

## New Challenges for Fieldworkers in Latin American and Caribbean Studies

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### Abstract

This essay aims to stimulate debate by proposing that the imposition of ill-conceived research protocols by universities and grant giving institutions not only blocks the kind of personal contact between researchers and subjects that makes for insightful findings, but actually imperils the researchers themselves. The protocols are meant to protect research subjects from harm, but this goal is a far more complex and difficult undertaking than those imposing these protocols seem to grasp. In particular, ethics administrators' focus on obtaining 'informed consent' underscores their lack of direct experience with the full process of research and dissemination of findings because the decisive point that bears on the safety and happiness of research subjects is not the moment when these people accept the fieldworker's invitation to share their thoughts and experiences. Rather, it is the inevitably difficult decisions made by researchers regarding how much of the material they have collected can actually be published. *Keywords:* fieldwork, risk to researchers, risks to research subjects, ethics protocols, informed consent, universities' liability.

Resumen: Nuevos desafíos para investigadores de campo en el estudio de América Latina y del Caribe

Este ensayo argumenta que las instituciones universitarias y de financiamiento académico imponen protocolos de investigación inadecuados que no sólo impiden el tipo de contacto entre investigadores y sujetos que generan resultados interesantes, sino que además ponen en peligro a los mismos investigadores. Los protocolos están diseñados para proteger a los sujetos de investigación de cualquier peligro, pero aquel objetivo es mucho más complejo y difícil realizar de lo que los administrando los protocolos aparentemente entienden. La importancia que los administradores de la ética académica le otorgan al 'consentimiento informado' pone en evidencia su falta de experiencia directa con la totalidad del proceso de investigación y de difusión de resultados. Lo que realmente determina la seguridad y el nivel de felicidad del sujeto de investigación no es el momento en que este acepta ser parte de la investigación, sino que son las siempre difícil decisiones que toma el investigador al decidir que material conviene, o no, publicar. *Palabras clave:* trabajo de campo, riesgo a investigadores, riesgo a sujetos de investigación, protocolos de ética de investigación, consentimiento informado, responsabilidad civil de la universidad.

In September 1958, on the first day of the first class of First Year Spanish at South Side Middle School, *Maestro* Feldman addressed us, hoping to inspire his students. ‘Think of this,’ he said,

There are more than 300 million Spanish speakers in the world, and if you apply yourself in this class, you will find 300 million people out there you can pal around with. Of course, a few of them you may not like. But any of the others could become your friends once you can introduce yourself and talk to them.

Today there are more than half a billion Spanish speakers and another 200 million Brazilians available to become friends – as my middle school teacher would have it – with anyone who is fortunate enough to grow up speaking Portuguese or Spanish or has the chance to study these languages.<sup>1</sup> Of course, communication with indigenous people in the region has always required an even more serious commitment to language acquisition than learning Spanish, Portuguese, French, or Dutch. But at the core of any of these efforts is the idea that the study of Latin America and the Caribbean requires getting out and actually talking to people, that is, coming to know the region through face-to-face contact.

For anyone who accepts this simple proposition, it might seem that we are better equipped than ever to realize all the advantages of direct personal connection with Latin American and Caribbean people. The great advances in communication and transportation networks in the region should make it easier to set up the kind of direct encounters that ideally underpin scholarly inquiry of any kind. Indeed we might imagine that with these improvements, it has become less daunting to conduct the kind of fieldwork that allows students of Latin American and Caribbean societies to gain direct access to potential research subjects at every socio-economic level and in every corner of the Americas.

Sadly, it is not the case that human contact is easier to establish than ever before, not the least because ‘the field’ has, arguably, become a more dangerous place as, for example, when students of urban Brazil find that the favelas in Rio that relatively recently provided a rich setting for the study of grass roots mobilizations have come under the control of drug lords and gangs that have co-opted, expelled or murdered the leading social movement activists.<sup>2</sup> If we also consider the use of police and army to quell unrest in the lead-up to the World Cup and the 2016 Olympics, and the persistence of ‘death squads’ active in ‘cleansing’ slums, we must regard with amazement and respect any researcher intrepid enough to study the affected communities.

