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Death as a Condition for Life? Reply to Bernard Berelson's "The State of Communication Research"

Juan Carlos Arias¹

Abstract

In 1959 Bernard Berelson published his famous text "The State of Communication Research", in which he radically states the death of this field. The reactions were swift, and authors like Schramm, Riesman and Bauer replied to Berelson's text defending the vitality of the field of Communication and disqualifying his diagnosis. Today, however, it could be useful to reconsider Berelson's sentence in order to understand what particular field was vanishing. The death sentence pronounced by Berelson, beyond expressing the state of the field of Communication in the 50s, reveals some fundamental questions about Communication research in general; Questions that many have assumed to be resolved but which may be more relevant today than ever.

Key words: Bernard Berelson, communication research, object, method, communication theory.

¿La muerte como condición de la vida? Respuesta al texto de Bernard Berelson "Estado de la Investigación en Comunicación"

Resumen

En 1959 Bernard Berelson publicó su famoso texto "El estado de la investigación en comunicación" en el cual afirma radicalmente la muerte de este campo. Las reacciones no se hicieron esperar, y autores como Schramm, Riesman y Bauer replicaron al texto de Berelson defendiendo la vitalidad del campo de la comunicación y descalificando su diagnóstico. Sin embargo, hoy puede ser útil reexaminar la sentencia de Berelson intentando comprender qué campo particular era el

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que estaba desapareciendo. La sentencia de muerte pronunciada por Berelson, más allá de expresar el estado del campo de la comunicación en los años 50 pone en evidencia preguntas fundamentales sobre la investigación en comunicación en general; preguntas que muchos asumen como resueltas pero que pueden ser hoy más relevantes que nunca.

Palabras clave: Bernard Berelson, investigación en comunicación, objeto, método, teoría de la comunicación.

A morte como condição da vida? Resposta ao texto de Bernard Berelson "Estado de Pesquisa de Comunicação"

Resumo

Em 1959, Bernard Berelson publicou seu famoso texto "O estado da pesquisa em comunicação", no qual afirma radicalmente a morte deste campo. As reações não se fizeram esperar e autores como Schramm, Riesman e Bauer replicaram o texto de Berelson defendendo a vitalidade do campo da comunicação e desqualificando seu diagnóstico. Contudo, hoje pode ser útil reexaminar a sentença de Berelson tentando compreender que campo particular era o que estava desaparecendo. A sentença de morte pronunciada por Berelson, mais além de expressar o estado do campo da comunicação nos anos 50, põe em evidência perguntas fundamentais sobre a pesquisa em comunicação em geral; perguntas que muitos assumem como resolvidas, mas que podem ser hoje mais relevantes que nunca.

Palavras-chave: Bernard Berelson, pesquisa em comunicação, objeto, método, teoria da comunicação.

In 1959, Bernard Berelson published "The State of Communication Research". Appearing in the *Public Opinion Quartet*, it was a controversial text in which he reviewed the state of the field and put forth several suggestions for the future development of communication studies. Undoubtedly, the most striking statement in Berelson's text concerns the death of communication research: "My theme is that, as for communication research, the state is withering away" (Berelson 1959, 441). This is the main object of the responses written that same year by authors such as Schramm, Riesman and Bauer, who focused on offering evidence of the current vitality of the field, which probably would be the immediate reaction of any scholar who feels his conceptions and beliefs about a topic have been questioned so explicitly. Nowadays, with the advantage of greater historical distance, we could offer numerous examples of approaches, lines and problems in communication research that would refute Berelson's judgment. Actually, it would suffice to take his suggestions on the future of communication research and empirically test whether or not they have developed up to now. If we found that one of his proposals has materialized, we paradoxically would prove him wrong in his diagnosis.

However, the problem is not reduced merely to presenting empirical evidence to prove Berelson's diagnosis is wrong, but above all to understand what assumptions were behind his radical affirmation and, if possible, to critique them. This response probably would not lead us to *demonstrate* Berelson was wrong and the field was, and is, alive. Nevertheless, it might allow us to understand the implications of a dead sentence and to reflect on some of the problems inherent in the field. The question is *which* field is dying and, more precisely, whether it is necessary to defend the vitality of *that* field in order to reply to Berelson's judgment.

