yet what the author ignores is that there is also a political continuity in the struggles for the first foodstuffs in France, a continuity that goes back

as far as the practice of *taxation populaire* and the General Maximumy during the Great Revolution.

In my opinion, Giesselmann does not consider the truly decisive issues and problems in his study because – as is not untypical for the “protest research” approach – he almost completely wipes out the aspect of class confrontation in subsistence riots. The creation of a national grain market in France, initiated by merchant capital (*Handelskapital*), totally altered the nature of the periodic hunger crises, which by then were influenced by large-scale speculation and were used to siphon off income from the masses, to accumulate capital and redirect it into new areas of industrial investment. The crisis of 1846–1847 illustrates this.<sup>8</sup> On this road to industrialization, the issue of the reproduction costs of the lower classes became critically important. Ways to control these costs became a central topic in social conflict beginning at the end of the *Ancien Régime* until the wage system and factory work – and thus the patriarchal structures within the proletariat as well – were finally established by the second half of the nineteenth century. And during this period of subsistence riots until 1848, the presence of women in the blockades of corn transports, mandatory price regulations and plundering were in no way merely the expression of elementary anxieties, as Giesselmann argues. They were an eminently political point in time, for it was the plundering women who first turned the reproduction issue into a public issue of collective appropriation and thus confronted the highly developed, nationally organized system of grain trade. There are also other important aspects that should be considered here: the absorption of the social movements and their demands before 1848 by the political staging of the February revolution, the superimposing of the subsistence issue by the issue of universal suffrage, the sweeping of the “rebellious women” from the street by republicanism, parties and parliaments as forms of masculinizing the public sphere.

As mentioned above, Giesselmann’s descriptive, disjointed “listing of several price riots” (p. 57) is based, although not always very exactly, on the abundant sources of the contemporary press (*Gazette des Tribunaux*, among others) and archival material (Archives Nationales de Paris, BB 18).<sup>9</sup> He summarizes his findings by stating that such riots “occur in the marketplace or the bakery”, that “market days” appear to have offered “the most favorable conditions for occurrence”, that the “triggering event” is a “price increase in produce or the shipment of produce to other markets”, and that “the actors in price riots [. . .]” are “urban or rural masses”, who “break existing law” (p. 58). The banality of

<sup>8</sup> See Karl Obermann, “Wirtschafts- und sozialpolitische Aspekte der Krise von 1845–1847 in Deutschland, insbesondere in Preußen”, *Jahrbuch für Wirtschaftsgeschichte*, 7 (1972), pp. 141–172.

<sup>9</sup> Albert Soboul was the first to point out the importance of these files in *Problèmes paysans de la révolution (1789–1848)* (Paris, 1976), p. 282.

such findings is reproduced to an even greater degree in a section on “causes and motives of price riots”. Here Giesselmann turns his attention to the “deprivation or poverty theory” (pp. 66ff.)<sup>10</sup> in which famine and “material poverty” cause “many of the price riots”. In the author’s words: “The central cause for this [for the continuation of the major famines; AM] lies in the insufficient productivity of French agriculture compared with the growing population” (p. 67).

In other words, the price riots can be explained in a Malthusian sense by the gap between population growth and agricultural production. Giesselmann also refers to the French economic historian Ernest Labrousse in order to give the “parallelism” of high grain prices and price riots a “causal connection between both phenomena” (p. 62). The first very simple question to ask at this point would have been what the price of wheat was in 1847 and how this price was arrived at. At any rate, it is clear from Giesselmann’s remarks on the 1846 crop failure that he did not waste a moment considering whether the subsistence crisis of that year could have been caused by something other than a periodically occurring food shortage, which provoked “price riots”.

This is then confirmed in the second volume of the work where the author handles “socio-economic factors” and once again returns to the 1847 crisis, which he – again in connection with Labrousse – defines as a “crisis of the older sort”, meaning one caused by agrarian sub-levels of production (p. 712) without considering the modern elements of crisis noted in research or even the influence of speculation on the Paris grain exchange.

