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Reply to review

by Tomasz Domański *Correcting the Picture? Some Reflections on the Use of Sources in Night without End: The Fate of Jews in Selected Counties of Occupied Poland, volumes 1-2* (*Korekta obrazu? Refleksje źródłoznawcze wokół książki „Dalej jest noc. Losy Żydów w wybranych powiatach okupowanej Polski”*) vol. 1-2, eds Barbara Engelking, Jan Grabowski, Warsaw 2018
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In *Night Without End* I prepared the chapter about the fate of Jews in Złoczów county. Our intention was for the research framework to include one of the eastern pre-war Polish counties which were located in District Galicia (*Distrikt Galizien*) during the German occupation, and had been part of the General Government since August 1941. We were looking for a multicultural area, diverse in social, economic, and religious terms, which had been fraught with various ethnic problems even before the war. The concept developed by our research team involved analyzing those relations even before the outbreak of World War II, as the interwar perspective seemed very important in the context of the events which happened later, during the war and the occupation. It was also our objective to verify how the German occupation system functioned in several selected areas. The reviewer should realize that making such decisions is the autonomous prerogative of every scholar and research team.

Złoczów county, in its pre-war borders, fulfilled the preliminary criteria for the selection of research territory. Aside from Poles and Ukrainians, who were the two largest ethnic groups, it was also inhabited by Jews, who lived in average-sized communities in towns or much smaller ones in the countryside. In the Tarnopol Province, where it was located, it was one of the counties with the largest total population, including Jewish population. It had a provincial character and was an ethnic mosaic with a majority rural population (with a very large percentage of Ukrainians). Though Złoczów county had no major city, there were several smaller towns. During the German occupation an open ghetto was established there and became its distinctive feature. This was the basic premise for my choice of this county — the interesting local micro-world as a kind of a research sample, a microstudy of social relations during the occupation.

The reviewer fails to notice any of those aspects and consequently, he formulates an ungrounded accusation that the research areas proposed in our book are “freely selected by the individual authors, without adhering to the principle of uniformity and factuality” (Domański, p. 5). My analysis of Domański’s review suggests that he knows little about my research area and that he is simply unfamiliar with the sources, which is why he finds

no argument justifying that choice. “[D]espite the names of the individual ‘counties’ in the titles of the individual parts, there is total randomness [as far as the research territory selection is concerned]. This is concealed under the pretense of uniform terminology, [of which] the reader should be dutifully informed,” writes Domański about my study (ibid.). This accusation should be regarded as completely ungrounded, as our editors emphasize as early as the introduction that not all of the studies discuss research on *Kreishauptmannschaften*, that is, the German counties (Engelking, Grabowski, “Introduction”, vol. 1, p. 14). We, the authors, discuss this in our chapters and also include maps of the research area. A freely made selection of research territory is the prerogative of every scholar, on which depends freedom of academic research - but the reviewer seems to have forgotten about that.

In line with the premises presented earlier in my chapter, the terrain I carried out research on is clearly marked against the background of Kreis Zloczow (*Night Without End*, vol. 1, maps after pp. 625 and 672). Moreover, in the main text of my chapter, I clearly state that my research covered the area of pre-war Złoczów county (Zapalec, vol. 1, p. 669). Apparently, Domański did not read this carefully, nor did he devote much time to analyzing the maps, since he formulated the following accusation: “The object of Zapalec’s dissertation is only the middle part of Złoczów ‘county’ (Kreishauptmannschaft Zloczow) and not ‘Złoczów county’, which did not exist on the map of the General Government” (p. 6). As a matter of fact, during the occupation the middle part of Kreis Zloczow perfectly matched pre-war Złoczów county. The reviewer again proved that he failed to understand my premises and disregarded the pre-war administrative divisions in Eastern Galicia and the changes that occurred during the Soviet and German occupations. Consequently, I consider his statement that I misled the readers with my selection of the research territory, because of my purported failure to specify it sufficiently clearly, to be unfounded. One might not agree with my choice and I do admit that researching a larger area would contribute to examining a larger sample of the reality of the occupation period, but I cannot be accused of intentionally misleading the readers. In our book, all authors, including myself, did specify and define their research area. And it was our individual prerogative. For our book is not a monograph or synthesis, but rather a collection of micro-historical analyses, which do not aspire to a comprehensive depiction of the Holocaust in occupied Poland. I also have the impression that the reviewer understood neither the guiding principles of our research project nor our publication’s objectives and construction.