As a peace process appears to be taking hold in Colombia, can we project a point when researchers will be able to move about more safely in the western part of the country where the FARC (Revolutionary Armed Forces of Colombia) and the right wing paramilitary forces have contested control for decades. At the same time, however, towns large and small in Honduras have become so

violent as to be unlivable for their own inhabitants, not to say ‘insecure’ as a research site.

Likewise, fieldwork in both rural and urban Mexico always presented risks, but nothing close to the perils that now confront those researchers who venture into the many regions where rival drug cartels currently vie for territorial control. Back in the early days of my own fieldwork in Mexico in the 1960s and 70s, thieves so feared the horrific consequences of being taken into police custody that they overwhelmingly limited their activities to ‘crimes of stealth’: deftly picking pockets, gently slicing open bags and briefcases on crowded buses, in short, employing selective techniques designed to avoid any direct confrontation between the perpetrator and the victim who, ideally, would not realize for hours that any valuables were missing. Mexican friends would sometimes boast: ‘Our *ladrones* are so light-fingered they can steal your sox without taking off your shoes!’

Today, of course, there is a *sequestro* for every pocketbook: from industrialists whose banker holds a million dollars at the ready to pay out in the event of an ‘express kidnapping’, down to the Mexican migrants at the prep table in the basement of a Manhattan deli who feel obliged to chip in their entire week’s wage to ransom the mother of the co-worker whose generous remittances made his family the target of the local kidnap gang. But the most terrifying forms of violence, from the point of view of Mexicans and foreigners alike, are more random and less disciplined. These manifest as what by now come to well over a hundred thousand savagely mutilated dead who are both the intended and unintended victims of drug cartel activity, with the hideous death of 43 student teachers in Ayotzinapa, Guerrero, only the most widely known of recent massacres.

Unfortunately, the physical insecurity of field sites is not the only factor that discourages would-be field workers. However shocking it may seem to speak of our universities and research councils on the same page as the dangers posed by paramilitary operatives, police and military forces, drug cartels, and street gangs, also working against our chances of carrying out meaningful forms of inquiry are our own academic institutions.

With this essay I would hope to stimulate debate by proposing that the very institutions that should be supporting our fieldwork and our efforts to understand the Americas through direct interactions with people of the region not only block the kind of contact that makes for insightful research findings, but actually *imperl* the researchers themselves. Moreover, even as the authors of these protocols purport to protect research subjects from harm, this desirable goal is a far more complex and difficult undertaking than those who impose these protocols on researchers seem to grasp.

I will show that the ethics administrators’ laser-like focus on obtaining ‘informed consent’ (preferably in writing) underscores their lack of direct experience with the *full* process of research and scholarly dissemination of findings. If they, themselves, had carried out fieldwork and *written up and published*

their findings, they would understand that the decisive point that bears on the safety and happiness of research subjects is not the moment when these people accept the fieldworker's invitation to share their thoughts and experiences. Rather, it is the long, often painful and always difficult decisions made by researchers regarding how much of the material they have collected can actually be published. The key questions with which fieldworkers must grapple centre on how much they can report without revealing the identity of the source and placing that person in danger. They also centre on what they can include in their writings without destroying relationships among family members, neighbours, and communities, and precipitating the ruptures that come about when the author of a monograph is not mindful of how much 'the truth can hurt'.

### **The unspeakable / unspoken truth**

The imperative to write this article comes in large part from the understandable reluctance most researchers feel to set out in print what they commonly speak very freely about with colleagues. In private discussion, we often note the huge gap between the commitments we are required to give to research institutions and the practices we necessarily adopt in the field. Often these exchanges are accompanied by much shaking of heads when we consider what would be our prospects of returning from the field with useful material, or, indeed, returning at all, if we were to follow faithfully the practices imposed by the research protocols.

In my own work on student, peasant and urban popular movements in Mexico, street vendors in Latin America and Europe, and the survival strategies of national and transnational migrants, like the readers of this essay, I have necessarily reflected on the ethical questions above all, but not exclusively, in the study of people who are vulnerable. At the end of my most recent fieldwork project, I decided to go beyond the usual acknowledgments of my debt to others to offer readers a methodology section in which I explored the impossibility of remaining within the official guidelines set out by ethics committees. I wanted to reflect *publicly* on the profound contradictions between what we are obliged to say we will do (and most terrifying, what we are formally required to tell students that they must do), and what is possible – *and desirable* – to do once we actually find ourselves in the field (Hellman, 2008).

