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Editorial

The problem of finding a problem to call your own

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When you enter the academic environment, the word *problem* changes its meaning in people's lives. In everyday life, we see a problem as something to be avoided, but in the academic environment a problem is something to be identified and solved. Research activity must lead to this result: a complete solution, a partial solution, or even a step towards a solution. That's what researchers are for. The solution to a problem should generate new knowledge, and this contribution should benefit an entire community, or at least a segment of it. The problem is the most important element in the development of a research project (Saunders et al., 2019). It sounds simple and even obvious, but it isn't.

Particularly in the field of applied social sciences, and more specifically in the business environment, the possibilities are endless, as are the possible ontologies and epistemologies. On the one hand, this creates enormous fertility; on the other, it creates great difficulty in understanding, advancing, and consolidating new knowledge. We hope that the new knowledge will eventually have an impact on changes in the community.

Accepting the principle that knowledge should benefit someone is a starting point to be respected. A new technique, a new model, a different way of looking at things, a solution that hasn't been presented before, for example, are still some of the possible contributions to the main final beneficiaries, those on whom we focus our impact. They are public and private organizations. After all, this is what is expected of applied social science research. Dealing with a topic that the final beneficiary considers to have already been solved may be potentially attractive due to the ease and abundance of knowledge available, but in terms of advancing knowledge, it may be innocuous and sterile. Relevance has a variable shelf life and intensity over time. It arises and changes in context. Those who don't recognize this will find it difficult to develop relevant knowledge.

The final beneficiary would be an organization implementing a new management model, for example, while the intermediate beneficiary would be the one who identifies or improves the knowledge so that the final beneficiary can benefit from it, even if it is not operationalized. When someone questions the fact that many researchers only publish for other researchers, before condemning them, I recommend that they assess whether the stage of the new knowledge still requires a bridge that can be provided by someone who is strongly rooted in organizations and, therefore, in a position to ensure that the knowledge reaches the final beneficiary. The big problem doesn't lie in the intermediate beneficiary, the one who translates to the final beneficiary, but in the suitability of the problem chosen, which some would call the quality of the problem chosen to work on.

Incidentally, there are other areas of knowledge in which this is exactly the way to reach the final beneficiary: researchers doing research for other researchers, so that the solution to the problem, if relevant, can be treated within a segmented and consistent line of thought until it reaches those who will actually use the new knowledge. Perhaps in business we are so lacking in being perceived as innovators that we don't see the future potential of a consolidated line of thought, the result of the accumulation of knowledge, which requires a long-term perspective, continuity, and not just opportunity from different actors.

When we compare our publications with other fields of knowledge, we find a wide range in terms of ontological and epistemological diversity and methodological design, which makes it difficult to get an applied social science article published in a short space of time. At the same time as we have papers with a high perception of timelessness, there are others with rapid obsolescence. Paradoxically, the community tends to question the fairness/legitimity of publication in a very short time frame, and is unlikely to attribute the time frame to the efficiency of the journal.

The review system is supposed to create reliability for publications, and this is left in the hands of countless people who are difficult to coordinate. Either we completely change the research and communication model – which is really complicated, even in the ChatGPT world – or we'll have to become satisfied with small reductions in time frames, which are always possible with segmentation. What does that have to do with the problem? As time passes, the need for a solution becomes obsolete or the solution becomes less effective. Either we reach organizations in a timely and intelligible way, or they will not need us researchers to develop. I think they do, by the way, and very much so.

If we don't meet the needs of organizations on various issues, how are they surviving? Consultancies could answer that. They are pragmatic, they divide problems into slots and manage them, and they are recognized and very well paid. After all, organizations don't demand as much methodological precision as we do from an academic perspective. *E la nave va*. Do we have anything to learn from consultancies? Of course, starting with the way they establish a relationship of trust with their clients and the points of analysis that will be adequately addressed, and the way they collaborate to ensure that the problem to be addressed is relevant to their client and feasible to solve within the agreed conditions. On the other hand, methodological approaches are absolutely pragmatic and rarely questioned, and if the proposal works, the problem is solved. What matters to the consultant is the result perceived by the client, sliced up job by job.

Do we just want to be cited, or do we want the knowledge to be applied? If we only want the first choice, this text won't help. If you recognize that there is a huge opportunity on the second axis, let's include a type of researcher that we often ignore. Does anyone imagine that a CEO or CFO is going to spend time reading a 15-page article to deepen their knowledge and then make a deployment decision? Does anyone think they'll dedicate themselves to learning things they have no idea they'll be able to use? Who will they blame if things go wrong (pardon the pragmatic Machiavellianism)? Does anyone believe that they should understand our Cartesian methodology and arguments?