For the reasons delineated above, I conducted research within the borders of pre-war Złoczów county without coopting any other pre-war counties to cover the area of the entire *Kreishauptmannschaft*. This decision was influenced by both objective and source-related considerations as well as the fact that during World War II, the eastern counties underwent administrative changes at least twice. In my analysis I deemed it important to present certain occupation period phenomena and contrast them with the pre-war situation, for instance, the population changes, which is an important part of the dynamic of historical processes. Moreover, in the case of microhistorical research, *Kreishauptmannschaften* on the eastern terrains are too large for a scholar to access the

lowest level of the local communities (individual villages and small towns). They were subject not only to administrative changes, but also migration during the Soviet and German occupations. The methodology of microhistory involves the use of the same research methods for areas differing in size.

Another reservation voiced by Domański was that the “counties’ selected do not constitute a thought-out exemplification of the entire Polish territory” (p. 7). According to Domański’s interpretation, the authors and editors failed to select a research territory that met the expectations of a “thought-out geographical variety and a selection representative of entire Poland” (ibid.). While this notion appears plausible, it cannot be achieved, since nine authors cannot carry out research on the vast Polish territory under two occupations over the course of a few years. Moreover, choosing any other group of counties would not have ensured a complete representation, either. This could be achieved only if research were carried out on all counties. If the reviewer thinks that a “thought-out exemplification” ensuring a complete representation is possible, then he should share his ideas with the readers. I would like to read what he has to say about this. To reiterate, the only solution I can see is the continuation of research on other regions/counties, which can bring us closer to learning about the local conditions during the occupation in individual parts of Poland. Only then will it be possible to verify the findings and paint a comparative picture in the form of a monograph. This is bound to take long, but it is an important research endeavor for the future and it was rightly suggested by Jacek Chrobaczyński in his review of our book (“Osaczeni, samotni, bezbronni... Refleksje po lekturze książki *Dalej jest noc. Losy Żydów w wybranych powiatach okupowanej Polski*,” *Res Gestae. Czasopismo Historyczne* 6 [2018], p. 272). By contrast, Domański was unable to make any creative or constructive contribution to the discussion. Instead, he carelessly criticized the authors. He must have forgotten that a critical review’s value lies not only in its critical remarks, but also in its propositions resulting from the reviewer’s own research. Only then can the academic discourse begin. This is the foundation of debates, even the most difficult ones. After reading this review, I have the impression that in his critique Tomasz Domański is not using an equal discourse, but is rather trying to prove that he would have written this book better. Moreover, he is apparently convinced that, as he is in possession of the truth, there is no need for publications penned by other scholars representing other perspectives. Such an approach to academic research is incorrect from methodological point view, also when it comes to reviews. Of course, Domański is free to write such a model book and present the results of his research and to confront them with our findings.

I cannot agree with the accusations concerning my chapter. Their groundlessness becomes evident when one reads and understands the introduction and then studies my chapter carefully, including the footnotes, of course. When historians write and publish a text they obviously assume that the reader will read the footnotes, which explain and supplement the main text, constituting an inseparable, and sometimes very extensive, part. Domański ignores this and criticizes me for placing comments and information concerning negative opinions of the Złoczów Judenrat in one of the footnotes. In Domański’s opinion I should have included this in the main text, for only then can it “give

the readers a truly fuller picture and enable them to familiarize themselves with those different opinions” (p. 62). He even suggested that I was guilty of manipulation, because he disagreed with my overall positive assessment of the Żłoczów Judenrat (pp. 61–62). I do not consent to such an approach. Domański seems to have forgotten the principles of writing academic texts. He is also trying to undermine my autonomy to construct an authorial narration. I will return to the subject of the evaluation of the Żłoczów Judenrat later in my response.

In Domański’s opinion one of my first shortcomings is that I utilized neither the collection of memoirs stored in the Ossolineum in Wrocław nor the collection of testimonies of Poles deported and exiled into the interior of the USSR during World War II, which is stored at Stanford University’s Hoover Institution on War, Revolution and Peace in Palo Alto. It is completely unprofessional for a reviewer to indicate unused archival materials which he has likely neither seen nor read himself. Domański simply reiterated this accusation from a review which is to be published, written by other Institute of National Remembrance employees, this time from Wrocław (p. 27). It is difficult for me to respond to this accusation, as Domański did not list any specific sources or threads he regards as crucial.

