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Discussion and Reply

Comment on “Electromagnetic geophysics: Notes from the past and the road ahead” (Michael S. Zhdanov, 2010, GEOPHYSICS, 75, no. 5, 75A49–75A66)

Discussion by Misac Nabighian[1]

The direct current and electromagnetic methods have been around for almost 180 years and have been applied successfully in mining, petroleum, geotechnical, engineering, environmental, groundwater, and tectonic studies. Over such a long period of time, it is possible to lose track of who the pioneers were that developed the various techniques in current use and who wrote the seminal papers on these techniques. As such, a historical review paper is most welcome and, in the past, there were only few attempts to undertake this task (e.g., Rust, 1938; Ward, 1980; Fountain, 1998). Each of these historical reviews covered only a limited number of topics and I was glad to see the recent attempt by Zhdanov to undertake a similar task in his 2010 paper entitled “Electromagnetic geophysics: Notes from the past and the road ahead.”

I was eagerly looking forward to reading this paper because I have been practicing geophysics for almost six decades and have witnessed the birth and successful development of most modern techniques. I was also encouraged by the fact that in the abstract Zhdanov wrote “This paper describes the evolution of the conceptual and technical foundations of EM methods.” Upon reading the paper, however, I have found a number of improper or missing citations of seminal papers and discrepancies between the sequence of events as stated in Zhdanov’s review paper and my recollections corroborated by the many published papers by many authors and also by the review chapters in the two-volume book “Electromagnetic methods in applied geophysics,” which I edited. Some of these omissions and discrepancies are shown below.

On page 75A52, Zhdanov writes: “Berdichevsky was the first to realize the importance of accounting for the effects of horizontal geoelectrical inhomogeneities on MT data. Berdichevsky’s work on the distortion theory resulted in the method of deep geomagnetic sounding of the earth, created in collaboration with M. S. Zhdanov (Berdichevsky and Zhdanov, 1984).” The reference cited, however, is a book entitled “Advanced theory of deep geomagnetic sounding” that is a synthesis of activity in the area, rather than a seminal paper. Berdichevsky was one of the major contributors to the development of the theory of magnetotelluric and deep geomagnetic soundings, but many others were equally as active, such as Vanyan, Dmitriev, Rikitake, Cantwell, Madden, Swift, Schmucker, Chave, Wannamaker,Weidelt, Weaver, Booker, Price, Jones (A.G.), Jiracek, and Jones (F.W.) and most of their work started in early 1960s. In addition, in the foreword to the above book, on page vii, Berdichevsky and Zhdanov wrote: “The basic ideas in the area of deep geomagnetic sounding have been put forward by H. Wiese, W.D. Parkinson, W. Kertz, M. Siebert, T. Rikitake, and U. Schmuker.” Citing some of the above authors with some specificity would have given a more correct historical perspective about the development of this method.

Berdichevsky was not at all involved in the implementation of the method — this was done by a whole host of others, mostly in the United States, Germany, and Canada in the 1970s and 1980s. Berdichevsky’s main collaborator was Dmitriev and they and their students were studying distortion of MT data numerically in the early 1970s, producing albums of effects for type structures, but almost all of the early literature was published in Russian and was little known outside the Soviet Union at that time. The first papers in English were published by Berdichevsky and Dmitriev in 1976, and most recently, they were included in their 2008 book *Models and Methods in Magnetotellurics*.

On page 75A52, Zhdanov writes, without citing any paper: “Berdichevsky was the first to realize the importance of accounting for the effects of horizontal geoelectrical inhomogeneities on MT data. He introduced the tensor measurements in the MT method, which soon became widely used all over the world.” Before Berdichevsky, however, earlier successful attempts in introducing tensor measurements in the MT method were made by Ted Madden and his students at MIT in their Ph.D. theses (Neves, 1957; Cantwell, 1960) followed later on by Swift (1967). This fact is acknowledged by Berdichevsky and Dmitriev on page 4 in the above 2008 book where they cite only Cantwell (probably because none of the Ph.D. thesis mentioned above were published in geophysical journals and they could only obtain Cantwell’s thesis). Later on, Jimmy Larsen (1977) formulated how a galvanic distortion tensor could affect 1D impedances in the mid-1970s, followed by Schmucker and Wannamaker in the early 1980s who examined galvanic distortion of the magnetic field also. This was followed by Bahr (1988), Groom and Bailey (1989, 1991), Chave and Smith (1994), Smith (1995), and McNeice and Jones (2001), to name but a few, who

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extended it to galvanic distortion of 2D MT responses. There have also been some distortion decomposition attempts in 3D and an innovative, general approach by Caldwell et al. (2004) with the phase tensor method. As an aside, Cagniard in his 1950 patent introduced the linear relationship between the electric and magnetic fields or the tensor admittance, which is closely related with the tensor impedance. Even earlier, Hatakeyama and Hirayama (1934) recognized the existence of the tensor relationship, but their paper was published in Japanese.

