Was Franz Baur’s infamous long-range weather forecast for the winter of 1941/42 on the Eastern Front really wrong?

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ABSTRACT

In October 1941, Nazi Germany’s High Command realized that the war against the Soviet Union could not be ended before winter. The German professor Franz Baur prepared a long-range weather forecast for the winter of 1941/42. Baur never revealed anything about this forecast. However, according to an article published ten years after Baur’s death, Baur predicted that the winter of 1941/42 would be normal or milder than normal, primarily based on the main argument that the previous two winters had been very severe and never in climatic history had more than two severe winters occurred in a row. The winter ended up being one of the worst.

Today, Baur’s prognoses from wartime are public. In this article, it is shown that the previous description of Baur’s prognosis for the winter of 1941/42 is incorrect. Baur had a problematic relationship with his colleagues, so it is possible that the story of his prognosis is incorrect due to personal and professional contradictions. Baur’s postulated prognosis for the winter of 1941/42 destroyed his reputation. Based on the original prognoses from wartime and Baur’s scientific and personal history, this article shows that this judgment was too harsh and unfair.

CAPSULE SUMMARY

This study analyzes the possible human and scientific reasons for one of meteorological history’s greatest postulated mistakes, Franz Baur’s long-range weather forecast for the winter of 1941/42.
In 1975, the American Meteorological Society began publishing a series of articles called "Great Historical Events That Were Significantly Affected by Weather". The articles went back all the way to the Mongol invasion of Japan in the 12th century. By 1987, part 8 was reached: "Germany's War on the Soviet Union, 1941-45. 1. Long-range weather forecast for 1941-42 and climatological studies" by Jehuda Neumann and Hermann Flohn (1987). Their article is partly about the German meteorologist Franz Baur's prediction of the weather on the German Eastern Front for the coming winter of 1941/42. Franz Baur argued that this winter would be normal or milder than normal. The winter ended up being one of the worst in human memory. The main reason for Baur's prediction was, according to Neumann and Flohn (1987, p. 621), that the previous two winters had been very severe and “never in climatic history did more than two severe winters occur in a row”. When temperatures on the Eastern Front dropped sharply in early December, Baur was asked if he maintained his forecast. He answered that “the observations must be wrong” (Neumann and Flohn 1987, p. 622).

Was Baur just unusually stubborn, was he a bad statistician, was he a resistance fighter, or was there another explanation for his failure? It is, of course, problematic to assess this more than 80 years after the events took place. However, Franz Baur is now a historical person whom it is fair to examine further.

Contrary to the successful weather forecast for D-Day, see, for example, Krick and Fleming (1954), Stagg (1971), Petterssen (1974 and 2001), and Ross (2014), very little
material has been available about Baur's long-range forecast. Baur never revealed anything about this forecast or other details about his work during the war. Three published sources are particularly relevant: Baur's necrology written by Helmut Landsberg (1978), the previously mentioned article by Neumann and Flohn (1987) and a 74-page transcript of an interview with Hermann Flohn, one of Germany's best-known climatologists (Flohn 1992). In addition, there are scientific articles by Baur himself, but none of these are related to the events during the war. However, the archives of the German Meteorological Library at the German Meteorological Service (Deutscher Wetterdienst, DWD) include many original materials signed by Baur. The material comprises monthly and seasonal long-range weather forecasts from the war, including the prognosis for the winter of 1941/42. In this article, this prognosis is compared with the description by Neumann and Flohn (1987). The article also investigates the man behind the scientist based on what Franz Baur and some of his closest colleagues wrote before, during and after the war to determine, if possible, the reason for his unfortunate fate.

Franz Baur, “a tragic loner”

The prewar times

Franz Baur was born in Munich in 1887. His father was a colonel, so it was natural for Franz Baur to enter the military after graduating from high school (Landsberg 1978, p. 310). During World War I, Franz Baur suffered a serious riding accident that left him with a severe concussion and permanent psychological injuries (Flohn 1992, p. 13). By the end of the war in 1918, at the age of 31, Franz Baur had become captain. More than ten years in the Imperial Army must have influenced the young Baur. After the war, he left the military like so many others. He studied physics, mathematics, geography and meteorology at the Universities of Munich and Freiburg. He received his doctorate in 1921 from the University of Freiburg (Landsberg 1978, p. 310). The speed with which he received his doctorate likely reflects the benefits of his education as an officer. During his studies, he became the head of a medical-meteorological institution in St. Blasien in the Black Forest.

Franz Baur's main interest, however, was long-range forecasting. This interest had already been aroused during his studies in Munich when Professor August Schmauß, to
Baur's irritation, stated during a lecture that it would likely never be possible to make useful long-range forecasts, as Baur recounted many years later in a 1961 interview (Die Zeit 1961). Professor Schmauß was already an important person in the German meteorological community. In 1923, he became chairman of the German Meteorological Society (Deutsche Meteorologische Gesellschaft, DMG), a post he held until his death in 1954, although the association went into hibernation in 1945.

As early as February 1923, Baur published a forecast for March 1923 (Landsberg 1978, p. 310). Baur's work received much attention within the German agricultural scientific community, which encouraged him to write a so-called monograph, the traditional approach to permanent employment in the German university world at that time. However, he faced "considerable opposition by the orthodox meteorological community, which simply considered long-range forecasting outside the serious meteorological endeavours" (Landsberg 1978, p. 310). That is, Schmauß's attitude was not exclusive. Despite this resistance, Baur managed to publish his monograph in 1926 as a book entitled “Premises for Seasonal Temperature Forecasts for Germany” (Baur 1926). From then on, according to Landsberg (1978, p. 310), Baur was a controversial figure in the meteorological community. In a photo from the annual meeting in Karlsruhe in 1926 held by DMG, Baur is seen standing in the background and looking in a different direction than everyone else (Fig. 2).