First of all, I wish to emphasize that practically the entire review diverts from the objectives defined in our book as well as from its title and principles. Domański yearns for a monograph on the reality of the occupation period which elaborates on the social stances of all the various social groups and nationalities within Polish territory under the German and Soviet occupations, with the Holocaust discussed somewhere against this background. The reviewer apparently does not realize that a synthesis of the social history of Poland during World War II has to be preceded with studies based on research into specific issues, including microhistorical research, and broad discussions about the individual issues (as emphasized by Jacek Chrobaczyński in his review of *Night Without an End* [“Osaczeni, samotni, bezbronni...,” pp. 272–273]). We will have to wait a long time for such a monograph, and if the debate is to be conducted in the manner proposed by the reviewer in his text, then this development seems very distant indeed.

Tomasz Domański reduced our book mainly to an analysis of Polish attitudes towards Jews during the occupation, and this is what he focused on. But *Night Without End* has a much wider thematic spectrum, and the issue defined as Polish attitudes and behaviors towards Jews is just one of the specific issues addressed in our research. In our two volumes we describe many other occupation-period phenomena, to which the reviewer paid no attention whatsoever. Was this an oversight or an intentional omission? For instance, the chapter on Żłoczów county provides knowledge about the local occupation structures, describes the course of the individual liquidation actions, reveals the perpetrators’ stances, presents a range of the German repressions against the Jews and the various forms of violence, shows the fate of Jewish families and the individual experiences of their members, lays great stress on Jewish survival strategies, and also explains many other aspects of the occupation, including, of course, the attitude of the local population towards its Jewish neighbors. However, in his assessment of my text, the reviewer extracts only the issues which fit his preconceived critical thesis and omits those

which constitute the essence of the entire study. It is easy to see that my quite extensive comments on the occupation administration and the German liquidation actions did not interest him, and that he simply omitted them. Instead, he focused on the local population's attitude towards the Jews and presenting the negative attitudes of certain Złoczów Jews in the ghetto and labor camps, for instance, collaboration with the occupier and denunciations (pp. 66–69). Even though the reviewer did notice (p. 57) my conclusion that in Złoczów county certain Jews were involved in collaboration with the German occupier (Zapalec, vol. 1 p. 737) he decided to ignore it and formulate the following accusation, simply because he thought that instances of Jewish collaboration should have been described in more detail: “Trimming sources to fit a presupposed thesis, paired with concealment and subjective selection of the material, leads to creating new mythologies” (p. 60). Domański demands and wishes to see examples of such stances even in passages where I discuss something completely different. Let us consider, for instance, my discussion concerning the construction of the bunker of the Strassler family from Złoczów and how that was managed, for during the occupation such an enterprise was an extraordinary effort for Jews and it involved preparations for going underground for a period of several months (Zapalec, vol. 1, pp. 716–717). Here the reviewer expected me to include a detailed profile of Lonek Zwerdling, who was hiding there too (a trusted man of Obersturmführer SS Friedrich Warzok - the commander of labor camps in Kreis Zloczow), and explain the circumstances of the killing of one of the Jews who were staying in that bunker (pp. 68–69). Such a description was missing not because I wished to avoid that topic, but because I did not describe any of the individuals living in that bunker in detail, nor did I analyze the particulars of the conditions in which that group lived underground. Instead, I laid stress on the planning and construction of that shelter, as that was of importance for presenting the crucial factor of that survival strategy. This is yet another example of the reviewer's outlook, which ignores the narrative's context and the reasons for why certain examples are given.

The reviewer regards my description of the Jewish institutions in the ghetto as insufficiently critical. Consequently, Domański formulates the following accusation: “Familiar with the testimony given by Maria Cukier, a Jewish woman, Zapalec uses it so as to conceal from the reader the opinions unfavorable to the Judenrat” (p. 62). Let me point out that the fragment of Maria Cukier's testimony was quoted to document the positive stances of some of the Polish intelligentsia from Złoczów towards the provision of help to Jews, as that quotation referred to the essence of said issue. Besides, that citation should have been read and evaluated in the context of the remaining information provided in the text (Zapalec, vol. 1, p. 741). Removing it from its context simply distorted the intentions with which I used it. This is puzzling, as Domański accused us of precisely that — ignoring the context. I wish to stress that both this fragment of the source and the other quotations included on that page were included to document the positive stances of the Polish intelligentsia in Złoczów towards helping Jews and not to discuss or assess the operation of the Judenrat, which I did much earlier (Zapalec, vol. 1, pp. 681–682).