In the chapter "New paradigm in EM modeling: Flux and voltage representation of EM fields," the author writes: "It is more natural from the physical and geophysical points of view to describe the EM field by the corresponding flux and work (or voltage) of the field instead of using traditional vector representations (Zhdanov, 2009a, 2010)." This approach has proponents and detractors, but regardless, this topic is not new as stated by Zhdanov in the title of this paragraph: It was already fully treated over two decades ago in refereed geophysical journals (e.g., Jones, 1988; Poll et al., 1989). In addition, the integral forms of Maxwell's equations considering field fluxes and pathway voltages is the basis of the powerful staggered grid differential equation modeling method (e.g., Newman and Alumbaugh, 1995).

In the chapter "Hydrocarbon exploration," Zhdanov writes "The first experiments with marine EM field measurements were conducted by Russian geophysicists in the Arctic Ocean, e.g., Novysh and Fonarev, 1966; Trofimov and Fonarev, 1972." The two Russian papers cited involved, however, only measurements on top of floating Arctic ice and not on the bottom of the ocean. Before them, in the early 1960s, Charles Cox at Scripps Institution of Oceanography began working in seafloor MT (his students Filloux (1967) and Larsen (1968) wrote their Ph.D. theses on this topic). In 1961, Cox, together with Jean Filloux, developed equipment suitable for deep seafloor MT and CSEM soundings and deployed it offshore California in 1 to 2-km water depths. Their equipment was measuring magnetic and electric field components. In 1965, they made measurements 650 km offshore at depths of 4 km (Constable, 2010). Their work was published by Filloux (1967) and in the book *The Sea* (Cox et al., 1971). Filloux subsequently made marine MT practical, and conducted a series of surveys around the world. Cox began working in marine controlled source EM in the late 1970s based on work he did in the early 1970s showing that the EM environment on the seafloor was extremely quiet. His first marine CSEM experiment was in the late 1970s and published in Cox (1980). He especially noted that marine CSEM will be useful in detecting resistive layers offshore. The method was later further expanded by Charles Cox, Steve Constable, and Alan Chave, and ultimately, was transformed into the technology used commercially today. Later on, Nigel Edwards proposed a time-domain approach to seafloor EM surveys and published a number of seminal papers on this topic. A complete treatment of the early work can be found in Chave et al. (1991).

In the chapter "Birth of geophysical inversion and regularization theory," Tikhonov's outstanding contribution to the development of the theory of inverse problems is beyond question. Although the regularization technique per se has been around in the mathematical literature since early 1920s, Tikhonov was undoubtedly the first to apply it to inverse geophysical problems and he further extensively developed the basic ideas of the theory. However, much parallel work was also done independently in the west. Much of the credit for practical inversion in geophysics (for the deterministic approach at least) must be given to George Backus, Freeman Gilbert (Backus and Gilbert, 1968), Robert Parker (1980, 1994) who showed the importance of resolving kernels and the regularization functional (or model objective function) in determining the final model and outlined the numerical underpinnings that has allowed the field of practical inversion to develop. Robert Parker and his students were especially instrumental in making regularization practical.

In the chapter "From frequency-domain to time-domain methods," there are a number of omissions and discrepancies. The successful application of the frequency soundings in the far zone was developed and applied in the 1960s by Vanyan (1967) and his coworkers and the method was used by many soviet companies to investigate large areas of the USSR for petroleum resources. In those times, this method together with MT was the leading method of petroleum exploration in USSR. It is difficult to imagine any further developments in this area without their work. The paper omits the very important period when frequency soundings in the far zone were replaced by transient soundings in the far zone. This became possible as a result of the theoretical work of Tikhonov and his coworkers (Tikhonov and Skugarevskaya [1959]; Tikhonov and Shakhsuvarov [1959] among others) and also Vanyan. The resulting TDEM method found extensive application in petroleum exploration in USSR and was also used latter in the west by G. Keller over many years, predominantly for geothermal studies.