![Participants of the annual meeting in Karlsruhe in 1926 held by Deutsche Meteorologische Gesellschaft (DMG) - Franz Baur stands on the far left as No 4.](image-url)
point of time, he already seems to be in "bad standing" among colleagues. No. 20 is Professor August Schmauß, long-time chairman of DMG. No. 47 is the world-famous Alfred Wegener, who is better known today as the father of continental drift theory than a meteorologist. No. 19 is Ludwig Friedrich Weickmann, and No. 51 is Kurt Diesing, who are both mentioned later in the text. From Archiv des Meteorologischen Instituts (2021) and Lüdecke (2008).

However, Baur used his popularity in agricultural circles to persuade the Ministry of Agriculture to set up a small new research institution in Frankfurt in 1929, the Research Centre for Long-Range Weather Forecasting (Forschungstelle für langfristige Witterungsvorhersage), with him as leader (Baur 1936, p. 148). The institute was later moved to Bad Homburg just north of Frankfurt, still with Baur as the leader. In the mid-thirties, the institute became a part of the meteorological service of the German government (Reichsamt für Wetterdienst) and attached to the air force (Luftwaffe). Here, Baur and his staff periodically prepared 5- and 10-day forecasts during the summer months from 1932 until the outbreak of the war (Baur 1972, p. 147). Baur presented the theoretical background for these prognoses in an article in BAMS in which he concluded that “the 10-day forecasts rest on a combination of statistics and synoptics” (Baur 1936, p. 149). According to American studies, the five-day forecasts were reasonably good, while a similar judgment could not be made about the 10-day forecasts (Landsberg 1978, p. 310).

Baur also taught at the University of Frankfurt, where in 1930 he was appointed as an honorary professor (Putnins 1967, p. 408). Here, both Landsberg and Flohn followed his lectures in statistics and vector calculations (Landsberg 1978, p. 310, and Flohn 1992, p. 49). Landsberg calls him an excellent lecturer whom unfortunately only a handful of students followed. The fact that Baur taught statistics and wrote a textbook about correlation calculations (Baur 1928a) and scientific articles on the basis of statistical methods (e.g., Baur 1928b; 1930) shows that Baur definitely had a high level of knowledge and experience in statistics. In addition to following Baur's lectures, the 25-year younger Flohn also performed some small tasks for Baur (Flohn 1992, p. 49). This might have led to a good relationship between master and apprentice but did not, as Flohn, like so many others, was skeptical of Baur's long-range forecasts (Flohn 1992, p. 49).
49). However, Baur’s concept of Grosswetterlage (large-scale weather situation) gave Baur some recognition (Flohn 1992, p. 13 and 48). By looking at the weather on a large scale spatially and temporally, Baur believed he could uncover statistically significant patterns in overall weather development.

A great admirer of Baur's methods was the American meteorologist Irving Krick, who visited Baur in Bad Homburg in 1934 (Ross 2014, p. 90). Krick further developed Baur's principles into the so-called analog method. It was Krick's weather forecasts based on the analog method that could have led to a disastrous landing on 5 June 1944 instead of the successful landing on 6 June (Ross 2014, p. xiii).

**Wartime**

On 22 June 1941, the Germans began a campaign against the Soviet Union with the aim of ending the campaign before winter. The soldiers therefore had no winter equipment, not even in the depots behind the front. The equipment, e.g., guns, rifles and tanks, was not suitable for heavy frost and snow, in contrast to the Soviets’ equipment. In the first few months, the campaign went as planned for the Germans, but then came autumn with rain and mud, the time of year that the Germans call *Schlamm Periode* and the Russians more poetically refer to as *rasputiza* (Stahel 2013, p. 95). The Germans faced poor mobility and were stuck for four weeks (see Fig. 3).

The High Command realized that the campaign could not be completed before winter; thus, it was important to know how cold the winter would be. According to Neumann and Flohn (1987, p. 621), “Baur was requested by the headquarters (HQ) of the German Air Force to distribute his long-range forecasts to about 25 military offices. A forecast for winter 1941-42 was issued by him, probably at the end of October 1941, based on regional climatology and (supposed) sun-spot-climate relationships. The prediction called for a normal or a mild winter. Baur’s main justification for this rested with the assertion that never in climatic history did more than two severe winters occur in a row. Since both of the preceding two winters, 1939-40 and 1940-41, were severe in Europe, he did not expect that the forthcoming winter would also be severe.” As we all know, the winter of 1941-42 was very harsh, and the German troops suffered greatly (Lejenäs 1989).
Franz Baur did not write anything publicly about his activities during the war. Five years before his death, Baur explained the reason for this (Baur 1972, p. 147): “Only during the Second World War did the author [Baur] regularly prepare monthly forecasts, but not voluntarily, but on the orders of the High Command of the Air Force and only for this [the High Command] under the express condition that no publication takes place, which hasn’t happened either”. Nor has anyone else written about Baur’s professional wartime activities during his life time. In an article in BAMS by Pauls Putnins on the occasion of Baur's 80th birthday in 1967, there is no mention of Baur's work during the war (Putnins 1967, p. 408). However, in the necrology in BAMS, Landsberg (1978, p. 310) wrote slightly more: “No documentation or reliable information exists on the role Baur played in forecasting weather for campaigns, but rumors to that effect persisted. Particularly tragic was a controversy swirling around Baur and his most eminent scientific collaborator, Dr. Horst Philipps.” The incident with Philipps will be commented on later. Here, it is important to note that it was not until ten years after Baur’s death that significant new information, perhaps the “rumors”, was published in the article by Neumann and Flohn (1987). Their description of Baur’s wartime activities has no references to original archival material and is probably largely based on Flohn's memoirs from his years in the weather service.-Flohn’s memories are not in favor of Baur’s reputation, which thus suffered a major blow ten years after his death. To date, the description by Neumann and Flohn has been the “authorized” source on Baur’s wartime activities. The article has been referenced in professional contexts (Teich 1993, p. 684;
Rasmussen 2010, p. 322) and by the press on Baur's 125th birthday (Frey 2012, p. 62). Frey even wrote, “It was his most famous – and probably the most famous wrong prognosis of all time”.