The reviewer disagrees with my overall positive evaluation of the Złoczów Judenrat, wishing that I had discussed more negative opinions about that Jewish ghetto institution.

His arguments are weak however, because they are based on two opinions voiced by individual witnesses (Domański, pp. 61, 63). Nevertheless, Domański goes as far as insinuating the following: “Anna Zapalec, who made a single testimony a summary of the local Judenrat’s activities, quoted — as a general conclusion — the following words of Helena Kitaj-Drobner: ‘The Złoczów Judenrat was famous in the region for taking good care of its people. The Ordnungsdienst commanded less respect’ (Zapalec, vol. 1, p. 681). Nevertheless, in the footnote to this quotation Zapalec concludes that there was certain controversy about the functioning of the Złoczów Judenrat resulting from the occupation period circumstances and the carrying out of German ordinances, and also ‘more critical [opinions] or even ones completely opposite to the one quoted in the text’ (Zapalec, vol. 1, p. 682). It is a pity that she does not follow this path in the main text to give her readers a truly fuller picture and an opportunity to learn about those different opinions. Even that might have wrecked the dominant narration” (pp. 61–62).

This manner of discrediting the content of my chapter is characteristic of Domański’s review. He ignored all other positive opinions about the Złoczów Judenrat quoted in the text as well as the objective premises stemming from an analysis of the operation of the Judenrat, which gave me a right to the overall positive conclusion (see Zapalec, pp. 681–682 ff). Besides, Helena Kitaj-Drobner’s opinion finds substantiation in facts and other testimonies, which I pointed to while discussing various endeavors made by the Judenrat. Despite several negative opinions about its activities (which I indicated in footnote 161 on pp. 681–682) I am still convinced (which I also emphasized in the main text) that it should be regarded as overall helpful to the ghetto residents. At many difficult moments and during tragic events aimed against Złoczów Jews, the Judenrat acted contrary to the German occupier’s expectations. I was convinced of this not only by the positive opinions of witnesses who had direct contact with the Judenrat when they were settling their affairs, but also by that institution’s actions described by eyewitnesses: contrary to the SS’ expectations, the Judenrat did not compile a list of Jews to be deported from the Złoczów ghetto during the first deportation and it did not conceal information about the deportation from other people. Expecting the second deportation, it organized guards who were to inform about the impending danger. It also interceded on behalf of the victims and organized aid to labor camp prisoners. Within the framework of its activities, the Judenrat conducted a campaign of social self-help with great determination and — as reported by a Jewish survivor — the councilors refused to grant their consent to the liquidation of the ghetto in Złoczów even when they were threatened with death. One can read about all this and learn where this information comes from in my chapter. Unfortunately, the reviewer remained unimpressed and unconvinced, for he deeply believes that we intentionally concealed fragments of sources and quotations (p. 69). He also senses a conspiracy among us; we purportedly wrote to prove one thesis, a belief which he expressed in various fragments of the review (pp. 19, 24, 69, 72). This reasoning and approach prevailed in his reading and assessment of our book. Moreover, with regard to Złoczów county, he wished for the analysis of the provision of help to Jews to reach beyond not only the research territory, that is, prewar Złoczów county, but even beyond the territory of Kreis Zloczow. For example, he criticised my failure to present the figure

of Father Jan Pawlicki from Zborów, who helped Maria Cukier. There was nothing to suggest that Father Pawlicki did his pastoral service in Złoczów county. Before the war, the Roman Catholic parish in Zborów was located outside Złoczów county. During World War II, Zborów was not a part of Kreis Zloczow either. Consequently, that locality was outside my research territory, which means that I did not have to analyze the activity of Father Pawlicki from Zborów. Hence, the reviewer should not have insinuated that Father Pawlicki “was apparently insufficiently interesting to the author” (p. 62). By adopting such a manner of analysis, he might as well have postulated that I present all priests who aided Jews from the terrains bordering on Złoczów county, or perhaps from more distant areas too. If Tomasz Domański had conducted more microhistorical research, he probably would have a better understanding of the limitations and difficulties encountered by authors of such studies.