In the same chapter, the history of the development of transient sounding in the near zone needs some further clarification. In 1965, Frolov published a paper about the asymptotic behavior of the transient field of an electric dipole in the far zone above a two-layered medium, when the basement is an insulator, and he discovered that the known relation between the magnetic field and longitudinal conductance in the far zone remains valid regardless of the distance between the source and receiver. This result was surprising at that time and it stimulated the beginning of a new direction in the study of transient soundings. Three years after Frolov's work, Obukhov (1968) published a paper in which he considered a similar model and obtained the same result as Frolov. During the same year, Kaufman and Morozova derived the late stage expressions for small separations between source and receiver and for the more general case when the basement has a finite resistivity (Kaufman and Morozova, 1968). All of the above publications did cite Frolov's paper and acknowledged that their work was a continuation of Frolov's work who rightfully should be cited instead of Obukhov. Almost simultaneously, the above theoretical results were also confirmed experimentally by V. Sidorov. Afterward, the main developments of the transient soundings in the near zone was concentrated mainly in Novosibirsk (Kaufman and his colleagues) where they developed the theory, calculated apparent resistivity curves, built the equipment, and applied the method successfully for mapping an oil-water contact in East Siberia. Their work was published in many papers and resulted in the publication of the first book about the theory and interpretation of the method and a book of instructions for the use of the method. Note that around the same time, two other groups of geophysicists in Moscow and Saratov were also involved in the development of different aspects of the method.

In the chapter "EM soundings using high-power EM pulses: the Khibini experiment," the author describes at length the use of a MHD generator in USSR for deep sounding. In my opinion, this technique was used only once because, besides being prohibitively expensive, it has not been shown to yield superior results to, say, an MT survey or to stacking the signal from a much weaker source over time to narrow the receiver bandwidth and thus increase the signal to noise ratio. As an aside, similar attempts to develop a portable MHD generator for controlled-source EM studies were also done in the 1980s by the Berkeley group led by Frank Morrison (Zollinger et al., 1987) again with no further follow-up. The results of these experiments were reviewed by Spies (1993).

The chapter on "Electromagnetic imaging and migration" appropriately cites Zhdanov's papers on this topic starting with the original ones published in 1983 in collaboration with Frenkel (the original title of their paper was "The solution of the inverse problems on the basis of the analytical continuation of the transient electromagnetic field in reverse time"). This concept was also developed independently in the west by Lee et al. (1987). Recovering conductivity information inside the earth is a difficult task because the electromagnetic response is mainly controlled by a diffusion process, which broadens the signal. Absorption and dispersion effects are much larger in EM data than in seismic data and as such only low frequencies are available for EM imaging. Only under some limiting conditions does the electromagnetic field behave as a seismic wave. As such, the approach proposed by Zhdanov has proponents and detractors. The method has gained presently only very limited acceptance mainly because some knowledge of the background conductivity is crucial to start with, and this is what you are trying to find. How well does the proposed new iteration migration scheme (where the total model is refined based on the misfit of the computed fields from the initial migrated model at the receiver compared to the observed) does overcome this problem? This issue is not addressed and as such, it is difficult to fully determine the merit of this technique.

In the chapter on "3D Numerical Modeling and Inversion," I found most of the cited publications to refer to papers published only relatively recently with some important earlier seminal papers missing. In particular I refer to the important original work done at the University of British Columbia by Doug Oldenburg and his students (Yaoguo Li, Colin Farquharson, Partha Routh, Eldad Haber, etc.), which set the framework for many of the practical inversion algorithms that followed. The 1994 (2D) and 2000 (3D) papers by Doug Oldenburg and Yaoguo Li on inverting DC resistivity and IP data led not only to useful inversion programs, which are predominantly used today by industry, but they also provided an important training ground for learning about the practicalities of solving underdetermined inverse problems and for inverting the more complicated EM problems. UBC has been a leading group in the development of rigorous inversion programs for 3D EM problems in time and frequency domain. The paper "Controlled source electromagnetic inversion for resource exploration" (Oldenburg et al., 2005) was among the first, if not the first, paper to show an example of rigorous 3D multiple source EM inversion applied to a mineral prospect. That paper also demonstrated that the same 3D inversion algorithm was applicable to the marine EM problem. Last, but not least, the UBC group was the first to invert full waveform TEM data for multi-transmitters (Napier et al., 2006) for a mining application. The UBC programs for forward and inverse modeling are now predominantly used by industry.