As mentioned earlier, I think it is imperative to consider the historical development of the national grain market in France if we are to gain a better understanding of subsistence riots. One opportunity to do this would have been as Giesselmann quoted the essay by Louise A. Tilly on the food riots in France,<sup>11</sup> in which she correctly interprets the price riots in a political context and within the framework of the “long-term transformation of the grain market”. Giesselmann, whose “deprivation approach” at that point is simplified to a degree rejected by Louise Tilly, in turn rejects Tilly’s explanation as “*formule simpliste*” (p. 69), only to casually reintroduce it shortly thereafter under the guise of being his own insight into the “structural causes of price riots”:

several structural causes of the price riots [become] evident, namely the induced process of a capitalist transformation of the French economy, which manifests itself in an increasing commercialization of agrarian production and the creation

<sup>10</sup> Giesselmann is not particularly exact in his wording. Sometimes he refers to “deprivation theory” (p. 66), other times to “deprivation thesis” (p. 68) or “deprivation approach” (p. 1004), and occasionally “deprivation” is also labeled as “category” (p. 955). This impression alone gives cause to doubt the scientific nature of the author’s terminology.

<sup>11</sup> Louise A. Tilly, “The Food Riot as a Form of Political Conflict in France”, *Journal of Interdisciplinary History*, 11 (1971), pp. 23–57.

of a national market [this is just what Louise Tilly argues; AM]. This capitalist modernization is resisted by the lower classes who must at first pay the price for this development, with backward-looking actions of a defensive-reactive nature. (p. 74; see also p. 93)

How is it that the author characterizes anti-capitalist actions as being "backward-looking"? The stereotype of a "backward-looking anti-capitalism" is known to have come from Marxist historiography, but it has been adopted since by "protest research" as its own, usually with reference to E.P. Thompson. Taking Thompson's criticism of "economic reductionism" into account, Giesselmann introduces another normative and traditionally determined factor for inflation protest, besides deprivation, to explain the "causes and motives".<sup>12</sup> This is what Giesselmann has labeled, in a hideous choice of words, the "ethno-cultural [*volkskulturell*] approach" (pp. 69, 641). Thompson, who has always studied the transformation of "traditional patterns of behavior" and "cultural norms" (p. 60) within the context of struggle, is quoted like a key witness on behalf of the traditionalism of the lower-class culture.

This is not the place for me to demonstrate how the ideas of E.P. Thompson and his concept of moral economy have been used in West German historiography until they have become threadbare. Perhaps it will suffice to present phrases quoted from Giesselmann's work in order to communicate the proper impression. In page after page, phrases such as the following are used like a *passé-partout*: the "traditional norms of the plebeian economy" (p. 72), the "traditional plebeian concepts of a moral and paternalistic economy" (p. 80), the "traditional concepts of a moral economy and a paternalist elite" (p. 107), the "workers' traditional paternalist model of the elite" and the "moral economy of the lower classes" (p. 109), etc.

These are not used solely in connection with price riots but finally also to explain agrarian unrest and "workers' protest". Perhaps I do not fully understand E.P. Thompson's underlying concept. I had the impression that his work showed the turning-point at which the forms of legitimization for a moral economy – derived from the subsistence economy and the village community – change into a permanent threat of revolt by the poor effective enough to influence the market regulation of corn prices and reproduction costs: in other words, it changes into the "most modern" form of social antagonism. However, it seems to me that Giesselmann uses a rather abbreviated definition of moral economy if he applies it to his analysis as an adjunct, so to speak, to the "deprivation thesis" in order to inject the element of subjectivity and provide proof of the following - or rather, in place of such proof:

<sup>12</sup> It does make a difference, by the way, whether one is referring to the "causes" of the riots or from the "motives", meaning the normative justifications. Giesselmann does not always sufficiently distinguish between the two.

All of the normative justifications of inflation protest analyzed to date have been traditional in nature. They are derived from socio-culturally anchored norms and value concepts belonging to the plebeian milieu that have evolved throughout the course of centuries, and they emphasize the defensive, backward-looking character of the actions. (p. 82, similarly stated on p. 93)

Finally, Giesselmann does not offer us any closer analysis of the connection between the subsistence riots of 1846–1847 and the revolution of 1848. After having discussed the “precipitous increase in the frequency of protest” in 1847 in conjunction with the quantification of protest, he then adds: “This mobilization continues to rise and finally breaks the dam in February 1848. The revolt turns into a revolution” (p. 585).

What more does this say than that one event followed another? Giesselmann attempts to explain this in a quote that I include here because the way in which he argues is typical for the entire study:

The revolution is in no way the inevitable result, practically the culmination, of the preceding wave of protest; instead, this waning movement is rekindled by it. However this does not mean that the revolts of 1846–47 were irrelevant for the outbreak of the revolution. Rather, the discontent expressed in these continues to exist and the lasting impact of the revolts is particularly evident in the political and psychological sphere. (p. 586)

I think it is a crude simplification to argue that these two events are linked by “continuing discontent” without considering – to give just one example – the public agitation with which the press opposing the July monarchy, first and foremost *La Réforme* and *Le National*, seized the movement of 1846–1847 and reshaped it into a powerful political campaign for universal suffrage on the eve of the February revolution.