The reviewer also provided several examples of Jewish collaboration with the German occupier in Złoczów county, on the basis of which he formulated a generalized accusation that I intentionally omitted those issues (pp. 66–68). However, the instances mentioned by the reviewer are only additional examples and an elaboration of my conclusion that such stances did exist in Złoczów county, which I summarized on p. 737 of my chapter (examples of primary sources documenting my conclusion are listed in a footnote). The statement that my “special predilection for avoiding personalization of the negative stances of Jews” (p. 57) unjustly suggests that I intentionally approached those issues differently than the negative stances of other national groups. What if one reversed the reviewer’s thesis and inquired about the reviewer’s predilection? Is it not the case that the instances of Jewish collaboration he so painstakingly provides, without analyzing or understanding them properly, testify to his special aptness to emphasize such phenomena as well as to his lack of comprehension of the circumstances of the occupation period? This is not what reliable criticism is about, which the reviewer understands poorly. Generally speaking, I did not personalize the negative stances of Ukrainians and Poles either, with a few exceptions. Detailed information can be found in the primary sources I discovered and which I gained access to during my search queries and listed in the footnotes and in the bibliography. Domański, for one, had no problem using them.

Generally speaking, the fragment of the review about Złoczów county is a series of the author’s wishes for more details. However, meeting this demand would not have brought me to different conclusions. It would only have resulted in the addition of several more examples supporting the conclusions formulated.

Here I wish to discuss one more issue which seems important: the formulation of general evaluations of the attitudes of Jews, Poles, Ukrainians, or representatives of other national groups during the German occupation, which, by the way, will be subject to further research and discussions. Domański expects a similar approach to evaluations of the negative attitudes of Poles and Jews during the occupation. He expresses this postulate in the following way: “The description of the pathology and servility of certain groups towards the Germans should encompass both Polish and Jewish stances as this is discussed in witness testimonies. We shall then get a chance to see a depiction of the complicated fates of people instead of the distorting mirror which is selectively focused

on a negative depiction of one group but ignores similar stances exhibited by other groups” (p. 20). This must be why Domański so carefully listed the several examples of reprehensible Jewish stances in Złoczów county about which he read in the sources. Even though in my chapter I included information about various negative actions of the Judenrat (Zapalec, vol. 1, pp. 681–682, footnote 161; *ibid.*, pp. 688, 690), the brutality of OD-men in the camp in Lackie (*ibid.*, p. 678), informers in labor camps (conclusion in the summary: *ibid.*, p. 713), and instances of informing or denunciation among Jews (as a conclusion drawn from research: *ibid.*, p. 737), the reviewer still criticizes me for being too general, for instance, in my discussion of OD-men’s stances (p. 57), and, as far as the Judenrat is concerned, of intentionally concealing unfavorable opinions of it (p. 62; I responded to that accusation earlier). In the review, Domański lists various negative actions of Jews during the occupation, including ones from Złoczów county, which we purportedly did not want to discuss. As for the description of the operation of Jewish institutions in the ghetto, for instance, of the Jewish police (German: Jüdischer Ordnungsdienst — Jewish Order Service) he postulates: “a historian should aim at nuancing the individual histories and showing various stances and degrees of involvement in serving the Germans — just like in the case of the blue policemen” (p. 56). He is also trying to shed light on the negative side of the Judenrat’s activities. This also illuminated how the reviewer understands the conditions of the occupation system, to comprehend which the victims’ perspective is key. During the occupation in Poland, people’s actions were influenced mainly by two determinants: life and death. That was what the choices made by the different actors of the occupation period drama, including Poles and Jews, depended on. Here I must emphasize that Domański does not mention either Ukrainian attitudes towards Jews in Złoczów county or the role and importance of the Ukrainian police (German: Ukrainische Hilfspolizei — Ukrainian Auxiliary Police) in the occupation system. Ukrainian affairs and the attitudes of Ukrainians towards Jews are practically absent from the reviewer’s sphere of interest, and when he does discuss them then it is a marginal part of his discussion of other matters. Consequently, it remains unknown how he perceives the role of the Ukrainian police in Złoczów county, where there was no ‘blue’ police (Ger. Polnische Polizei im Generalgouvernement — Polish Police of the General Government). He should have mentioned that in his analysis of the situation in Złoczów county, since those issues are presented in the chapter.