These questions are more relevant today, when we seem to assume communication is already an established field situated somewhere between the social and "hard" sciences. Replying to Berelson is not only a question of responding to a particular text written in the 1950s. It is also a matter of proposing reflection on the way the "field" of communication research has been regarded throughout the last 50 years.

The first assumption in Berelson's text, and perhaps the most evident, is a historical one that refers to how the author could know the field was "withering away". He does not compare the current state of the field to other existing fields or disciplines, not even to a general criterion about the development of the disciplines. Berelson makes an internal comparison: the field is dying compared to its own origin.

Thus, he proposes a temporal division of the field, and placed its modern origins twenty-five years before his published his article; that is, in the 1930s, with the works of Laswell, Lazarsfeld, Lewis, and Hovland. "The modern version of communication research began around twenty-five years ago with the development of both academic and commercial interests, the former largely being coordinated, if not stimulated, by the Rockefeller Foundation Seminar in the late 1930's and the latter being developed in the response to radio's need to prove its audience" (Berelson 1959, 441).

The first impulse on the part of an unwary reader might be to criticize this particular delimitation. On one hand, we could argue Berelson's criterion for deciding what the major approaches are is unclear. It appears he decided to privilege the names of those four authors, because he believed they exerted more influence on other authors and research lines than the so-called "minor approaches". However, he did not explain how he measured the influence of

those authors on the development of the field. Therefore, one of the main problems is how to classify the authors and the texts he left out of that specific delimitation. Why did he fail to include other names and approaches? Why did he place the origin of the field at that point in time, and not before or after? Is there a “pre-modern” period in the field, and what would be its characteristics? All these questions point to a historiographic issue concerning the temporal delimitation of the field.

On the other hand, it seems difficult to maintain a criticism based only on a purely temporal perspective. It is impossible to identify clear temporal borders to define the correct origin of the field and to demonstrate the falsehood in Berelson’s division. The unwary reader should set a possible delimitation, just as Berelson did, and produce another historiographic version of the origins of the field of communication research.

There are several options to choose from, and a review of a few of them probably is enough to show the complexity of the issue. According to McQuail (2002), the concept of mass communication, which served as the axis of the “first generation of post-war research,” appeared during the 1930s. Peters and Simonson (2004) point out the concept did not have a unified shape before the 1950s. DeFleur, more radically speaking, places the origin of communication research within the influence of the nineteenth century concept of mass society framed by the theories about mass communication (Bineham, 1988). Finally, Chaffee and Hochheimer “begin their account of mass communication history with Lazarsfeld’s 1944 publication, *The People’s Choice*” (Bineham 1988, 237).

The conclusion, thus far, is very simple. Since there is no unique and universally accepted temporal differentiation of the origins of the field, it is impossible to void Berelson’s position

from a purely historiographic perspective. It is necessary to make a conceptual shift from history to epistemology.

As Sproule (1987) points out, every historical approach depends on a theoretical perspective; that is, on an epistemological delimitation. Thus, the question about the delimitation of the communication research field is “not focused on the ‘factual’ validity of the different positions, but on the theoretical commitments that sustain those positions” (Sproule 1987, 232).

In short, it would be possible to affirm that Berelson’s diagnosis of the death of the field of communication research assumes a historical delineation. However, following Sproule’s perspective, this historical delimitation, as any other, presupposes an epistemological perspective that defines the object of the field. Hence, the problem is not what the empirical delimitation of the historical origins of the field is or is not, but what is the criterion used to define the empirical object of the field that allows Berelson, or any author, to define a specific historical period.

The same shift applies to criticism based on current examples to counter Berelson’s judgment. The empirical arguments of Schramm, Riesman, and Bauer are not only historical examples that accept Berelson’s epistemological assumptions and simply try to add a new author or approach that had been left out. They are, on the contrary, inquiries into the theoretical definition of the object of the field. When they add a historical instance, they expand or relocate the epistemological definition of the field.