I have the impression that Giesselmann – with regard to the “price riots” as well as later in his book – adopts the set pieces from all theories and approaches in social history in order, time and again, to offer new “conclusions” without increasing our knowledge of protest one bit. True to academic conventions, Giesselmann tries to cover up the fact that he is theoretically treading water by referring to the “complexity” of the topic, which in his own words lead to a “multi-factor approach”:

Even if this conclusion must be satisfied with merely having identified the most important determining factors of the price riots, it has still revealed that the causes of the riots were much more complex than several influential theories lead one to expect. Certainly these events are a “mutiny of hunger” (Marx) or a “form of political conflict” (Louise Tilly), and they are indeed grounded in the culturally transmitted “concepts of legitimacy” of the lower classes (Thompson). These interpretations are not wrong, but they do not depict the entire truth. They are incomplete and concentrate only on various aspects. But the price riots can only be fully understood once the various socio-economic, political, cultural, and socio-psychological determining factors have been integrated with one another. Such a multi-causal approach is not allowed to evade the problem of properly weighing the influence of each of these individual

factors. With regard to the priority ranking of these factors, it is clear that top priority must be given to the short-term economic factors, the agrarian crisis of low productivity and the deprivation accompanying this that affected larger sectors of the population. They provided the decisive catalyst that set the other causal mechanisms in motion. The merely periodic eruption of price riots resulting from crop failures clearly demonstrates this priority. (p. 94)

So in the final analysis we learn nothing more than the fact that the "price riots" were caused by crop failures and "inflation-related deprivation".

### RESULTS: "THE CONCEPT OF 'RELATIVE DEPRIVATION' "

I have concentrated solely on the chapter on "price riots" because the other case studies in Giesselmann's book follow a similar outline. In particular, the summaries of each of the chapters are all worded nearly the same (see pp. 193, 217ff.). Therefore, I will forgo addressing the problems, for example, that I see in applying the term "workers' protest", a term which incorporates such a mass of other "protest events" that the significance of the strikes and wage battles of French artisan-workers during the July monarchy is lost.<sup>13</sup> Furthermore, I will forgo discussing the section on "the Left" (pp. 326ff.) as well as the scattered remarks on early socialism, particularly because they are completely unsatisfactory compared with the findings of recent research. It is just not enough to present a case using such clichés as the "motley multiplicity of early socialist ideas" (p. 163), the "ambivalent and confused theories of Saint-Simon and Fourier", or the "militant-insurgent concepts of Blanqui and the Babouvists" (pp. 975ff.). In search of an explanation for the lack of "ideological coherency within the Left wing", meaning specifically the separation of the bourgeois-republican movement from the social movement, the reader does not appreciate the reference to a "biologically related turnover within the leadership of the Left" (pp. 334, 347). He wants neither to have the developments after 1830 simplified to the "road from spontaneous mass demonstration to an uprising of the people, and finally to overthrow highly organized underground terror" (p. 391), nor to find that the *Bund der Gerechten* is only mentioned in a footnote (p. 387). It is also not right to maintain that the "impetus" from oppositional liberals, social democrats and communists was "the same" (p. 348) during the banquets of 1840 – not to mention all the other inaccuracies.<sup>14</sup> All that remains to be said is that the author is overtaxed by the scope of the topics he addresses.

<sup>13</sup> A chief source for Giesselmann's case studies on "workers' protest" is the study by J.-P. Aguet, *Contribution à l'histoire du mouvement ouvrier français. Les grèves sous la Monarchie de Juillet (1830–1847)* (Geneva, 1954).

<sup>14</sup> On page 346 we read: "Babeuf's fellow conspirator Buonarotti, who returned to France during the July monarchy, had with his *Conspiration des Egaux* (1838) [. . .]", etc. The author's name was Buonarrotti, the book was published in 1828 in Brussels under the title *Conspiration pour l'Égalité dite de Babeuf*.