Today, Baur’s original prognosis for the winter of 1941/42 is publicly available at the German Meteorological Library at the German Meteorological Service (Deutscher Wetterdienst, DWD), in addition to 83 other long-range weather forecasts prepared by Baur from February 1940 to November 1943, totaling 765 pages. The material includes general monthly and seasonal prognoses for large parts of Europe, as well as prognoses for special areas, for instance, in September 1940 for England and the English Channel. For the present analysis, four prognoses are especially interesting: the prognoses for the winters of 1940/41, 1941/42, 1942/43, and 1943/44 (Baur 1940; 1941; 1942a; 1942b; 1943).

The content of Neumann and Flohn's article differs somewhat from the content of the archival material found, as will be documented. At the top of the cover of the prognosis for the winter of 1941/42 (Baur 1941), the red-stamped word “Secret” has been crossed out and replaced with “For official use only”. It has not been possible to clarify when this change occurred and on which date Baur’s wartime prognoses were made publicly available (D. Wintergerst, Bundesarchiv, Abteilung Militärarchiv, 2022, personal communication). The other text on the front page reads, “Temperature forecast for the winter of 1941/42 (with 3 supplements) edited in the Research Institute for Long-Term Weather Forecasting under the direction of Prof. Dr. Franz Baur. 21. Copy.” This is followed by two pages with the forecast itself and three pages with figures. The forecast is signed, “Bad Homburg, 5. November 1941, F. Baur.” The date 5 November is one month later than remembered by Flohn. As Baur made a new forecast at the end of every month, either for the next month or the next season (three months), it is understandable if Flohn remembered the date incorrectly. The number of copies, 21, is consistent with Flohn's indication that the forecasts were circulated to 25 services.

The main part of Baur’s long-range forecast regarding the Eastern Front reads (the underscores are Baur’s): “On average in terms of time and space the winter will neither in Western and Central Europe nor in Northern and Eastern Europe be severe. In particular, for Northern and Eastern Europe, the probability that the average temperature of winter is above the normal value is greater than the probability of a winter that is too cold.” Figure 3 in the appendices to the forecast (Baur 1941, p. 5) shows that the probability of a milder
winter than normal ranges from approximately 0.5 in Central Europe to above 0.8 around Moscow. This means, on the other hand, that the probability for a winter colder than normal was estimated to be less than 0.2 around Moscow. This prognosis is not inconsistent with “a normal or a mild winter”, as stated by Neumann and Flohn. However, they did not mention Baur’s reservations related to the probability calculations.

The three main justifications for Baur’s forecast were as follows (Baur 1941, pp. 2-3):

1) “…a special procedure based on multi-year rhythms”

2) “…that in the last 110 years after Octobers, which - like X in 1941 - were more than 10 C too cold in Central Russia, November and December were often too cold, but in 2/3 of all cases the winter (December, January, February) as a whole was warmer than normal both in Russia and in Central Europe”.

3) “The overall development of large-scale weather in late summer and autumn was broadly similar in 1941 to that of 1890.”

Point 1 has no counterpart in Flohn’s list of Baur’s justifications. Today, the rhythm theory (some kind of periodicities) is abandoned, while the influence of sun spots is still under debate. It seems likely that, so many years after the war, Flohn mistakenly confused these two controversial methods. Flohn’s use of parentheses “…(supposed) sun-spot-climate relationships”, implies that he was not in favor of this theory.

Point 2 was based on 110 years of statistics on the dependence of winter temperatures on autumn temperatures. This is not the same as analyzing the dependence of winter temperatures on the winter temperatures in preceding years as postulated by Flohn. The sentence “but in 2/3 of all cases” in Baur’s argumentation is far from the phrase “never in climatic history” used by Flohn. It is possible that Baur conducted a further investigation of consecutive winter temperatures that was not recorded. A year before, he did use such an analysis—among several other analyses—to conclude: “Will the coming winter [i.e., 1940/41] in the middle of Europe again be so severe as the last one? This question can be answered with a completely no” (Baur 1940, p. 2). He again returned to such methods in the prognoses for 1942/43 (Baur 1942a; 1942b). Appendix A shows that the winter temperature in Central Europe is independent of temperatures in the preceding winters.
Point 3 seems to be what Flohn called “regional climatology.” Such a comparison with older observations corresponds to the analog method used intensely by Krick. Here, there seems to be no discrepancy between the two sources.

It may be concluded that Neumann and Flohn’s description of Baur’s long-range forecast for the winter of 1941-42 is incorrect. He was not so definitive in his forecast as reproduced by Neumann and Flohn, and his argumentation rested only partially on the methods stated by Neumann and Flohn.

![Graph showing morning temperatures at Moscow](image)

**Fig. 4.** Morning temperatures at Moscow 07 LST, local solar time) in late November and the beginning of December 1941 based on data from Neumann and Flohn (1987 p. 621).