What is it that we are required to do? The rules are generally spelled out by the ethics committee of a researcher's own university, a body on which I, myself, served several terms back in the 1980s. In general, guidelines are derived from the rules set out by a university's ethics committee, which may also be referred to as an institutional review board, an ethical review board or research ethics board.

In practice, as most of us are quick to notice, the protocols established by the ethics boards correspond more closely to the challenges that arise in clinical experimentation where the researcher expects to be in control of the 'exper-

iment' rather than to the kind of open-ended situation in which the field of study is the real world. These rules require the researcher to produce an 'informed consent' document *before* opening a conversation with any 'research subject' whose words could possibly become part of the findings of the study. Generally, the form is supposed to spell out the purpose of the project, the hypotheses, goals and objectives of the study, the procedures to be employed, the possible risks to participants, the research subject's right to remain anonymous, to decline to participate or to withdraw from the experiment or study at any point, and the direct future benefits, if any, for the participant in the study. Overall these guidelines may be applicable to physiologists wiring up experimental subjects with electrodes, or for social psychologists running a simulation that explores the relationship between prisoners and guards in a psychology lab, or in other settings in which the 'experimenter' has a great deal of control over the experimental situation. (Hellman, 2008, p. 233).

Other sections of universities' ethics codes may come closer to the fieldwork experience of researchers in Latin America and the Caribbean insofar as they actually anticipate the special ethical questions that arise for anthropologists in the field. However, generally the picture of fieldwork to which these rules correspond is that of a small-scale society, and often the guidelines presuppose that permission to study the entire population in an indigenous community will be obtained from the 'elders' or even from the chief!<sup>3</sup>

Yet another problem, noted by Gareth A. Jones (2015), is that 'the terminologies and social relations of ethics review are culturally bounded'.

... there is an idea that 'consent' is a singular, identifiable, and common term. Yet in practice, the notion of consent in the context of the legal-meaning role is highly contextual to the social role. Consent as an action implies respect and reciprocity. [However,] that will mean different things in the streets of the UK or Canada compared with Bolivia or Brazil; it may [also] be understood differently across class, ethnicity, gender, and age.

In summary, a major obstacle to establishing a fruitful, direct encounter with the people of the region is not the lack of funding available for fieldwork in LACS, much less a shortage of researchers, both seasoned and new to the subject, who are eager to carry out field projects. Rather it is the confusing, impracticable dictates of research protocols that have a dampening effect on the prospects of young researchers who, like those of us who were fortunate enough to begin our work decades ago, appropriately want to take their turn in the field.

Moreover, the most worrying aspect of the heavy hand of academic bureaucrats over researchers is that any careful parsing of the specific demands made on those who are preparing to go off to the field reveals that universities are primarily concerned with their own 'exposure' to liability rather than with the welfare of the research subjects, let alone the safety of the researchers themselves. Indeed, as I will argue below, if those enforcing the protocols were

concerned with the welfare of researchers, they would not insist that fieldworkers approach potential research subjects in a manner likely to create distrust and even hostility rather than good will.

### **In practice**

Fortunately, in the course of the first few days of fieldwork, even the most fresh-faced, inexperienced neo-fieldworker comes to see the absurdity of the practices that the protocols call upon researchers to adopt. These absurdities begin, but by no means end, with the idea that we may not open a conversation with anyone who *might* turn out to be an informant without announcing to this hapless stranger – conceivably a seatmate in a jitney or a bus or the proprietor of the local *bodega* – who we are, the nature and purpose of the study we are conducting, the hypotheses, goals and objectives of the work, possible risks to participants, and all the rest. It does, however, become clear within days, if not within hours, (possibly beginning at the airport, or on the first taxi ride into the centre city, or in speaking with the servant who opens the gate at the guest-house), that we cannot go about Tegucigalpa, or Port au Prince, or Guayaquil, or Kingston conducting ourselves in the bizarre manner that the ethics bureaucrats have made us promise we would.