One of the most important techniques used today in EM exploration geophysics is the time-domain method. The author devotes significant space in the paper in describing the advantages of this technique and one of the chapters in the paper is appropriately titled "From frequency-domain to time-domain methods." Surprisingly, none of the original publications that led to the development of this technique are mentioned in the text of this historical review paper and there is only a short footnote about this topic.

James R. Wait helped establish through many seminal papers the theoretical background of many electromagnetic methods used currently in exploration. In 1951, Wait published in GEOPHYSICS a most important paper in which he described the transient response of a conducting sphere when the primary field varies as a step function. This was the first paper published in a geophysical journal where it was clearly shown that by using time domain methods, it is possible to determine the parameters of a conductor by measuring the transient field which led to the development of time domain methods of exploration. Wait was working for Newmont at the time and Newmont, in 1956, obtained a patent on the technique. Shortly afterward, George McLauglin built the first time-domain instrument and, together with William Dolan carried out the first successful survey over the Mavrovouni deposit in Cyprus. Afterwards in the 1960s and 1970s, Newmont carried out extensive numerical and analog modeling to better understand the response from various conductors, to account for the effect of overburden and surrounding media on time domain measurements and to develop robust interpretation techniques (Nabighian et al., 1971). Similar studies began in the Soviet Union after 1958 when Yakubovsky in Moscow and Velikin in San Petersburg obtained a copy of Wait's paper and, inspired by it, immediately started doing research with their colleagues on time domain techniques which led to the development of the theory, interpretation, and the required equipment for a coincident loop and central loop transmitter-receiver system. Further developments of the transient soundings in the near zone were concentrated afterwards mainly in Novosibirsk (Alex Kaufman and his colleagues). Because Wait's (1951) seminal paper helped establish the time domain method and has had such a tremendous impact on both sides of the iron curtain, the lack of a proper acknowledgement and citation in the main body of the paper presents an incomplete picture of the historical development of this important technique.

The reference to the Wenner paper for measuring earth resistivity is incorrectly dated, in the text and the reference list: the date should be 1916 and not 1928.

It is worth mentioning that up until late 1970s, much of what happened in the Soviet Union was unknown in the west and vice versa and therefore had no influence. So, the concept of primacy across the Iron Curtain is meaningless at best, as work was done largely independently in two places. Proper acknowledgment of accomplishments on both sides of the iron curtain is the most balanced way to present a historical perspective.

As a final note, looking at the list of references cited in this historical review paper, I was somewhat surprised by the disparity between the number of citations of papers authored or co-authored by Zhdanov (26) and those by other authors (maximum three).

Before concluding, I want to acknowledge the help provided by Alex Kaufman in checking my entries and also making the necessary corrections to the sequence of events that took place in the Soviet Union during the development of the techniques mentioned above. During the last six decades, Kaufman published numerous books and seminal papers on EM methods and was an active

participant and an eyewitness during the development of most of the techniques mentioned here.

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Reply to the discussion

Michael S. Zhdanov[2]

Electromagnetic geophysics has a long and rich history spanning almost two centuries. During this time, many generations of scientists and engineers have contributed to the development of electrical and electromagnetic methods. Thousands of papers have been published on this subject. It would be absolutely impossible to try to cover the huge volume of wonderful results and publications developed over two hundred years in one relatively short journal paper.

That is why my paper has the title "Electromagnetic geophysics: Notes from the past . . . " and I wrote in the introduction to the paper, "It is impossible to include in one relatively short paper a long and rich history of EM geophysics," and a few lines below, "So in this paper, I present a few stories, describing some of these individuals and their discoveries. I also discuss recent developments in data-acquisition, modeling, and interpretation methods." Thus, my paper was not intended to represent a comprehensive historical review, as Nabighian mistakenly stated on several occasions in his "Discussion." Quite the opposite, my paper is a collection of "notes from the past," providing snapshots of some past and recent discoveries in EM geophysics which, from my point of view, would be of interest to the geophysical community.

In selecting the stories to be told in that paper, I tried to find topics and events that had received relatively little coverage in the geophysical literature and, at the same time, reflected important turning points in the development of EM geophysics. I also wanted to write about people whom I knew personally and with whom I had the pleasure to work with for decades. I was fortunate to have as my teachers and colleagues such giants of modern science as academicians A. N. Tikhonov, E. P. Velikhov, and M. N. Berdichevsky, among others. The goal of my paper was not to present a mechanical, if not dry, account of the chronological sequence of different research papers (which is a standard practice in writing review papers), but rather to tell a few stories showing the human side of several scientists who made important contributions in our field, and to describe, even briefly, the circumstances surrounding those discoveries.