However, the problem that arises now is that Berelson’s epistemological definition is as ambiguous as his historical one. This actually is one of the elements underlined in Schramm’s reply when he asked about Berelson’s understanding of the word “approach”. For instance, when

Berelson mentions Laswell and his “political approach,” he seems to be focused on Laswell’s disciplinary perspective of the *object*. In the chart he includes in his text, Berelson defines Laswell’s main interest as a “broad politico-historical approach. Concern with power” (Berelson 1959, 442). However, when he mentions Lazarsfeld’s sample survey procedure, he privileges the *method*: “Specific short-range empirical problems; tie to market research. Concern with audience and effect” (442). Moreover, the boundaries between these approaches, regardless of their nature, are not as rigid as Berelson seems to propose. Schramm (1959) provides several examples where the same author mixes methods and disciplinary perspectives. His conclusion seems to be definitive: “The ‘approach’ changes with the field” (Peters and Simonson, 448). This is such an important topic that I will return to it by the end of this piece. However, for now, I believe it is important to take an in-depth look at Berelson’s epistemological criteria.

That very same ambiguity in the criteria used to define the communication research field shows the lack of an explicit epistemological definition in Berelson’s text. Let us assume Berelson must have had one, but did not make it explicit in his reflection. It is possible to identify a few “clues” in the text, so as to make it explicit and, eventually, to take a position with regard to his perspective.

Let us review Berelson’s historical version of the field, this time from an epistemological standpoint. Berelson highlights two main points in the definition of the modern communication research field: the Rockefeller Foundation Seminar and the studies on radio audiences, both in the 1930s. What could be the differences introduced by these two “events” in contrast to previous research. Could they be, for instance, all the reflections about modernity and communication developed in the late nineteenth century

by thinkers such as Cooley, Dewey and Adams? If Berelson placed the origins of the “golden age” of the field in the 1930s, he would have to assume a conceptual difference –and not just a historical one– among the four major authors cited and all previous reflections. Let us forget, for a moment, the various discussions about the temporal borders of the field mentioned at the beginning of this text, and assume there could be several historical reasons for placing the origin of the communication research field between the two World Wars, specifically in the 1930s. Today, a number of authors agree on drawing a boundary in that period by separating the background of the field from its constitution as such. Peters and Simonson, for instance, separate the “Mass Communication Theory Coalesces” from “Communication Research” (Peters and Simonson, Index). This division seems to be accepted today, and could be defended by using several historical data. However, the question is whether that historical division, marked by Berelson fifty years ago, also implies an epistemological one.

In reading about the history of the field, one could perceive continuity rather than a separation between the different “pre-modern” approaches –using Berelson’s terminology– and the modern existence of the field. How could this “pre-modern” period be defined?

Progressive thinkers, for example, were interested in communication processes as important elements of a well-functioning democracy. Jane Addams’s text on the dangers the theater poses to the moral education of youths is a good example. She did not assume communication issues as external processes that affected social dynamics, but as a constitutive part of them (Addams 1909). This also was the main assumption of the Chicago School in the 1920s. As Dewey points out, “of all our affairs, communication is the most wonderful” (Peters and Si-

monson, 35). Far from being a personal opinion produced by fascination with the modern media, Dewey's statement implies an ontological dimension. Communication is a symbolic production that "joins the gap between existence and essence" (Peters and Simonson, 36). In this sense, studying communication dynamics is equivalent to studying the processes of society's constitution and maintenance. This ontological premise implied a social-political commitment: understanding communication processes leads to social reform toward the ideal of democracy. World War I focused the attention of these communication studies on the propaganda issue. On this point, one can see a subtle epistemological shift. Authors such as Lippmann (1925) and Laswell (1927) posed the discussion in terms of *propaganda* instead of *communication* (Peters and Simonson 2004). This is not a complete change in the object of communication studies or in the assumptions about communication processes. However, this apparently insignificant shift could be central to Berelson's division of the field. The focus on propaganda implied the objective of communication studies was to present the effects of mass media messages on the public's thoughts and actions. It seems the World War focused the attention of social scientists, and government agencies and advertisers as well, on the way in which the media were changing modern life. The problem was not only to describe the effects, but also to determine their possible uses in order to strengthen democracy. In this sense, the so-called "propaganda analysis" retains some of the assumptions of progressivism and the Chicago School: communication studies were aimed at improving social conditions; yet, at the same time, they introduced a new emphasis on the object.