Someone has to do the **translation** from the field to academia and back from academia to the field. Understanding the problem correctly, according to the logic of the field, is the beginning. Returning the new knowledge from academia to the field is a relevant stage, and it's the one that people have been criticizing, not realizing that the difficulty started much earlier. You can't support one side without the other. I recognize that there are researchers who do well on both sides, but that is not always the case.

At this point I imagine that an important agent, a special kind of relevant researcher who has their own methods, is the consultant who can translate the knowledge to their clients. They won't cite our work, but they will apply it if we know how to communicate and make them partners. What happens is that they gain, to varying degrees, the client's trust to have the space to propose solutions. This is very true for contact and even for obtaining reliable data to see the knowledge being used effectively.

This is where the question about researchers publishing for other researchers becomes interesting. Are there alternative solutions for some colleagues? Bringing the academic field closer to organizations is something that has been tried for a long time and, I admit, successfully by some teaching and research institutions; in some cases much more by focusing on personal relationships than institutional ones, but this is a fundamental way of valuing research.

I spent time with executives at a company whose CEO posted a message in the salesperson's lounge: *THE CUSTOMER ISN'T HERE*. That's about it, dear researchers: THE PROBLEM THAT WILL GET YOU RECOGNITION AND GLORY is... in the organizations, and you need to know how to identify, translate and model it, with or without partnership. Is there a magic formula for finding a good problem? It's not magic, but it is hard work, studying the segment and the type of organization where you want to spend your time. Without a relevant problem that has a possible solution, the research may not be efficiently anchored in the organizational ecosystem at any given time and will be a huge waste of time and resources. Identifying and structuring a relevant problem takes time, but it's worth it.

Of course, it is fundamental to delve into what has already been studied on the topic, and once a relevant problem is chosen, mapping out what is already known is key to identifying the gap, which is the decisive point for evaluating the development of research. In other words, what is not known about the problem? To do this, the framework must provide a construct that allows us to look at the empirical question and analyze it in a structured way in the country's environment and beyond.

The logic of the ecosystem can be adapted to the choice of problem. At some point, it will provide the definition of the research question to guide and delimit the problem (Cooper & Schindler, 2001). Borrowing the logic of biology, an **ecosystem** consists of a community of organisms together with their physical environment. Thus, there is an interrelationship between these organisms, and the more the identified problem has a broad impact on the ecosystem, the more relevant its solution will be. From this perspective of comprehensiveness, some suggestions for improving the way problems are dealt with can be offered:

1. Closer prior contact with organizations, taking into account the segmentation of mutual interest and prospects for contribution. This suggestion is necessary for the second, and without it, we'll continue to be out of touch with the demands for relevant knowledge in terms of usefulness and delivery time. On the side of the organizations, there are cases where they call us to discuss problems that they sometimes can't measure the magnitude of themselves.
2. Creation of a course that would provide those entering postgraduate programs with an understanding, identification, choice, and design of problem-oriented research. A course, even if it is relatively short compared to traditional ones, would have a format in which representatives of public and private organizations interact with students under the coordination of professors. A workshop format, field visits as a starting point, and interactive discussions with the aim of defining a problem in a given context. Doesn't this happen in methodology courses? Probably not with the intensity proposed, because all methodological development should be linked to the problem, but this view doesn't always prevail.
3. Bringing researchers closer to the intermediate beneficiaries – in this case, not exclusively represented by consultants – and paying attention to perceptions of need and urgency. Learning from this interaction can generate new opportunities for relevant problems, with feasible solutions and great impact.
4. Expose and validate the problems identified in different segments and/or publics, with the aim of perceiving the possible impact of their total or partial solution. Validating a good problem can take some time, but it's worth it to make the research process more assertive and to assess the scope of the impact.
5. View research as an activity that requires both individual and collective attitudes. This can be done in a variety of ways to bring people and groups together, but one practical way would be the "problem fair," where people offer problems to identify groups that want to work on developing research. Moving from the individual to the group environment requires confidence, maturity, willingness, flexibility, and resilience on the part of the researchers, but it brings benefits in terms of obtaining different talents in the management of the research project and possibly shortening time frames.
6. When choosing problems, we can identify one that can be broken down appropriately, without it being a "salami" technique, but by managing a longitudinal view in which a portfolio of viable problems is possible based on maturity, an evolutionary view, and with that can be treated in a segmented way. It could be interesting to increase the ability to assess whether the problem is very suitable or whether it's something that can be solved and doesn't require continuity. Again, the long term adds value to the problem and its treatment as a research project.

The knowledge provided by a relevant problem with an evident gap, innovative contribution, and perceived impact is what makes researchers useful to the ecosystem of organizations. I hope everyone finds some good problems to call their own.

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