In early December 1941, the weather suddenly turned unusually cold (see Fig. 4), which led to the following strange episode (Neumann and Flohn, 1987, p. 622): “On or about 8 December, K. Diesing, chief of the CWG and scientific adviser to the chief of the weather service of the Air Force (General Spang), asked Flohn to listen in on a second earphone to a telephone call to Baur. In the call, Diesing cited to Baur the reports of very low temperatures in the East and asked him if he maintained his seasonal forecast in face of the reports. Baur’s response was ‘the observations must be wrong’. This reply of Baur’s greatly impaired the esteem held for him by his colleagues.” The CWG was the Central Weather Group (Zentrale Wetterdienstgruppe, ZWG). According to Flohn (1992,
p. 49), Diesing and Flohn stared at each other in dismay. As already documented about the winter 1940/41 prognosis, there are some discrepancies between the archival material and the description based on Flohn’s memories. Therefore, the record of the telephone conversation has to be handled with some reservation. It is not clear from Flohn’s memoir whether Baur was challenged in his response. However, it would have been natural to discuss the response in more detail. Nor is it apparent from Flohn's memoirs whether Baur was aware that Flohn was listening. If Baur were, then we would have had a situation where the 54-year-old professor was called to account in front of Diesing and Flohn, the latter his 25-year younger former student. It would hardly have been a pleasant situation for the former imperial captain. It seems very human that Baur became a little stubborn and maintained his prognosis. Or was the answer in fact meant to be sarcastic? If he meant that a few measurements in early December could not overturn his long-range forecast for the whole winter of 1941/42, then that was logical enough from the point of view of the forecasting methods he had developed. About ten years later, he writes—without any reference to the 1941 forecast—that a winter predicted to be warmer than usual could naturally include a very cold month. “But only one such month can be involved since we know that the average temperature of the whole winter is above normal” (Baur 1951, p. 325). Perhaps he believed so much in his forecast in 1941 that he thought that an unusually cold December would result in even higher temperatures in January and February. That he still argued for such a view on a general level ten years later is surprising.

Attempts to present other more pessimistic forecasts to Hitler were halted, including by Göring, who stated that "in Russia it will never be colder than minus 15 degrees" (Neumann and Flohn 1987, p. 626). After a conference on 7 December 1941 about the weather situation in the East, Hitler repeatedly said "If I had known this before" (Neumann and Flohn 1987, 627). It was a complete abdication of responsibility because an army should never be sent into Russia without solid winter equipment, regardless of optimistic or pessimistic weather forecasts. Hitler knew, however, that things could go terribly wrong, as happened for Napoleon in 1812 (see Fig. 5). Hitler therefore forbade all mention of Napoleon's failed campaign and the terrible winter that cost the lives of so many soldiers (Neumann and Flohn 1987, p. 625).
A different potential explanation of Baur's inaccurate prognosis is that Baur was so much opposed to the Nazi regime that he deliberately presented a prognosis that would lead the German armies into misfortune. That he was against the regime appears in the interview with Hermann Flohn (Flohn 1992, p. 21). Flohn was an active meteorologist during the war, and the interviewer asked if there were regime opponents among the many meteorologists. Apart from a few Jews who had escaped before the war (including Helmut Landsberg), Flohn could only remember two: Horst Philipps and Franz Baur. However, their opposition had vastly different backgrounds: Philipps was strongly left wing, while Baur was grounded in the imperial, right-wing officer environment, in which there were many opponents of the Nazis. Philipps and Baur had worked together but came into conflict in 1941. The conflict led to Philipps’s dismissal. After the war, Baur was held accountable for this incident by a so-called denazification court (Landsberg 1978, 310). He was acquitted but fined for his membership in a Nazi organization. Although Baur may have been skeptical of the Nazi regime, it seems unlikely that, as a former
officer and an ambitious long-range weather forecaster, he would have deliberately produced an erroneous forecast leading German soldiers into misfortune.

Diesing died in June 1943 and was succeeded by Werner Schwerdtfeger as head of ZWG. After Schwerdtfeger’s death in 1985, his relatives found a description of an episode he had written a year before he died, which Neuman and Flohn (1987, p. 622) summarized and commented on as follows: “…during the summer of 1942 the field HQ Hitler requested a forecast of temperatures in Eastern and Central Europe during the winter of 1942-43 and that, in turn, Diesing asked Baur to prepare the forecast. According to Schwerdtfeger, Baur’s long-range forecast was that the coming winter would not be cold, since the preceding three winters, and particularly two of the three were rather cold; a sequence of four cold winters in a row has never been seen in the 150-year-long series on hand. Schwerdtfeger adds that the winter of 1942-43 turned out much below normal and that the failure of the prediction and the suffering caused to the troops impaired the reputation of meteorologists in the eyes of many a military leader.” But here Flohn defended his old master (Neumann and Flohn 1987, p. 622). Flohn himself was at ZWG in 1942, unlike Schwerdtfeger, who first came in September 1943. Flohn did not remember that Baur was asked again. Flohn believed that Schwerdtfeger simply remembered wrong so many years after the war and "projected" the forecast for the winter of 1941/42 to the following winter.

Baur was not “asked” by Diesing to deliver a prognosis. As already stated, he had to prepare a new prognosis every month on a routine basis. However, Schwerdtfeger was not completely wrong about Baur’s arguments. In the summary of the long-range weather forecast for the winter 1942/43, Baur wrote (Baur 1942b, p. 2) “It can be considered practically certain that the winter of 1942/43 will not be as severe in Central Europe or Eastern Europe as the winters of 1939/40 and 1941/42.” The arguments for this were, among others, related to sunspots, periodic processes, correlation analyses and to the temperatures of the preceding winters, a method Baur had already used in 1940 (Appendix A). This time, Baur’s prognosis was correct, as the winter of 1942/43 was not colder than normal, particularly compared with the previous winter.

It is remarkable how many different methods Baur used as the basis for the four prognoses for the winters of 1940/41 through 1943/44 and how much the methods changed from year to year. In total, Baur used 10 different methods. The four prognoses
were based on 6, 3, 7 and 5 methods, respectively. Three times, he used methods based on
sunspots, sun radiation, analogy, and regression analyses. Two times, he used analyses
based on winter and summer temperatures and rhythms (waves and periodicities), and one
time, he used the temperature in the autumn, the overall weather situation (Grosswetter)
and the temperature in North America. Baur was obviously trying to develop new and
better methods and seems to have had many ideas, but on the whole, the work seems
scientifically inconsistent and reveals a high degree of uncertainty. In 1972, Baur wrote
about the possibilities of making monthly and seasonal weather prognoses in the years
just after the war (Baur 1972, p. 81): “With the knowledge available at that time, this
would have resulted in too many false predictions that would have been damaging to the
reputation of the long-term weather forecast”. He truly did not like the results of his work
during the war but was forced to do it and probably did his best. However, his reputation
had already suffered irreparable damage “at that time”.