This subtle shift from a symbolic dimension of communication to concern over media effects defined the lines of study after World War I. This nuance seems to be the basis of

Berelson's field division. Both the Rockefeller Foundation Seminar and the studies on radio audiences, which are the main aspects highlighted in his definition of the field, focused on the same objective.

On the one hand, radio audience research concentrated on the possibility of determining the influence of the radio on the public's behavior. Berelson does not specify examples of these studies, but it would be possible to cite Lazarsfeld's work as director of the Princeton Office of Radio Research since 1937, or an earlier study, *The Psychology of Radio*, done by Hadley Cantril and Gordon Allport in 1935. Cantril and Allport maintain a certain progressivism in their idea that "any device that carries messages instantaneously and inexpensively to the farthest and most inaccessible regions of the earth, that penetrates all manner of social, political, and economic barriers, is by nature a powerful agent of democracy" (Peter and Simonson, 111). This concern about the possibility of democracy is developed in the determination of the radio's influence on people's daily habits and perceptions.

On the other hand, Berelson mentions the Rockefeller Foundation Seminar. Gary (1996) draws the reader's attention to the Rockefeller Foundation's official concern about the effects of propaganda on the democratic basis of society. "Concerned that the public was satisfied by a "pathology of substitutes" and manipulated by a "pathology of influence" instead of genuine knowledge, Marshall thought the Foundation should support inquiry into how these pathologies affected democratic processes" (Gary, 125). The Rockefeller Foundation Seminar was, therefore, focused on the problem of media effects. The media represented a threat of manipulation, but also a potential way to achieve processes of knowledge and common participation.

Thus, it seems clear the transition between a “pre-modern” period and the modern communication research field was based on a growing interest in the effects of communication processes, especially the influence of the media. However, the movement from the ontological symbolic dimension of communication to the problem of empirical effects is so subtle it sometimes is difficult to distinguish. Going back to Jane Addams’s *The Spirit of Youth and the City Streets* from 1909, it could be difficult to see the difference. Undoubtedly, her concern is about media effects, specifically how the theater affects in the moral education of young people. “Is it not astounding that a city allows thousands of its youth to fill their impressionable minds with these absurdities, which certainly will become the foundation for their working moral codes and the data from which they will judge the properties of life?” (Addams 1909, 27) This question points to a causal relationship between the movies and young people’s behavior that seems very close to the objectives and assumptions of the modern field of communication research.

The difference between studies such as the one by Addams and modern research could lie in a second consequence of the Rockefeller Foundation Seminar, one not mentioned directly by Berelson. Peters and Simonson highlight the importance of that seminar as a space for reflection about the field itself:

The Rockefeller Seminar played several important roles. It brought together a network of scholars who together shaped the mainstream of mass communications study through the 1940s, both in their own work and their training of graduate students. Just as important, it gave them a common conceptual focus by developing what Marshall called a “general theory” (Gary 1999, 100), but what more accurately was a research agenda. (Peters and Simonson, 88)

Thus, Berelson’s definition of the field seems to be related to these two main aspects: first, with a particular object of research; namely, the study of media effects on the public; and secondly, with a certain awareness of the field as such. The Rockefeller Seminar appears to have introduced the necessary distance to create the conceptual boundaries of a field.

This is the field Berelson perceived to be dying. One could object to his particular definition of it by showing he left out several approaches that could have reshaped his diagnosis. This was one of the arguments his critics posed in 1959. Actually, it would be possible to show several nuances between the authors grouped by Berelson to question the possibility of stating a unified definition of the field. I would like to use three cases.

The first concerns a difference in the *methods* within the same approach. If one compares some of the base assumptions of the Columbia School to Laswell’s first reflection about propaganda in 1927, the differences would be obvious. In fact, it would be enough to compare Laswell’s *Propaganda Technique in the World War* in 1927 to his 1949 work, *Why be Quantitative?* Berelson classifies Laswell’s research as a “documentary and content analysis” method. However, there are two different approaches used by the same author. The study Laswell’s describes in his 1927 text was entirely qualitative. Laswell himself calls it as an analysis that consists of “the discovery and illustration of propaganda themes and their use” (Laswell 1949, 40). In 1949, however, he separated himself from this methodological approach, pointing out its sources lack accuracy:

Can we assume that a scholar reads his sources with the same degree of care throughout his research? Did he allow his eye to travel over the thousands upon thousands of pages of parliamentary debates, newspaper, magazines and other sources listed in his bibliography or no-

tes? Or did he use a sampling system, scanning some pages superficially, though concentrating upon certain periods? (Laswell 1949, 42-43)

Here, Laswell highlights a methodological deficiency in his 1927 approach. He is calling for an exhaustive delimitation of data that provides research with precision on its object. This seems to be a minor nuance from Berelson's perspective, since he groups both of them into the same approach –a political one– probably privileging Laswell's disciplinary base, which was political science.

Let us conduct the opposite exercise and try to put together two authors who were divided by Berelson. What is Berelson's criterion for separating the approaches taken by Hovland and Lazarsfeld? It seems clear, for Berelson, that both of them share the same object. Hovland wrote about the persuasive consequences of educational movies, and Lazarsfeld was interested in the conditions that determined people's political decisions. Overall, both of them were interested in media effects. The methods used by each one seem to have been decisive to dividing their approaches. Berelson defines Lazarsfeld's approach by the use of sample surveys, and Hovland's perspective by the use of experimental methods from social psychology. Apparently, Berelson does not consider sample surveys to be an experimental method in communication research. True, Lazarsfeld was not the first to use sample surveys, but it seems as though he did not regard his method as a simple application of the traditional sample survey. He describes the method used in *The People's Choice* as a "new research method" (Lazarsfeld 1948, 3), one centered on the repetition of interviews to the same people. Actually, it was the main methodological turn introduced in his research that allowed him to present some conclusions that contradicted what, up till then, appeared to be a generally accepted idea: media had a

broad influence on people's political behavior. In changing the traditional sample survey method, Lazarsfeld showed media effects on people's political behavior were limited and indirect. Why did this variation of the traditional sample survey method not count as an experimental approach in Berelson's view?

A third "hypothetical" case is the discussion between Lippmann and Dewey. It shows an *ethical-political* difference within the field. Berelson does not mention these names and probably would consider them as part of the "pre-modern" background of the field. Yet, let us forget his temporal delimitation for a moment and consider solely the epistemological one. If it was true, as I mentioned earlier, that Berelson assumed the object of communication research is to determine the effects of media, Lippman and Dewey could have a place in this field. They both reflected on the effects of communication processes in the consolidation of democracy. However, while Lippmann argued on behalf of the need for a body of intellectual elite who would direct the interests of the masses with a democratic ideal, Dewey believed in a more participatory model of democracy. To Dewey, democracy must be constructed on the basis of a person's daily practices. He could not accept the idea of a leading elite, because the idea of democracy implies not only a political system, but also a particular organization of society. This divergence implies a different perspective on communication processes. Dewey affirmed a positive role for the press in the construction of democracy, insofar as it could serve as a vital link between government and civil society. Media could work as the basis of a public sphere in which people's participation would be guaranteed. Lippman also cites the role of the media as one of mediation between the government and the people, not to produce a sphere of common participation, but as a means used by the elite to lead the masses. How would Berelson have classified them?

These are only three possible comparisons to show the difficulty of combining different perspectives within the same field, but also the complexity of dividing them. It would be possible to say, defending Berelson's viewpoint, that all these differences are included in his categorization of the *diverse approaches* contained in the field. At the same time, the similarities are guaranteed by the *unity of the problems*. Nevertheless, if those differences are approaches *inside* a field, this implies there is something that preserves the field in spite of the differences in the approaches.