This problem is by no means relevant only to the field settings of anthropologists, who are often the first scholars who come to mind in a discussion of limitations imposed on fieldwork experiences. It is equally true for anyone in any disciplinary or, increasingly, interdisciplinary programme in the social sciences who ventures forth with a project that involves on-site interviews, surveys, observation, participant observation or any of the other standard techniques that allow us to provide first-hand accounts of everyday life.

Why, then, do we find ourselves in the position of having to promise in writing that we will comport ourselves in ways that make no sense at all in settings in which we are the ‘strangers’? As in legal ‘case law’, almost every extreme demand made by the ethics approval procedures has its origin in a case, generally a severely exploitative act or series of acts committed by a researcher or research team that are widely considered to have injured poor and powerless people – an outcome that no one in any research field would want to see repeated. It is these cases that give rise to the protocols worked out by the American Anthropological Association and all the other major scholarly associations, grant-giving organizations, and national research councils.<sup>4</sup>

In practice, however, much of the responsibility for mandating inappropriate behaviour (like breaking into a description of the project and going on about one’s hypotheses to people who have yet to utter a word beyond ‘*buenos días*’) comes about because those in charge of imposing these rules are often permanent university administrators. Whatever their other skills may be, many of these administrators come to their positions with no experience of conducting fieldwork even in their own society, let alone in ‘strange foreign lands’. In

short, they rarely, if ever, have direct personal experience with working cross-culturally in face to face encounters *in a research site*, as distinguished from multicultural encounters that unfold in the safety of their own society which, in Canada, would be an everyday experience for anyone in university administration.

To be sure, the university bureaucrats do consult with academic experts in formulating protocols and at other stages of the review process. But enforcement of the policies that deliver or deny grant money to researchers remains in the hands of administrative staff who are generally without direct experience of the field. Indeed, the clearest evidence of their lack of any sense of the circumstances in which fieldworkers might find themselves or the kind of people researchers are likely to encounter in the Ecuadorian Amazon or rural Nicaragua is the stipulation, among so many others, that the Informed Consent document must include the email contact of the administrator of the university research office so that a confused or reluctant research subject (presumably during office hours) might communicate with a university representative to report any doubts or complaints about the research project or the researcher!

Most worrying of all is that the permanent research office staff who are in charge of disseminating the protocols to graduate students are drawn from this same pool. At the many sessions I have attended that are designed to instruct the would-be graduate student fieldworkers on the ethics requirements, the research administration officer's approach to the students is very cheerful and friendly and inevitably includes this pitch: 'We know that all these rules are going to sound like a lot of unnecessary fuss and bother. But they are important because they *sensitize* you to the real challenges of fieldwork.'

Unfortunately, this orientation session 'sensitizes' the students to the wrong 'challenge'. As I have argued above, the key question is *not* whether potential research subjects agree to talk with us and end up telling us things. Sooner or later we all find people in the field who will talk to us and tell us things. The sensitive issue is what we do with what we collect and how we identify material that would be best *not* to publish. In considering this task, it is worth remembering that the researcher's crucial decisions on what *not* to publish take place within a context of the tremendous pressure exerted by the very same university administrations and national research councils to publish a great deal and at a rapid pace.

Even as we are subject to these pressures, we also live with the troubling reality that we might hope to disguise the identities of research subjects from the 'authorities', along with the real names and locations of barrios and villages where these people live. However, we cannot deceive ourselves that we can always disguise our informants' identities from their extended families, their neighbours, their ex-spouse and others close to them who may take great exception to the way they are portrayed in our published work. In reality, the likelihood that informants may incur the anger and lasting enmity of people who are important to them now and in their future lives is greater than ever

when, even in the most remote settings, many returned migrants are available to read aloud from our writings, translating or paraphrasing as they go along.<sup>5</sup>

### **The peril to researchers**

I have argued above that the protocols are written and implemented in a way that suggests that the university is more worried about its own exposure to lawsuits brought by informants who feel ill-used than about the researchers themselves and the integrity of their work. One of the many ways in which our research may be compromised by the protocols is that by insisting that we open any interaction with potential subjects with disclosure of the objectives of the project, the protocols create a situation in which an obliging informant may be led to tell us what we have inadvertently indicated we hope to hear.