The "Discussion" paper by Nabighian, unfortunately, is based on an erroneous assumption that the "Notes . . . " represent a "comprehensive historical review." No, it was not intended to serve as a comprehensive historical review. My "Notes . . . " were intentionally structured as just that — "Notes" — so as to avoid any confusion with a comprehensive historical review.

I could probably wrap up my reply with this short clarification, which should explain the misunderstanding and confusion expressed in Nabighian's "Discussion." Yet, at the same time, I was reading the "Discussion" paper of Nabighian with interest, anticipating finding some new important facts and interesting references, which would complement what I had written. Unfortunately, I found very few new facts and rather many incorrect, if not erroneous, statements that provided a distorted description of the events presented in my original paper. This forces me to reply to his "Discussion" paper in greater detail.

Nabighian begins his "Discussion" with unwarranted questioning of the legacy of Mark N. Berdichevsky. In support of his argument, Nabighian cites Berdichevsky's own books, where the contributions of the works of others were acknowledged. I found this logic very strange. Berdichevsky always was very generous with his acknowledgments of the work of his peers. However, most of the fundamental results presented in his many books and papers were based on his own research and represented developments that were principally new for that time in the magnetotelluric (MT) method. All the books published by Berdichevsky were seminal works in magnetotellurics and not just a "synthesis of activity in the area," a strange term used by Nabighian in his "Discussion." Berdichevsky began studying the effects of horizontal geoelectrical inhomogeneities on MT data as early as the 1960s (e.g., Berdichevsky, 1960, 1961, 1963), which is also reflected in his pioneering book on the telluric method published in Russian in 1960 and later translated into English in 1965 (Berdichevsky, 1965). He was one of the founders of MT research and applications in the former republics of the Soviet Union and Eastern European countries. The complete list of publications by Berdichevsky can be found on the Internet at http://www.mark-berdichevsky.narod2.ru/BerdPubl.htm.

Contrary to Nabighian's assertion, Berdichevsky was actively involved in practical applications of the MT method in Russia and abroad. The importance and significance of Berdichevsky's contribution to magnetotelluric theory and methods are widely accepted and well recognized by the international EM geophysical community, and his work is firmly imprinted in the history of EM geophysics.

These facts do not diminish the significance of the work of others involved in the development of the MT method. I provided a brief review of the development of the MT method in my "Notes" and referenced practically all of the same outstanding researchers whose names were repeated in Nabighian's "Discussion." In fact, even references to the early works of Hatakeyama (1938) and Hirayama (1934) were already given in my "Notes." There are very few new names and/or facts in Nabighian's description of MT development in addition to what was discussed in the "Notes." However, the fact is that, in no way would the "Notes" and the "Discussion" paper by Nabighian provide a complete historical review of the development of the MT method. Many outstanding scientists, research groups, and companies, who have made significant contributions to the theory and applications of MT method, unfortunately, were not mentioned in my "Notes" or in Nabighian's "Discussion." For example, one should mention, among many others, the contributions by A. Adam, K. Baar, J. Booker, F. Bostick, M. Chouteau, S. Constable, V. Dmitriev, G. Egbert, D. Eggers, E. Fainberg, C. Farquharson, L. Fox, T. Gamble, J. Gough, J. Jankovsky, G. Jiracek, A. Jones, B. Hobbs, G. Hohmann, J. Hermance, S. Hjelt, R. Hutton, A. Kaufman, J. Kingman, K. Key, A. Kovtun, T. Madden, R. Mackie, M. Menvielle, G. Molochnov, F. Morrison, P. Nelson, A. Orange, D. Oldenburg, S. Park, T. Ritchie, W. Rodi, N. Sheard, T. Smith, U. Schmucker, A. Schultz, B. Svetov, M. Unsworth, S. Urquhart, Iv.

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Varentsov, K. Vozoff, P. Wannamaker, S. Ward, D. Watts, J. Weaver, P. Weidelt, etc. And still, this list is incomplete. The history of the magnetotelluric method may constitute the subject of a separate historical review paper which has not been written yet.