The issue is that ignoring these differences could imply a questionable dependence of the field on a supposed permanent object. Let us focus on the problem of the methods. In disregarding the differences in the diverse methods and privileging the unity of the object, one would assume the methods are external to that object and do not have the power to change it. Berelson could highlight a methodological distinction between Lazarsfeld and Hovland –and among them and all the other “major” and “minor” approaches– precisely to recognize that diversity, but the differences in approach were not enough to change his general frame of the field. From this perspective, the method could only shift the approach to the object, but not the object itself. This is probably a positivist heritage that determines Berelson's interpretation of the field and its processes of constitution.²

2 Durkheim, recognized as one of the founding fathers of sociology, is a good example of this positivistic relation between the field and its object. In 1895, he posed the same problem in the field of sociology. In *The Rules of Sociological Method*, he pointed out the need to start with the question about the object. From there, it would be possible to deduce a method for the emerging discipline. “Before beginning the search for [a] method appropriate to the study of social facts, it is important to know what are the facts termed ‘social’” (Durkheim 1982, 50). Durkheim thought the social facts existed themselves, but were mixed – for the observer – with psychological and biological phenomena. Thus, objects exist prior to the field of research and the methods within that field. In short, Durkheim assumes the object has an independent existence, but it must be delimited theoretically. With this delimitation, it is possible to define several methods for approaching the object. When these methods are applied, the field emerges as research.

The question here is whether or not it is genuinely possible to affirm the object exists *before* and *independent* of the field and its approaches. That is what Berelson seems to assume when he says “the subject matter or the problem triumphs over the approach and the method” (Berelson 1959, 441).³

However, a particular problem only becomes visible when there is a group of assumptions that allows us to see it. The object of the field only appears with the particular delimitation of the field itself. And, this particular delimitation depends on the epistemological assumptions about the problems, as well as the particular methods used to reveal those problems. A field is none other than the process to define its own object. And, as a process, it is always open. Schramm claimed “the ‘approach’ changes with the field,” but his words could be inverted; that is, the field changes with the approach. Being even more radical, one could say the object of the field changes with the field. Object and field emerge simultaneously.

On this issue, Martin Heidegger says – in his 1938 text entitled *The Age of the World-Picture* – the essential procedure of modern research is what he calls the “projected plan”. He uses the example of modern physics, saying natural events are its object. However, “every event, if it enters at all into the representation as a natural event, is determined, in advance, as a magnitude of spatio-temporal motion” (Heidegger 1938, 60). I would like to emphasize the expression “in advance,” because it shows what physics first must do is to define its own object. In other words, it must delineate the sector of reality to be identified as a “natural event”. Obviously, there is a considerable difference between physics and exact sciences, and human sciences and fields.

3 Berelson reinforces this affirmation when he mentions the existence of “communication problems *per se*” (444).

The human sciences, by contrast, indeed all the sciences that deal with living things, precisely in order to remain disciplined and rigorous, are necessarily inexact (...) The inexactness of the historical human sciences is not a deficiency, but rather the fulfillment of an essential requirement of this type of research. It is true, also, that the projecting and the securing of the domain of objects is (sic), in the historical sciences, not only different, but far more difficult to achieve than the rigor of the exact sciences (Heidegger, 60).

However, Heidegger does not say the “projected plan” is not used in the human sciences, but that it is far more difficult to determine. This *inexact procedure* to pre-determine the object of the science is what I would like to highlight in an effort to understand Berelson’s text.

All the aforementioned objections about the epistemological delimitation of the field could be correct and could show the limitations of Berelson’s reflections. However, did Berelson have another option? I do not want to defend *that* particular delimitation by trying to show its accuracy or correctness. I want to support the *practice* of delimitating a field.

It is possible to recognize, as Berelson calls it, the need to name a tradition; that is, to distinguish a historical background that makes it viable to organize the multiple paths that composed early communication research? The value of Berelson’s text, independent of its accuracy, is that it shows the need to compose a historical narration –in this case, a schematic one– that allows researchers to recognize their place in the present. Berelson joined a broad tendency among thinkers to shape the borders of the communication research field. His “death declaration” for the field is a radical rhetorical movement that tries to reframe that object called ‘communication’. It is more than just a motivational sentence intended to stimulate the creation of new

approaches *inside* the field. I would like to read Berelson’s text as evidence of the possibility of a *new* field and, consequently, *new* objects. Berelson was asking how to define a communicational phenomenon in his own time. The World Wars had ended and the attention on propaganda and its persuasive effects had dispersed. Once again, it was necessary to create a distance from the field, as its “modern fathers” did during the 1930s to create this novelty. “New” does not mean there something completely different, but something that responds to its own context, as the modern field did with its context. Perhaps it is necessary to declare the death of the field from time to time in order to re-create the field itself and its own object.

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