The post-war times

As Baur’s institute was part of the German Army, it closed in May 1945 and never
reopened. In 1964, Baur wrote a ‘confidential’ book meant only for his nearest family and
friends entitled “Preventing the continuation and completion of Franz Baur's life's work”
(Baur 1964). Here, Baur described his problems after the war in getting a job to continue
his research and again make 10-day prognoses. Later, he publicly wrote (Baur 1972, p.
81) that “The then head of the German weather service in the American zone, however,
did not agree with the efforts of the Hessian State Statistical Office and the Hessian
Chamber of Agriculture to attach a department for large-scale meteorology to the
Statistical State Office. The weather forecast for 10 days given on 30 August 1939
remained the last.” This head, the first leader of DWD after the war, was Professor
Ludwig Friedrich Weickmann. It is remarkable that Weickmann should have hindered
Baur in the continuation of Baur’s work, as Weickmann himself before the war had dealt
with long-range prognoses based on his work on waves and so-called mirror points, i.e.,
special days from which the air pressure would repeat itself in reverse order (Weickmann
1924). In 1934, Krick visited Weickmann in Leipzig and was very much inspired (Lewis
1994, p. 71). However, the times had changed both politically and scientifically. In the
years after the war, statistically based weather forecasting was overtaken by physically
based weather forecasts using computers as described by Kristine Harper (2008) in the book *Weather by the Numbers*.

However, Baur managed to obtain minor funding so that he could continue his research on a small scale. In 1951, he published an article in *BAMS* (Baur 1951) that lifts the veil on some of his methods. However, the presentation is not very convincing. There are many words and little factual documentation. His analyses seem like cherry picking (see Box A, from Baur 1951, p. 324). He appears to search for new relationships until seemingly striking statistical correlations emerge. With these kinds of models, introducing more relations and parameters always improves conformity with the observations (overfitting). Such models say very little about what is signal and what is noise and therefore have limited value as forecasting tools. He was aware that such criticisms could be raised and sought to counter them in the same article (Baur 1951, p. 324). He also referred to criticism of his methods in a European newspaper without mentioning the name of either the critic or the newspaper.

If in November (1) the deviation of the pressure difference, Ponta Delgada - Stykkisholm, is greater than +3,0 mb; (2) the pressure deviation \( \frac{1}{2} \) (Jacobshavn + Upernivik) is negative; (3) the deviation of average pressure, November 21 to 30 at at least two of the three stations, Angmagssalik, Stykkisholm, Thorshavn, is negative, and (4) the atmospheric pressure, November 21 to 30 on the average of these three stations, is more than 1,0 mb below normal, a mild winter (above normal) can be expected in southern New England with a high probability, except (5) when the average temperature of November is more than 5 F deg. below normal in Winnipeg, or more than 4 F deg. below normal in Chicago or Boston.

Box A. From Baur (1951, p. 324)
Right up until his death in 1977, Baur frequently produced monthly and seasonal forecasts but only if the overall meteorological situation allowed. He still used some of his old methods, especially the influence of sunspots. The forecasts were long published in addition to the weather maps published by the Meteorological and Geophysical Institute, Freie Universität Berlin. In 1959, he reviewed three of his own forecasts and observed "All these predictions have occurred" (Baur's italics) (Baur 1959, p. 541). He was not modest.

Discussion

The search for patterns to support long-term weather predictions is ongoing and will perhaps someday succeed. Sometimes, however, this search for patterns can lead to absurd claims, e.g., that summers in even years are bad and summers in odd years are good (Müller-Westermeier 2006, p. 99). Many weather warnings have been developed over time, especially in agricultural cultures. In Denmark, meteorologist Jesper Theilgaard assessed the validity of Danish warnings in the book "50 weather warnings that (perhaps) work" (Theilgaard 2006). In Germany, Gerhard Müller-Westermeier (2006) assessed some popular "Bauern Regeln", i.e., weather warnings rooted in farmers’ experiences (“farmer rules”) in his book Wetter und Klima in Deutschland. In the same book, there are also a number of so-called Baurian rules, i.e., weather warnings rooted in Franz Baur's research.

The two German words Bauer(n) and Baur are quite similar, which may have been one of the reasons why Baur wanted to distance himself strongly from amateur attempts to make weather rules: “Almost all long-term weather forecasts come from laymen and lack any reasonable basis. The authors call themselves meteorologists, but they are not. The word is not protected in Germany” (Die Zeit 1961). A few years before his death, Baur recalled his early years (Baur 1973, p. 865): “The path on which attempts have been made for half a century to create a scientific basis for reliable long-term weather forecasts was in the first years a path between Scylla and Charybdis, between the mistrust and hostility of the ossified reactionary forces on the one hand and the utopian ideas and wishes of a crowd of laymen misled by weather calendars and sensational newspaper reports on the other”. It is correct that most such layman rules and long-term weather forecasts are not convincing. However, professional efforts to compile scientifically based long-term forecasts very often also end in
failures, as was the case with many of Baur’s war prognoses. But on an overall level, Baur believed in his ideas right to the end.