It is quite unfortunate that Nabighian tries also to raise questions about the significance of the contribution and pioneering role of another titan of the 20th century, academician A. N. Tikhonov. The statement that "the regularization technique per se has been around in the mathematical literature since the early 1920s" is misleading, because no regularization theory in the form which was originally introduced and developed by Tikhonov existed prior to his research. The closest to Tikhonov's ideas was the Marquard-Levenberg method, which, however, represented a simplified case of linear least-square inversion (Levenberg, 1944; Marquardt, 1963). Nabighian erroneously cites the works of Backus and Gilbert (1968) and Parker (1980, 1994) as an example of "much parallel work was also done independently in the west." The contributions of Professors Backus, Gilbert, and Parker in inversion theory were outstanding. However, none of these authors contributed significantly to the field of *regularization theory.* At the same time, Tikhonov's name became synonymous for the regularization theory. His fundamental book, *Solutions of Ill-Posed Problems*, published in co-authorship with Vasiliy Arsenin (Tikhonov and Arsenin, 1977), has been cited almost 7000 times (according to Google Scholar). All modern papers on inversion theory and applications refer to the regularization method as "Tikhonov regularization." In this light, Nabighian's attempt to downplay the role and place of Tikhonov in the history of geophysical inversion has absolutely no merit. At the same time, the literature on the regularization theory consists of thousands of publications. A historical review paper on this rapidly developing subject has not been written yet.

Nabighian noticed in his "Discussion" that the "Notes" did not provide a complete historical review of the research in the field of marine EM. This is a correct observation. The point is that Steven Constable was also publishing a detailed review of marine EM methods in the same issue of Geophysics, where the "Notes" were published. I had no reason and no intention to duplicate his review paper in my "Notes." However, I included in my "Notes" a few references to the results of early work by Russian researchers on the Arctic Ocean, because that work had been previously unknown in the West (and actually was not included in the review paper by Constable). I was somewhat surprised that Nabighian was upset by this reference to early marine EM measurements in the Arctic Ocean. I think that all pioneering experiments, whether they were made on the sea bottom or on the Arctic ice, have the right to be placed in the history of EM geophysics.

In the discussion related to the use of an MHD generator in the USSR for deep sounding, Nabighian writes that "in my [his] opinion, this technique was used only once because, besides being prohibitively expensive, it has not been shown to yield superior results . . . " Unfortunately, this opinion is not supported by the facts. The MHD generators were used in the USSR and in Russia many times over several decades for regional geological studies, monitoring active seismic zones, and oil and gas exploration (see, for example, Velikhov and Panchenko, 2010). The MHD generator, indeed, represents a unique source which provides very unique data about the deep geoelectrical structure of the earth. I wrote in my "Notes" that "further development of an EM surveying system with a very powerful MHD generator was limited by the high cost of this exploration tool." In fact, the MHD generator is nothing else but a rocket "chained" to the ground. In this sense, the situation with the use of MHD generators in geophysics is somewhat similar to what happened with the Apollo missions to moon. The Apollo missions were extremely expensive and have not been repeated since the 1970s. However, they have resulted in the tremendous progress in the development of the science and technology, and will stay forever in the history of humanity. The use of MHD generators in geophysics also stimulated rapid development of distributed data acquisition systems and new 3D interpretation techniques in industry and in academia in Russia. That is why this work deserves its place in the history of EM geophysics.

The comments of Nabighian related to EM migration are based on a fundamental misunderstanding as to what migration is. Mathematically, migration is defined as the action of an adjoint operator on the observed data. This has physical significance, as for EM and seismic fields, the adjoint operator manifests itself as reverse time, and implies that it is possible to reconstruct a model from observed data. Nowhere in this definition does it imply any requirement on the wave or diffusive regime of the fields. In fact, when Zhdanov (1981) first proposed the analytic continuation of EM fields in reverse time and presented this result at the 6th IAGA Workshop on EM Induction in the Earth and Moon in 1982 in Victoria, Canada, it was John Booker from the University of Washington who suggested the method be called "electromagnetic migration." With this definition of migration, it is quite clear that EM migration does not require that "the electromagnetic field behave as a seismic wave," as erroneously stated in the "Discussion." I should note also that, the background conductivity in EM migration plays exactly the same role as the initial or a priori conductivity in an iterative inversion. Therefore, contrary to the erroneous statement by Nabighian, "the knowledge of the background conductivity" does not put any "crucial" limitation on EM migration any more than an initial or a priori conductivity places a "crucial" limitation on EM inversion. Mathematically, iterative migration is the action of the adjoint operator on the residuals of the observed and predicted data. This has been discussed in several publications, cited in my "Notes," and has been proven to be *equivalent* to iterative regularized inversion. The difference is in the physical interpretation *and* numerical implementation of both methods.