In conclusion I would like to turn my attention to the several summaries of Giesselmann's findings. It can be said that the pretentious nature of the findings presented fits perfectly to the unpretentiousness of the questions posed by the author at the beginning of his work. The "questions posed in this study" (pp. 28ff., see also pp. 1030ff.) are formulated with the aid of interrogative pronouns, namely the "when", "where", "who", "why", "how", and "what for" of protest. At the conclusion of the first volume, Giesselmann does admit in an "interim evaluation" that the method of analysis applied up to that point in his book, namely the "mere lining-up of individual case studies", has only produced a group of "disarranged pieces of a mosaic" (p. 548) but no "overall picture of protest". In order to "examine the entirety, the general characteristics and unwritten laws of protest during the July monarchy" and to grasp the "totality of the protest phenomenon", he repeats yet again – and not for the last time – his question of "why":

The question [to be] systematically and intensely examined in the third section is one that revealed its central importance in the case studies but could only be partially explained up to this point, namely the question of "why". The obtained findings of these studies will be combined in order to develop a theory applicable to the entire protest phenomenon during the July monarchy. This is the most important goal of our study. Therefore, it will be necessary to conduct a thorough causal analysis, one which identifies and examines all determining factors. (p. 549)

What Giesselmann selects "from the wide variety of available theories" and presents as a "theory on the entire phenomenon of protest" could be accused of being eclecticism if the author himself had not anticipated this (p. 649). Since Giesselmann maintains that it is not possible to develop "'one' comprehensive theory of protest", he wishes instead to offer the aforementioned "multi-factor approach", which is argued to be the "joining of a set of medium-range hypotheses" (p. 650). Distinctions are drawn here between demographic, socio-economic, political and socio-cultural factors, as well as between collective motives of protest and "micro-level" factors specific to each situation and personality (meaning the occasion, location, time and the "rebellious subject" as protagonists). Giesselmann studies how these factors interact with one another. However, in his compilation of the "socio-economic factors of protest", he is quick to confirm what could be read between the lines of the case study presentations: with certain reservations, the author favors the so-called deprivation theory. The generally known symptoms of early industrial mass poverty, such as starvation, low wages, unhealthy living and working conditions, and a high death rate, are all lumped together under this general term; hence, "deprivation" is specified "as the most important social cause of protest during the July monarchy" (p. 736). Although it is supposed to help explain what causes "protest", in actual fact the term becomes meaningless.

Whereas deprivation at least appears to refer to objective facts, this impression vanishes rapidly when Giesselmann switches from analyzing the causes of protest to studying the motives. In the fifth chapter of the second volume, the "collective motives of protest" are discussed, again with the promise of "a systematic summary and intensified analysis of the fragmentary findings that have already been ascertained and presented in the various chapters of the study" (p. 954). At this point, the presentation takes on a psychological flair:

Our research has revealed that a feeling of social discontent was the dominating element in the structure of motivations driving the rebels. This is the emotional reaction to the experience of deprivation, another important and complementary category in our analysis. Deprivation stands for the perceived discrepancy between the social needs, demands and expectations of an individual and his capacities to attain these. The individual involved registers this discrepancy as want, neglect and sacrifice, and since this discrepancy affects indispensable or at least essential values, it generates in the person intense feelings of discontent. (p. 955)

The supposition that a "feeling of social discontent" was the "main motive" driving the French to protest during the July monarchy is as banal a statement as it is a difficult one to prove empirically. However, Giesselmann argues that the "poverty theory of protest has limitations" that cannot be overlooked because poverty does not inevitably lead to revolt; all too often it leads to resignation and apathy. Besides, many a protester lived under "thoroughly bearable material conditions", whatever that is supposed to mean. According to him, we should view deprivation not only in the sense of a "tangible threat to one's existence" due to the poor provision of life's bare necessities, but we should see it more as a "feeling of discontent", one which is dependent on "individual levels of expectation", or more specifically on "subjective processes of comparison". If I have read this correctly, it means that the concept of "relative deprivation", as Giesselmann presents it, could be used as a "frustration theory of protest" to supplement the "poverty theory" (pp. 956–957). I have selected the following excerpts from the various attempts of the author to define this concept: "Relative deprivation" is introduced as "a category of social psychology and not one for social structures", "that reveals a discrepancy between value expectations and the capabilities to fulfill them, that is, the fact of disappointment and frustration" (p. 957).