However, this was not the case with most of his colleagues. Three of Baur’s most prominent opponents mentioned in this article were Schmauß (1877-1954) before the war, Weickmann (1882-1961) just after the war and Flohn (1912-1997) ten years after Baur’s death. They all three knew each other well. For example, in the early 1900s, Weickmann worked as an assistant for Schmauß (Börngen et al. 2007, p. 4), and during the war, Flohn and Weickmann both worked at ZWG (Flohn 1992, p. VI; Börngen et al. 2007, p. 14). When Philips was dismissed from ZWG, Flohn got his job on the recommendation of Schmauß (Flohn 1992, p. 13). After the war, Weickmann was the first head of DWD (Börngen et al. 2007, p. 15), where Flohn also worked and ended his career as head of DWD’s research department (Flohn 1992, p. VI). These relationships are not mentioned to suggest some kind of conspiracy against Baur. The contacts and relations between these three were a natural consequence of their dynamic professional careers as meteorologists and scientists. Baur, on the contrary, worked solely in Bad Homburg before, during and after the war. One of Baur’s few international relations (maybe even a friend) was Landsberg (1906-1985), his later necrology writer. Landsberg was a renowned and influential climatologist (Baer 1992). In his memory, AMS has awarded each year ‘The Helmut E. Landsberg Award’ to individuals or teams for exemplary contributions to the fields urban meteorology, climatology or hydrology (AMS 2022). After a symposium on long-range weather forecasting at Bad Homburg, Baur wrote (1963, p. V): “The English translation of the lecture excerpts was kindly done by Prof. Dr. Helmut Landsberg, Ph.D., Department Director at the U.S. Weather Bureau in Washington, one of my first listeners at the University of Frankfurt a. M. I would also like to take this opportunity to thank him warmly for his faithful helpfulness.” Landsberg himself wrote in the necrology (Landsberg 1978, p. 311): “In my own, not unbiased, view he brought over the years scientific respectability to a field still fighting with charlatans.” However, Landsberg was fully aware of Baur’s challenges: “Baur’s relations to his meteorological colleagues were generally strained. The German meteorological establishment essentially shunned him” (Landsberg 1978, p. 311). Flohn (1992, p. 13) was even more critical, as he referred to the psychological damage caused by Baur's riding accident during World War I: “Unfortunately, these disorders led to a pathological distrust of all employees.” Landsberg died in 1985 and could not defend his old teacher and colleague when Flohn published his criticism of Baur’s work and Baur’s person (Neumann and Flohn 1987; Flohn 1992).
Despite their criticism of Baur and his methods, all three prominent opponents, like Baur, had worked with statistical-based analyses of weather data as a basis for long-term weather forecasts, albeit to a much lesser extent than Baur. Weickmann’s activities based on waves and mirror points have already been mentioned. Schmauß (1941) and Flohn (1942) analyzed the so-called singularities or singular points, i.e., characteristic meteorological conditions that (supposedly) tend to occur on or near specific dates. Flohn even analyzed “Grosswetter” singularities (Flohn and Hess 1949). Both waves and singularities were also used by Baur in his 10-day weather forecasts (Baur 1936, p. 149-150). Still in 1962, WMO (1962, p. 9 and 11) described these methods as a part of a possible basis for long-range forecasting. However, a short time later, “Professor Baur warns not to lose valuable time in periodicity investigations and search for symmetry points” (Baur 1963, p. 10). In the U.S., the wind changed in the years after the war (Harper 2008); one of Baur’s few admirers, Krick, got in trouble and had to leave the academic world (Cervany and Holle, 2008). Krick subsequently made a lot of money by selling his prognoses, while Baur suffered in Bad Homburg (Baur, 1964).

Schmauß, Weickmann and Flohn, also dealt with organizational work and other, more physically based analyses, while Baur seems to have been nearly obsessed by only finding statistical relations as the basis for his long-range forecasts. He used many different statistics to find patterns where no patterns were, often leading to incorrect prognoses. He was bitterly aware that this was the case for many of his forecasts during the war. After the war, he never talked specifically about these failures, citing the probably long-expired promise of silence. Baur’s wish to continue his 10-day prognoses after the war was complicated by his scientific and general reputation and by the general development of weather forecasting. Today, such statistical-based forecasts for one to two weeks are unattractive and completely superseded by forecasts based on physical-based numerical models, and the interest in the spectacular methods used by Baur and Krick has totally ceased.

However, some of the Baurian rules are still popular in Germany, i.e., Baur's high-summer rule: “If the first half of June is mainly warmer than normal (temperature deviation at least 2.0 degrees Celsius), a wet high summer is likely to follow” (Müller-Westermeyer 2006, p. 101). Many North European pupils and students experienced exam periods in June with sun and heat, which was followed by a wet and cold summer holiday. Franz Baur is certainly
not forgotten in his home country, although his necrology writer Landsberg (1978, p. 311) called him "a tragic loner".

**Conclusion**

Fig. 6. Franz Baur’s signature on his infamous long-range weather forecast for the winter of 1941/42 (Baur 1941, p. 3).

In November 1941, the German meteorologist Franz Baur produced and signed (Fig. 6) a long-range forecast for the winter of 1941/42 as a part of a monthly routine. His forecast postulated that winter would not be severe in either West and Central Europe or Eastern Europe. However, he added that the probability of an average temperature above normal was greater than the probability of an average temperature below normal. Franz Baur’s arguments were based on more or less debatable assumptions about rhythms, autumn temperatures, and analogy-relations, but he did not use the argument that there had never in climatic history been three severe winters in a row as postulated by Neumann and Flohn (1987, p. 621). However, he did use similar arguments in his winter prognosis the year before and again the year after. Based on winter temperatures in Central Europe, this article documents that the probability of a severe winter is independent of whether there have been one, two or more prior severe winters, i.e., such arguments were wrong. The false argumentation is particularly notable since Baur was an experienced meteorologist and statistician, as documented in this article. However, Baur was obsessed with finding signals where only noise was present.

Franz Baur had a problematic relationship with his colleagues, most of whom simply considered his long-range forecasting dubious or even frivolous. He was not well liked among his colleagues, and as a knowledgeable but stubborn statistician and long-range weather forecaster, he could easily get in trouble. The prognosis for the winter of 1941/42 was not at all as definitive as stated by Neumann and Flohn (1987, p. 621). But only the main
statement was remembered, not the statistical reservations. It is, therefore, debatable to declare the whole prognosis wrong. The probability of a severe winter around Moscow was only as low as getting a six in a roll of a die. But it happens, and it happened in the winter of 1941/42, which destroyed Franz Baur’s reputation, a harsh and unjust assessment.