Another comment of Nabighian related to a new paradigm in EM modeling is also based on a fundamental misunderstanding. Apparently, there is confusion with regard to the difference between the integral forms of Maxwell's equations, which have been known for a century, and a new approach to Maxwell's equations, based on the algebraic theory of differential forms, which was introduced quite recently. What is discussed in my "Notes" is this new mathematical form of Maxwell's equations which emphasizes the importance of the fluxes and work of an EM field in the description of the field and in its numerical modeling. This topic has not been "already fully treated over two decades ago," as mistakenly stated by Nabighian, and the distinguished papers by Poll et al. (1989) and Jones (1988), did not discuss any theoretical developments related to algebraic theory of differential forms for Maxwell's equations.

There is just one paragraph in my paper dedicated to numerical modeling and inversion. It was impossible to include all, even the most important, papers published over the last century in this one paragraph. That is why I wrote, "During the last decades, considerable advances have been made in all of these areas. Overviews of the effective modeling methods can be found, for example, in Hohmann (1983), Avdeev (2005), and Zhdanov 92002, 2009)." Unfortunately, I have to repeat yet again that the goal of my paper was not to provide a comprehensive review of the developments in forward modeling and inversion, as Nabighian mistakenly bequest in his "Discussion."

At the same time, Nabighian provides an incomplete list of additional names and references in his "Discussion." He correctly cites the work done at the University of British Columbia by Oldenburg and his research associates and students, who have made outstanding contributions to the development of numerical methods and software and who were actually cited in my "Notes." However, if one were to be comprehensive about the history of 3D EM modeling and inversion, it would be necessary to mention the longstanding contributions of the Consortium for Electromagnetic Modeling and Inversion (CEMI) at the University of Utah, founded more than three decades ago by the late Hohmann. Several generations of EM geophysicists have graduated or conducted postdoctoral research at the CEMI consortium over the last decades, including (but in no way, an exhaustive list), M. Oristaglio, P. Eaton, G. Newman, P. Wannamaker, A. Tripp, Z. Xiong, L. Pellerin, J. Rijo, T. Wan, L. Beard, P. Traynin, B. Kriegshauser, P. de Lugao, S. Fang, G. Hursan, D. Pavlov, O. Portniaguin, N. Golubev, L. Wan, M. Endo, G. Wilson, L. Cox, N. Black, A. Gribenko, and V. Burtman, among many others. Also, from 1980 to 2008, Art Raiche led AMIRA International's longest running exploration project series, and was supported by the likes of F. Sugeng, Z. Xiong, P. Gupta, K. Vozoff, D. Jupp, and G. Wilson. One should also cite the major contributions made to the field of quantitative EM geophysics by the research group headed by Frank Morrison and Alex Becker at the University of California in Berkeley, which included such well known researchers as D. Alumbaugh, M. Hoversten, G. Liu, D. Pridmore, C. Torres-Verdin, and M. Wilt, among many others. Outstanding research has been generated for decades by the EM researchers at Schlumberger Doll Research, headed by M. Oristaglio and T. Habashy, and involving such researchers as B. Spies, V. Druskin, and A. Ababakar, to name a few. The contribution of the research group in the Moscow State University, headed by Vladimir Dmitriev, played a pivotal role in developing the distortion theory at the MT method. L. Tabarovsky and M. Epov and their associates from Novosibirsk State University made significant contributions in the development of the numerical modeling methods as well. Other significant contributors to 3D EM include D. Avdeev, M. P. Buonora, J. Carazzone, D. Colombo, S. Davydycheva, P. Dell'Aversana, R. Ellis, M. Everett, M. Frenkel, P. Fullagar, J. Macnae, L. Ó Súilleabháin, A. Price, K. Spitzer, Y. Sasaski, J. Singer, E. Slob, W. Siripunarapom, P. Tarits, M. Tompkins, Iv. Varentsov, C. Weiss, G. West, and many other researchers in Australia, Brazil, Canada, the Czech Republic, China, Finland, France, Germany, Italy, India, Japan, Hungary, Mexico, Norway, Poland, Russia, Swedish, UK and the USA, to name just a few countries. This is, of course, not repeating many of the names cited above regarding MT method. The bottom line is that it is impossible to reflect in one journal paper or a few discussion papers the huge volume of past and present research in EM modeling and inversion.