The reviewer rightly suggests an individual approach to matters as difficult as the involvement of some members of the Jewish community in collaboration with the German occupier. At the same time, he does not notice that it is often impossible to compare the motifs or the level of agency of the individual members of the Polish and Jewish communities who collaborated with the occupier. The situation of Jews during the occupation differed from that of Poles in many respects, as defined by the objectives of the occupier’s policy towards those two national groups, which differed with regard to Jews and Poles. When discussing Złoczów county, one should also mention that the objectives of German policy had yet a different character with respect to Ukrainians, whose collaboration and involvement in the German occupation system were relatively substantial and assumed forms which never manifested on the Polish side (for instance,

the support for the Germans' marching into Eastern Galicia, the establishment of the local Ukrainian administration under German auspices, and recruitment to SS-Volunteer Division 'Galicia'). Violence and terror were aimed at all people on the occupied territory, but not to the same degree and not at the same time. It should be emphasized that Poles suffered tremendous population and economic losses. However, when it comes to the German occupation policy towards Jews, the assessment of the chances of survival, and the scale and form of violence, then the situation of the Jewish population against the background of other nationalities was the most difficult and dramatic, basically devoid of chances of survival. I see no need here to list all forms of violence against Jews to illustrate that. Let me stress that in the ghettos thousands of Jews died as a result of the policy of indirect extermination and those who were deported from there ended up in the extermination camps where they were murdered right away. The Jews had slim chances of survival in concentration camps, as they were killed immediately or shortly after being transported there. Thousands of Jews were executed on the edge of mass graves. The extermination was conducted on all members of Jewish society regardless of age or sex; all Jews were subject to annihilation. The hunt for Jews in hiding consisted not only in their pursuit, but also in searching houses, basements, attics, and barns, conducting manhunts in forests, and looking for and unearthing bunkers hidden deep in the ground, tracking down every Jew, including children, who managed to hide. Of course, the negative attitudes of Jews towards their compatriots should also be written about. However, I think that one cannot suggest that the negative behaviors or violence of the Polish blue policemen or Ukrainian policemen should be nuanced with similar Jewish ones, not to mention equating the former with the latter. The form of the collaboration with the occupier was often the same but the motifs, reasons, significance, and context were usually different. Such instances should be examined in detail because only then can we see the different motifs, stances, and circumstances. Domański thinks that there is too little of a negative portrayal of the ghetto communities in *Night Without an End*, and that consequently, the negative attitudes of Poles are exaggerated and unfairly presented (p. 56). However, I cannot agree with this because of the reasons detailed above. Generally speaking, the issue of Jewish collaboration with the German occupier is a difficult research area, also in legal terms. Literature on the post-war trials of Jewish collaborators which took place in Israel addresses the many moral dilemmas accompanying their evaluation and a relatively large group of the acquitted (see Paweł Machcewicz, "Żydowskie rozliczenia po II wojnie światowej: od „mścicieli” do procesu Eichmanna", in *Rozliczanie totalitarnej przeszłości: zbrodnie, kolaboracja i symbole przeszłości*, ed. Andrzej Paczkowski [Warsaw, 2017], pp. 18–23).

In the reviewer's opinion, even unconfirmed instances of Jewish collaboration should have been included in the chapter. Consequently, he formulated the following critique: "Last but not least, we shall not learn who prevented the successful escape of the second group from Złoczów. The author only stated that 'they were denounced and arrested in May 1943.' Twelve of them were then murdered by the Germans (p. 712). According to Jakub Chamaides's testimony, the conspirators were denounced by a Jewish commander

of the workshops, a certain S., while S. Strassler claims that informer W. hid among the conspirators [...]” (p. 67–68).