APPENDIX

The probability of a severe winter

Neumann and Flohn wrongly postulated (1987, p. 621) that Baur’s main justification for his optimistic long-range forecast for the winter of 1941/42 was that never in climatic history had more than two severe winters occurred in a row; since both 1939/40 and 1940/41 had been severe winters in Europe, Baur did not expect that the winter of 1941/42 would be severe. Schwerdtfeger also (wrongly) remembered a similar argumentation related to Baur’s prognosis for the next winter (1942/43), now based on three severe winters in a row (Neumann and Flohn 1987, p. 622). The emphasis on this particular argument in the two situations over all other arguments by Baur may be because Neumann and Flohn found this argument in particular questionable, although they did not write this explicitly. Most likely, they supposed that it was not possible to assess the temperature of a coming winter on the basis of the temperature of the previous winter or winters. In a note to their article, Neumann and Flohn only wrote (1987, p. 627): “The present authors wish to stress that the references to failures of the above-mentioned long-range forecasts, both of Baur and that of the Soviet meteorologists involved, is not to be construed as an act of criticism. The truth is that not even in 1986, i.e., 45 years after the two long-range forecasts, are long-range forecasts very successful, especially not in case of extreme anomalies as in 1941-42.”

In this appendix, the validity of drawing conclusions about the temperature of the coming winter based on the temperatures of the preceding winter(s) is investigated. To do this, some of Baur’s methods and data will be used. As background for the long-range forecast for the winter of 1940/41 in Central Europe, Baur analyzed the winter temperatures for 165 years in the period 1775-1939 based on the temperatures of Utrecht, Berlin and Vienna (Baur 1940). Baur never listed the absolute temperatures in his prognoses, only the deviations from the average. Baur defined a severe winter as a winter
in which the average temperature in the three winter months was at least 3 degrees Celsius colder than normal (i.e., the average winter temperature of all 165 winters). Baur found 12 such winters, and in no case was such a winter followed by another severe winter. The coldest following winter was 1799/1800, which was 2.8 degrees Celsius colder than normal. Baur remarked that the summer and autumn temperatures in 1799 were much colder than those in 1940 and concluded (Baur 1940, p. 2):

“This statical fact, that it has never happened in the last 165 years that a severe winter was followed by another severe winter, is to be evaluated as follows: If any other evidence is found that points to a severe winter of 1940/41, then one must be aware that of course the conclusion must not be drawn from this fact, that a severe winter could never be followed by a severe one. However, if there are no signs that make a severe winter likely, then it can be expected with a very high probability that the winter of 1940/41 will not be severe. Subject to the results of further investigations, which are discussed below, we draw the preliminary conclusion: no severe winter 1940/41.”

This analysis was followed by 5 more analyses (sunspots, analog theory, etc.) that supported Baur's final conclusion (Baur 1940, p. 4): “A severe winter of 1940/41 can be considered impossible in Central Europe”. This sounds very definitive, but Baur added: “The probability of an average mild winter is greater than that of a cold winter. However, the possibility of a longer period of frost must be expected, through which the temperature of one of the winter months can be below the normal value.”

Using Baur’s data, the probability of one severe winter was $12/165 = 0.073$. As the winter of 1939/40 had been severe, Baur expected that the winter of 1940/41 “with a very high probability” would not be severe but did not indicate the size of this probability. If he meant that $1-0.073 = 0.927$ was a very high probability, he was right. However, as he used the argument “that it has never happened in the last 165 years”, he obviously meant that a kind of dependence existed; i.e., the probability was greater than the independent probability of 0.927. If the winter temperature is assumed to be independent of the temperature of the preceding winter, the probability of two severe winters in a row is $(0.073)^2 = 0.0053$. Over 165 years, the theoretical number of two severe winters in a row was 0.0053 times 165, equal to 0.87, against the observed number of 0. That no such situation had occurred is therefore quite to be expected and says nothing definitely about a possible dependence or not between winter temperatures from year to year. The average
temperature of the following winter, 1940/41, in Central Europe was only 2.2 degrees Celsius below normal (Baur 1943, p. 2); i.e., it was not severe according to Baur’s definition. In this sense, Baur’s 1940/41 prognosis was correct.

The next time that Baur used winter temperatures from preceding years was in his 1942/43 prognosis, as described in the main text (Baur did not use analyses of winter temperatures as background for his 1941/42 prognosis). In 1942, Baur also—for the first time—used winter temperatures from the Soviet Union: “Petersburg was chosen as the representative for Russia, as Moscow’s observations do not go back far enough” (Baur 1942b, p. 8). These (relatively few) data are not considered further here. The main conclusion for Central Europe and Eastern Europe for the winter of 1942/43 was a milder winter than the two severe winters in 1939/40 and 1941/42. Again, Baur was correct, as the average temperature in Central Europe in the winter of 1942/43 was 1.6 degrees Celsius above normal, versus 4.8 and 3.4 degrees Celsius below normal in 1939/40 and 1941/42, respectively. One of seven arguments for this (Baur 1942b, p. 4 and 5) was that after two consecutive severe winters over a period of 170 years, the following winter was never severe. Here, Baur defined a severe winter as a winter with an average temperature at least 1 degree Celsius colder than normal. Over 170 years, Baur found 8 such incidents, corresponding to a probability of 0.047. If the probabilities were independent, the probability of one winter with an average temperature 1 degree Celsius below normal was the square root of 0.047 or 0.22, and the probability of three cold winters in a row was 0.010. In 170 years, the expected number of three in a row was 1.7 against zero observations according to Baur. Again, that no such situation had occurred is likely and says nothing definitely about a possible dependence or not between the winter temperatures from year to year.