Relative deprivation, understood as the frustration-causing discrepancy between value demands and capabilities to satisfy these, is generated by an increase of the former and/or by the decrease of the latter. (p. 961)

I am not sure whether this concept is supposed to be compatible with the analysis in social science that views the increase of social demands by the lower social classes, such as for income, higher wages and

improved living standards, as being explainable in the context of a *revolution of expectations*, meaning the articulation of lower-class expectations during a period of epochal change. If Giesselmann did indeed mean this, then the “horizon of expectations” (p. 957), widened by the July revolution and by the entire cycle of bourgeois revolution, could not be viewed anyway as an individual and subjective one. And the dynamics of social movements in the period of transition to industrial capitalism could also hardly be explained solely by social-psychological motives, by social envy, a frustrating comparison of status (pp. 959, 962) and by the waxing and waning of expectations and frustrations – even if the rhythm of economic prosperity and crisis is incorporated into this, as occasionally it is in Giesselmann’s study.

Giesselmann does introduce a series of other “determining factors” or “normative justifications” besides discontent to explain the causes of protest. When discussing political factors, he particularly underscores the “insufficiency” of the Orléans police apparatus and methods of repression as well as “a deficient social control”, meaning inflexibility on the part of the regime (pp. 882, 889ff., 1018). Giesselmann also discusses several other theses, some of which he incorporates, others of which he rejects (e.g. the “mob and irrationality thesis” of protest). Even though Giesselmann makes the effort to review these other factors and theses, in the end it is the aforementioned “concept of ‘relative deprivation’” that prevails in the conclusion of the third and final section of the study, in which the author attempts to link the “factors of protest” into a “causal model” of protest. Giesselmann then maintains, as he has countless times before in this study, that this concept is “an improvement over the original deprivation approach”, because:

More than a few of our rebels do not live in particularly dire straits and can at times even boast of a modest amount of comfort. Despite this, they consider themselves to be subjectively deprived in a socio-economic sense. (p. 1002)

If this comes across as being conceived too simplistically, especially in light of the “social portrait” of the “typical rebel of the July monarchy”<sup>15</sup> that Giesselmann includes earlier in his book, the following should compensate for it:

The elastic concept of relative deprivation increases the efficiency of the socio-economic analytical approach [. . .] (p. 1003)

In addition to these structural and procedural generators of conflict, there are still other determining factors that are also to be found on the macro-level. (p. 1007)

<sup>15</sup> The “wanted” poster reads: “This is a man between 21 and 30-years-old, single, living in a small, rural community, who was born and lives in the *département* in which he is being tried, illiterate, employed in agriculture, no previous convictions, and it can be added, a member of the lower class” (p. 620).

On the political level it is also possible to identify further intervening, catalytic macro-influences. (p. 1009)

These excerpts have been presented here solely because I find it quite puzzling that no advice from the editor or publisher during one of the normal procedural steps of editing a scientific manuscript apparently hindered the author from making such intellectually inflated statements. The ultimate "summary" of the book reviewed here reads:

We will content ourselves with these thoughts on linking the factors of protest that have been discussed. [. . .] Above all, the delineation of the causes depicted here has explained the most frequent and typical path to protest. It [the path? the protest?] is completed in three steps: it emerges from causes located on the macro-level, the outstanding of these being the fact of deprivation in the social structure, then turns to the motives found on the level of the subjects involved, where the feeling of social discontent is the most important, and finally arrives at the action level with the outbreak of protest in its chronological, spatial and social allotment. (pp. 1017ff.)

I do not want to overlook Giesselmann's concluding remarks. Here the scientific jargon of the author, which has coagulated into stereotypes, is compensated by inspired commentary. As we know, the author does not want to be accused of "callousness" towards his subject, although he thinks he is fully aware of the "detached sobriety and abstraction saturated with theory" that accompany the "scientific treatment" of a subject. As a way out of this conflict, he finds it helps to acknowledge that "the act of revolt [. . .] also [possess] a stirring dimension of humanity and morality" (p. 1044). One can predict the discourse on Camus that then follows.

Let me not be misunderstood: I am not polemicizing here against Giesselmann's intent, as stated on the last page of the book, to pursue a historiography of a critically enlightened nature, one dedicated to emancipatory developments, as opposed to the historiographic tradition in Germany dominated by conservative elitism. This claim is one he shares with an entire generation of historians. However, I doubt that such a claim can be taken seriously if one reverts to the following statements upon the conclusion of a scientific study:

Protest [represents] a matter of conscience, nearly an act of faith, and is therefore not drained of its meaning by mere denial. [. . .] Thus protest is an essential and indisputable dimension of man and his history. (pp. 1046ff.)

Giesselmann's "appeal on behalf of the relevance of protest research", which is supposed to evoke the sympathy of the historian for the cause of civil disobedience, indicates once more that this sort of research has been plagued by the programmatic omission of distinct historical terminology.