In the footnote, the reviewer added: “E. Halpern indicates yet a different person responsible for the revelation of the preparations for the escape. It was purportedly Odoman S.” (p. 68). Let me mention that the reviewer’s comments refer to the preparations for the escape of Hillel Safran’s group and other Jewish engineers who worked in the camp workshops in Złoczów in the spring of 1943. The escape failed because they were denounced, which I indicated in my chapter in the following way: “But in May 1943 they were denounced and arrested by the Criminal Police” (Zapalec, vol. 1, pp. 711–712). I found several pieces of information about that in the sources, but they were partly contradictory, which the reviewer noticed, because he also had trouble determining who denounced that group of conspirators. People said different things about their arrest. None of the witnesses quoted by Domański saw the arrest with their own eyes. Furthermore, they pointed to different denouncers. The reviewer apparently got lost in this, because he erroneously attributed information provided by E. Halpern to that escape, although it referred to a completely different instance. If anything can be established about the person who denounced that group of Jewish conspirators, then Jakub Chamaides is probably right. The surname he provided was also mentioned in one of the German sources, which the reviewer did not find (let me specify it for clarity: AYV, O.5/61, Protokół przesłuchania Ernsta Stella z 23 V 1947 r. [record of Ernst Stella’s interrogation of 23 May 1947], p. 17). Due to the different pieces of information presented in the sources and the lack of any further specific information about the perpetrator and the motives of his behavior, I decided not to point to a specific person. The reviewer also stated that twelve people were executed, but this number seems incorrect to me (the reviewer did not read the appropriate fragment of the text carefully enough). The number provided by Benjamin Hochberg, who witnessed the execution, is more probable. He claims that six people were shot dead (AYV, O.5/61, Odpis zeznania Benjamina Hochberga z 23 V 1947 r. [duplicate of the record of Benjamin Hochberg’s testimony of 23 May 1947], p. 56). This approach is another indicator of the reviewer’s technique and research attitude. All sources concerning preparations for this escape and the execution of the Jewish engineers which I found (of which the reviewer used only some) are listed in the footnote to the fragment of the text I am referring to.

It is highly doubtful whether Domański read my chapter carefully. One of his reservations concerns my alleged insufficient documentation of the negative role played in the campaigns against the Jews by the Złoczów Criminal Police (Kriminalpolizei, Kripo), whose functionaries also included Poles. This is how the reviewer formulated his accusation: “it is at least surprising that the author feels authorized to formulate definite judgments even though she listed not a single specific instance of a negative stance of Kripo functionaries from Złoczów of a nationality other than the German one” (p. 31). Domański seems to have forgotten that earlier in my chapter I wrote that one of the Polish witnesses who testified at the Opole branch office of the Commission for Persecution of Crimes against the Polish Nation stated, while discussing the underground activity of the Złoczów Inspectorate of the Home Army (*Armia Krajowa*, AK), that 90 percent of the Kripo

functionaries were Polish (Zapalec, vol. 1, p. 667, footnote 102). The witness phrased this in his testimony in the following way: “The commander of the Złoczów Inspectorate was Lieutenant Mieczysław Lipa, codename ‘Wichura’ — he is dead now. His task was to infiltrate the Kripo, that is, the German Police, 90 percent of whose functionaries were Polish, and to sanction firearm possession by individual villages by establishing an industrial guard of the brown coal mine in Kozaki” (quoted after: Institute of National Remembrance, Cracow Commission for Prosecution of Crimes Against the Polish Nation, S 47/13/Zn, vol. 1, Protokół przesłuchania świadka Juliana W. z 9 V 2001 r. [record of interrogation of witness Julian W. on 9 May 2001], p. 26). This is not the only source quoted in my text which informs that the Złoczów Kripo had a large percentage of Poles (see Zapalec, vol. 1, p. 667). If the reviewer was truly familiar with the sources from the Ossolineum in Wrocław, the use of which he suggests, he would know that during the occupation the Złoczów Kripo was commonly referred to as the ‘Polish police’ (see the Ossolineum in Wrocław, manuscript 16718/II, Inspektorat Złoczów AK [Złoczów AK Inspectorate], Meldunek sytuacyjny nr 10 z 26 III 1944 r. [situational report no. 10 of 26 March 1944], p. 66). Information about the participation of the Kripo functionaries in campaigns against Jews in Złoczów was presented and documented in my chapter on pages 696, 707, 712, and 721. Despite this, the reviewer formulated the following accusation against my text: “The only thing she found was information about the activity of German (!) Otto Zikmund” (Domański, p. 31). As for Zikmund, he came from Austria. The evidence presented and referred to in my chapter (including sources listed in the footnotes) discusses not only the Kripo commander’s participation in repressions against the Jews, but also the negative role of his subordinates in those repressions and their participation in various campaigns against the Jewish population. The participation of Kripo functionaries in various similar anti-Jewish actions was reported in other territories of occupied Poland, and it has been relatively well proven in publications on this topic. Consequently, Domański’s accusation that I formulate “hard theses *a priori* and present absolutely no results of primary source research” (p. 31) must be deemed absurd.