Why then did the critical story “never in climatic history did more than two severe winters occur in a row”, told by Neumann and Flohn, arise? To investigate this further, it is necessary to analyze the entire time series of winter temperatures for “threshold temperatures” other than -1 and -3 degrees Celsius, as used by Baur. It has not been possible to find Baur’s entire original time series based on the three cities mentioned above. However, in 1975, two years before he died, Baur published a similar time series (Baur 1975) of the monthly temperature deviations in these three cities plus Basel, based on the average temperature in the period 1761-1970. The winter temperature deviations...
found as the mean of the temperature deviations in December, January and February from these four cities are compared with 97 winter temperature deviations from the three cities extracted from Baur’s war prognoses, see Fig. A1. The average of the 165 winter temperature deviations from the four cities in the period 1775 – 1939 was 0.02 degrees Celsius, while the average of the 97 winter temperature deviations from the three cities in the same period was – 0.16 degrees Celsius, i.e., only a difference of 0.18 degrees Celsius, showing that it is reasonable to use the time series from (Baur, 1975) as a proxy for the time series used by Baur during the war.

Fig. A1. Winter temperature deviations in Central Europe 1762–1975. Example: 1941 corresponds to December 1940, January 1941, and February 1941. The temperature deviations from the four cities, Utrecht, Berlin, Vienna and Basel, are based on the data from (Baur 1975), while the 97 temperature deviations from the three cities, Utrecht, Berlin and Vienna, are from (Baur 1940, p. 6 and 7, 1942b p. 5 and 8, and 1943, p. 2 and 6).

With an average temperature deviation of 0.02 degrees Celsius, the "threshold temperature" corresponding to Baur’s definition of a severe winter is 0.02 – 3.0 = -2.98 degrees Celsius. Ten winters were below this value, and two winters were close to this value. Two rather cold
winters occurred in a row only once, namely, 1799 and 1800, there were never three cold winters in a row, consistent with Baur’s observations.

Baur argued (see above) that if only winter temperatures were considered, then the absence of observations of two consecutive severe winters would lead to a “very high probability that the winter of 1940/41 would not be severe”. To investigate whether this argument holds, the probability of two or three consecutive cold winters is analyzed depending on the choice of the threshold temperature (see Fig. A2).

Here, P(S) is the independent probability of a winter colder than the threshold temperature (red graph), P(S/S) is the probability of a winter colder than the threshold temperature given that the previous winter was also colder than the threshold temperature (blue graph), and P(S/SS) is the probability of a winter colder than the threshold temperature given that the previous two winters were colder than the threshold temperature (yellow graph).

Fig. A2. Probability of a cold winter in Central Europe based on the 165 winters between 1775 and 1939.
If the winter temperature is independent of the winter temperatures in the preceding winters, all three graphs should superpose. \( P(S) \) and \( P(S/S) \) are superposable to approximately \(-2.5\) degrees Celsius, while \( P(S/SS) \) is superposable only to approximately zero degrees Celsius. At lower threshold temperatures, \( P(S/SS) \) lies below the other two graphs. This could indicate that the likelihood of a very cold winter decreases if the previous two winters have also been very cold. If Baur’s data are used for the next two winters, similar curves are observed. However, the limited number of observations in the case of low threshold temperatures gives rise to major uncertainties. This is clear from Fig. A3, where the observation series is extended with data from the following three winters: 1940, 1941 and 1942, i.e., the three consecutive very cold war winters. The red and blue graphs in Fig. A3 are virtually identical to the corresponding graphs in Fig. A2. In contrast, the yellow graph in Fig. A3 clearly differs from the yellow graph in Fig. A2 for threshold temperatures below approximately \(-1\) degree Celsius. Now, in Fig. A3, both the blue and yellow graphs lie more or less randomly above and below the red graph, which supports the assumption of independence applicable to all threshold temperatures. Therefore, as an example, the steep drop in the yellow graph at approximately \(-1\) degree Celsius is not a sign of a lower probability of a severe winter after two severe winters (Baur’s argument), and the peak at approximately \(-2\) degrees Celsius is not a sign of a higher probability of a severe winter after two severe winters. The best estimate is obtained using the red graph for all cases.
Fig. A3. Probability of a cold winter in Central Europe based on the 168 winters between 1775 and 1942, i.e., an expansion of Fig. A2 with three more winters.

If Baur’s data up to 1975 are used, the graphs are almost the same as those in Fig. A3. Therefore, was the winter of 1941/42 a gamechanger? No; instead, the temperature from this winter merely underpins the assumption of independence. This assumption is therefore further supported by data up to 1975. It may be concluded that the probability of a severe winter in Central Europe is independent of whether there have been one, two or more prior severe winters.

In 1928, Baur published a textbook on correlation calculations (Baur 1928a). Section 6 in this book addresses conditional probabilities, i.e., probabilities such as P(S/S) and P(S/SS), as used in this appendix. Baur thus had a thorough knowledge of conditional probabilities. However, he does not seem to have used this knowledge and method during the war, which may be due to a lack of manual computer power. It is therefore possible that before the winter of 1941/42, he could have seriously claimed that because there had been two severe winters in a row, the next winter would not be severe. He was perhaps so dazzled by looking for relations that he completely overlooked the simple fact of statistical independence.
Data Availability Statement.

Datasets regarding the morning temperatures at Moscow in 1941 are included in Table 1 in Neumann and Flohn (1987).

Datasets regarding monthly temperatures in Central Europe based on the measurements from De Bilt (The Netherlands), Berlin Potsdam (Germany), Wien (Austria), and Basel (Switzerland) are from “Beilage zur Berliner Wetterkarte des Instituts für Meteorologie, Zentraleinrichtung 2 der Freien Universität Berlin. 76/75 SO 19/75. Tabelle 1, 24.6.1975. Abweichungen der Monatsmittel der Temperatur Mitteleuropas vom 210jährigen Mittelwert (1761…1970) in °C, von Franz Baur.“

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