However, beyond the methodological problem of generating unreliable data by 'leading' respondents in this manner, there are more grave dangers to be considered. With the protocols it puts in place thinking to protect itself from liability, the university not only sets us up for failure in the field by undermining the interview process or insisting that we conduct ourselves in ways that are culturally inappropriate. More chilling is the way in which the protocols may directly endanger researchers.

A telling example of the potentially lethal outcome of following the protocols to the letter is provided by the research experience of a former student in our graduate programme in development studies. In her work, Catalina Ponce de Leon (2011) focused on the mobilizations of indigenous people against the incursions of large-scale mining companies in the Ecuadorian Amazon where foreign-owned extractive industrial ventures became the flash point in social and environmental conflicts. In this region, home to the Shuar people of south-east Ecuador, the inflow of outsiders following the discovery of oil in 1967 exacerbated longstanding grievances over indigenous peoples' lack of secure access to land and water. It also intensified their claims to self-determination and control over their territory as well as recognition of their rights to engage in small-scale exploitation of the same resources sought by foreign corporations.

This excellent study covered many issues: notions of citizenship, collective vs. individual rights, the role of the state, links between cultural identity and the natural environment, and much else. However, in my view, the most interesting aspect of the project was one that challenged the 'idealization of indigenous resistance as an expression of harmony and unity' (Ponce de Leon, 2011, p. 8). From the start, the researcher recognized that while the majority of indigenous people in the region chose a path of protest and resistance, there were outliers who, in spite of intense pressure to conform and join the movement, nonetheless viewed the coming of foreign extractive companies as an opportunity to improve their condition. They accepted the mining companies' position that extraction would bring jobs and schools and clinics and all manner of good things.

Many of these impoverished people who refused to join the resistance movement lived in mixed indigenous and mestizo communities. At the same time, some Shuar communities also were won over by the mining companies' appeal. Their attraction to pro-mining organizations created bitter divisions among the Shuar. These divisions gave rise to the growth of 'rogue federations' and 'shadow associations' of parallel organizations led by Shuar who openly supported the extractive project such as the Shuar Federation of Zamora Chinchipe, which represented 18 different communities.

The original fieldwork proposal called for Catalina to explore the dynamics of neighbouring communities that came down on either side of this divide. Her thesis committee, of which I was a member, became enthused at the prospect of a comparative exercise examining matched pairs of communities, an approach that could reveal how and why indigenous people of similar background and experience would arrive at such starkly different positions on mining.

Fortunately, once in the field, as she began to get a feel for the environment in which she was working, she realized that the enmity felt by the Shuar who engaged in the resistance movements toward neighbours whom they saw as selling out to the mining interests was so intense that it would be ill advised to share with the former any plans to interview the latter.

Moreover, soon Catalina heard rumours that some people who refused to embrace the resistance cause had been tortured or assassinated. Under the circumstances, she decided that whatever the research protocols call for with respect to explicitly explaining to each research subject the precise goals and hypotheses of her project, it was *not* a good idea to run around telling everyone every detail of all she planned to study as a way of introducing herself to potential research subjects. As she recently wrote to me, reflecting on this experience:

I mostly visited communities that were predominantly against mining where there were limited opportunities for dissent for those who held a different opinion. In one case, I distinctly and definitely was told a story that 'so and so', who was in favor of the [mining] project in that community, was doused with gasoline. It was implied that he was then set on fire but I could not verify that.

In this community someone [affiliated] with the pro-protest 'good guys' successfully vouched for me, and so I was allowed to stay and my safety was probably ensured after that point. The mysterious deaths were not quite so mysterious when you consider that particular story about the gasoline; I cannot clearly recall, to be honest, specific accounts of actual disappearances.... In the years since, I know that the bodies of Shuar activists have turned up in rivers and ditches with signs of torture. There is no way to prove that their torture was related to their work against large-scale mining projects. But that's just the point. It's a violent setting in which someone from outside may never really sort out all that is going on and it seems the

most shocking details – like the gasoline dousing – are the ones that have stayed quite fresh in my mind.