I was somewhat surprised by Nabighian's comments with regard to the chapter "From frequency-domain to time-domain methods." I feel again that these comments were based on yet another misunderstanding. I did not intend in my short "Notes" to provide a complete historical review of the development of controlled-source EM methods. Nevertheless, this chapter began with references to the work of Frischknecht, Vanyan, Keller, Wait, and Kaufman. The name of James R. Wait appears not just in a footnote, as erroneously stated in the "Discussion," but also in the *front note* of the chapter.

In reviewing the chapter "From frequency-domain to time-domain methods," Nabighian erroneously states that "Frolov's work ... rightfully should be cited instead of Obukhov." Unfortunately, Nabighian was not an eyewitness to the events which took place in the USSR in the 1960s, and he was probably unaware of the true sequence of events and the historic environment in which these events happened. In fact, the pioneering work of G. G. Obukhov on the development of the transient soundings method in the near zone was mostly unknown in the West, and his contribution to EM geophysics was downplayed by some of his former colleagues. At the same time, the situation with his discovery provides a very interesting and, unfortunately, typical history lesson.

Frolov's paper (Frolov, 1965), as Nabighian correctly stated, was dedicated to the analysis of the asymptotic behavior of the transient field of an electric dipole in the far zone above a two-layered medium. Frolov wrote in the abstract of the paper, "Magnetic field buildup is considered at large distances from the source of excitation in a horizontally layered conducting medium underlain by either a nonconductor or a perfect conductor. The derived formulas permit a relatively easy calculation of the entire buildup curve at large distances from the source." There is indeed a brief comment made by Frolov that one of the asymptotic solutions he received "represented the final stage of buildup for all values of r." That is it. However, Frolov did not realize and did not discuss in his paper the possibility of EM sounding in the near zone. It was the work of Obukhov on the physics of the transient EM field propagation in the near zone and on the principles of a new electrical prospecting method, which resulted in the paradigm change in the transient sounding method and which generated a flurry of activities in the development of new EM exploration technology in several Russian research institutions in Moscow, Novosibirsk, and Saratov in the 1960s.

Obukhov published this result in his seminal paper "About some properties of the nonstationary electromagnetic fields in the earth in their application in electrical prospecting" (Obukhov, 1968). In the introduction to this paper Obukhov wrote, "Thus, there is a possibility of performing electrical prospecting measurements in the immediate vicinity of a source operating in a pulsed mode, thus making it possible to considerably increase the efficiency and accuracy of electrical prospecting." Obukhov did not repeat the work of Frolov, as erroneously stated in the "Discussion" by Nabighian. In fact, Obukhov introduced and mathematically proved a principally new concept of the transient sounding in the near zone, which was a revolutionary new idea at that time for the reasons discussed in my "Notes." In conducting this research, Obukhov demonstrated a unique physical insight and scientific vision, which enabled him to make this discovery.

Obukhov presented his result at the research seminar in Moscow long before the publication of his seminal paper. However, Obukhov and his work were subject to harsh criticism by almost all of geophysicists present at that seminar. It is noteworthy that Frolov's paper had already been published by that time, but nobody had noticed any similarity between Frolov's work and the revolutionary ideas of Obukhov. Obukhov was humiliated and depressed by the unwarranted negative reaction to his presentation. It took a few months for most of his critics to realize and to accept that Obukhov was actually right and that their criticisms were wrong. This was a very dramatic event, which could not be captured by the mechanical citations of academic papers, as Nabighian was trying to do in his discussion paper.

It is unfortunate that more than 40 years after these remarkable events Nabighian, probably unintentionally, still follows the footsteps of the same old zealous critics of Obukhov, who originally could not accept his revolutionary ideas, and who later on tried unsuccessfully to downplay Obukhov's role in the development of the method of transient EM sounding in the near zone.

In conclusion, I would like to say that this example clearly demonstrates a major difference between my "Notes from the past" and a traditional academic review paper, which Nabighian anticipated to find in my publication. I did not write a "historical review paper," as Nabighian erroneously suggested. I was telling the personal stories of a few outstanding individuals, who made significant contributions to our science. I feel that it is important that in looking back in history one would pay attention to the historical environment and the circumstances of the scientific development instead of mechanically counting a long list of otherwise faceless references. I believe that the human side of scientific developments is as important as the technical side. We do not need to forget that many great discoveries in the past required heavy work and vigorous defense against the zealous critics, who tried, though unsuccessfully, to discredit the work of others.

Nabighian's "Discussion" aside, I received many favorable replies to my "Notes . . . " and I hope that they will help present and future generations of geophysicists to understand how difficult was the road to developing EM geophysics in the past and how challenging is the road ahead.

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