None of the issues Domański discusses or the accusations he formulates against my chapter change either the conclusions I reached based on my research or introduce any new ones. Each chapter of *Night Without End* says a lot about the occupation apparatus, the individuals it constituted of, war criminals, and also provides previously unknown data about the reality of the occupation period. Yet the reviewer has little appreciation for this. He does not mention Ukrainian attitudes at all, and he even goes as far as stating that “one will find little information here [in *Night without an End*] about Belarussians and Ukrainians because of the authors’ [limited] interest sphere” (24). This makes me think that Domański did not read my chapter carefully, for I provided a number of such examples, for instance, in reference to the July 1941 pogrom of Jews in Złoczów (Zapalec, vol. 1, pp. 654–659) and also later in that chapter (*ibid.*, pp. 662–664, 678, 682ff). While formulating his remarks regarding Złoczów county, Domański seems to have forgotten that this terrain had a very large Ukrainian population and that in my chapter I presented various negative attitudes towards the Jews and acts of violence against them, as well as murders committed by some of the local Ukrainians. The reviewer also insufficiently

stresses that the text about Złoczów county discusses the fate of Jews in an eastern county which had a multinational character.

A characteristic of Domański's review is that in different fragments of the text he justifies various forms of anti-Jewish violence on the part of the non-Jewish inhabitants. He says that those people were forced by the Germans, did it out of fear (pp. 23, 44–46), had no choice due to the circumstances of the occupation period (pp. 38–39), or were demoralized by, for instance, German propaganda or law (p. 13, footnote 23; pp. 21–22). Our book indeed proves that the reality of the occupation period was much more complex and that it had many different shades in terms of the region, social group, and individual stances. This also calls for in-depth research into the particulars. Many of these most complex conditions and various negative occupation phenomena are presented in our book. However, the reviewer dislikes this and tries to prove that those phenomena are presented incorrectly. Domański has an arrogant approach to the conclusions drawn from our research and the evidence we provide to support them. He also makes strenuous efforts to 'correct the picture' which emerges from our book. The reviewer fails to engage in a debate with the historians who carried out this research on the local microcosms of the occupation period. Instead, he makes various insinuations to discredit their research. This spoils the chance for a discussion on a number of issues which have been waiting for years to be expounded on and discussed in detail. It seems to me, however, that Domański was not interested in a discussion.

Last but not least, I wish to emphasize that I took part in the research project led by Professors Barbara Engelking and Jan Grabowski of my own volition. I was free to select my research territory and discuss the issues taken up in our book in the way I saw fit. I vehemently deny that during the realization of the project anybody influenced my search queries or the editing of the primary sources and research results, which Domański suggests by insinuating that we wrote our texts to prove a thesis (p. 24). The reviewer cannot accept the thought that we could obtain identical or similar research results (the events in Złoczów county had a slightly different dynamic) while writing texts independently of one another! He goes to great lengths to discover a collective conspiracy in our interpretation of sources, construction of narration, etc. I find this difficult to comment on. Domański sometimes moves in the sphere of stereotypical conceptions of the occupation. I shall not discuss his minor accusations because a careful reading of the text will enable one to see their groundlessness or their marginal significance for the content of my chapter and its conclusions.

Every piece of research is subject to supplementation and refinements; some facts are proven false, while others are confirmed, particularly when new sources are discovered. However, Domański, completely fails in this respect. He formulates numerous serious and derogatory accusations against me using distorted evidence, distorting my intentions, and referring to my mistakes of marginal significance. I am also concerned about what he revealed in his review, that is, the fact that various historians employed in the Institute of National Remembrance swapped their reviews of *Night Without End* among themselves. Domański could have also used those reviews before their publication. Hence, I wonder who this response is addressed to. To Domański? Or to the entire team of IPN historians

who spent almost a year on preparing their critical remarks? Did the Institute order the production of critical reviews of *Night Without End*, which it then published? If it did, then this would mean that a public institution was used to attack a group of scholars who produced research results which were not to the IPN's liking. It would also be an example of a centralized and coordinated campaign aimed at disavowing those scholars. This would negate an objective evaluation of the reviewed book and be reminiscent of the rightly by-gone era.