The best advice I was given before leaving for the field was to use common sense; it went a long way in helping me establish rapport with people on whom I depended, not just for interviews, but survival with respect to lodging, food, and the sort of situations where someone has to intercede on your behalf (Ponce de Leon, 2015).

The key point is that in a setting that, by definition, we do not fully understand upon arrival, the requirements of the research protocols seriously compromise our capacity to move around quietly and discreetly while building a better appreciation of the situation in which we find ourselves as we work to construct a network of reliable people who will support us. Most important, to the extent that these rules demand a great deal of disclosure on our part to people with whom we have yet to develop any kind of relationship of trust, the protocols, if followed to the letter, can endanger the researcher.

## Conclusions

These reflections focus on the way that the human subjects review protocols have had the unintended consequence of stifling exploratory research and redirecting intellectual inquiry from areas in which the researcher is likely to encounter human reality on the ground to high level theoretical exercises that no longer take place ‘in the field’. In the case of many graduate students, the zeal with which the university research office addresses them on their responsibilities has left them so uneasy about the field experience that the students come to fear that they may, at every turn, inflict harm on powerless people. This concern often becomes a major disincentive to building fieldwork into a research proposal.<sup>6</sup> Thus, we have the paradox that even as more students are prepared through their language acquisition and their concentration on LACS to connect in a direct and deeply committed way with people in the region, the whole matter of gaining permission to move ahead with fieldwork has become so fraught that many give up their dream of travel to Latin America and the Caribbean and settle for so much less – all of which, naturally, impacts on what we can expect as future research and findings in our field.

The great irony is that once we are in the field, we are all on the honour system, attempting, as best we can, to behave ethically and do no harm. For better *and* worse, no one back at the university – not our thesis committee, and certainly not the research office – can know much about our field experience on a day to day basis. Hopefully, we are too deeply immersed in the field setting and too busy with the work at hand and the people with whom we are forging the personal connections that are at the heart of fieldwork to be frenetically texting people back home as we might in the wired world. In the case of researchers who are empathetic, light on their feet, and able to exercise com-

mon sense, enormously gratifying and productive fieldwork can still be carried on in spite of the weight of bureaucracy and ill-conceived protocols. But undoubtedly there are pressures on the researcher that did not exist in the past, while, at the same time, there is no clear evidence that poor or powerless people are better protected because of the protocols, and certainly there is no clear evidence that the research we carry out is better because these rules are in place.

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## Notes

1. The figure provided in <http://spanishlanguagedomains.com/the-numbers-of-Spanish-speakers-in-the-world-exceeds-500-million/> is '517,423,452 million who speak Spanish including those who speak it as their second language'.
2. Compare Robert Gay's (1994) book with the one that followed (2005).
3. In this regard, it is very telling to read the *Tri-Council Policy Statement: Ethical Conduct for Research Involving Humans*, TCPS 2, 2014, especially Chapter 9, 'Research Involving the First Nations, Inuit and Métis Peoples of Canada', pp. 109-137, which explores the difficult question of submission to 'governing authorities' by researchers who have come to indigenous communities to study subordinate and outcast groups.
4. The National Science Foundation, the Economic and Social Research Council, the Netherlands Organization for Scientific Research, the German Research Foundation, the Canadian TriCouncil, to name only a few.
5. See Hellman, (2008, p. 236-9) for more detail on this question and a discussion of how times have changed since Margaret Mead could blithely – if not unreasonably – assume that her research subjects would never come to know what she had written about them. To hear directly from Mead's research subjects on their reactions to her publications, an excellent source is Barbara Gullahorn-Holocek's (1983) documentary film.
6. These concerns, in turn, are fed by various strains within the 'positionality' literature which posit that self-examination and self-disclosure are essential. When we consider

the demands imposed by the imperative to *disclose* one's position, it is hard to see how a research subject would hope to get a word in edgewise by the time the researchers deliver the requisite information about the study along with all the details of their background and personal experiences that have brought them to the position that they hold.

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