

TASO'S LIFE: PERSON AND COMMUNITY¹

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ABSTRACT

Oral and life history, two subfields of historical inquiry, have proved their usefulness, and merit wider acceptance. As part of that claim, the paper argues that Taso Zaya's life history betokened a significant broadening in the scope of anthropological inquiry. Together with *The People of Puerto Rico*, Taso's story marked anthropology's readiness to deal with all forms of cultural variety, rather than solely with the peoples called "primitive."

Keywords: oral history, anthropology, Puerto Rico, plantations, sugar, community

RESUMEN

La historia oral y la historia de vida, dos subcampos en la investigación histórica, han probado su utilidad, y ameritan mayor aceptación. Como parte de este reclamo, el artículo discute que la historia de vida de Taso Zayas auguró una ampliación significativa en el campo de la investigación antropológica. Junto con *The People of Puerto Rico*, la historia de Taso marcó la disposición de la antropología para tratar con todas las formas de variedad cultural, más que tratar únicamente con la gente llamada "primitiva".

Palabras clave: historia oral, antropología, Puerto Rico, plantaciones, azúcar, comunidad

RÉSUMÉ

L'histoire orale et l'histoire de la vie, sont deux aspects de la recherche historique qui ont corroboré leur importance et engagent un plaidoyer pour une meilleure acceptation. De fait, l'article démontre que l'histoire de la vie de Taso Zayas a illustré une extension significative dans la recherche anthropologique. Avec l'ouvrage *The People of Puerto Rico*, l'histoire de Taso a marqué la disposition de l'anthropologie envers différentes formes de variété culturelle, au lieu d'analyser uniquement les gens appelés « primitif ».

Mots-clés : histoire orale, anthropologie, Porto Rico, plantation, sucre, communauté

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I want to situate the story of my friend, Anastacio (Taso) Zayas, within a discussion of two somewhat unconventional ways of writing history. These methods of capturing the past—historical subfields of a sort—are called oral history and life history. They are considered marginal by many historians. I do not share that opinion.²

In defense of oral history, I offer the following examples. The Works Progress Administration (WPA) was a government agency created during the first term of Franklin D. Roosevelt (1935) to cope with the massive unemployment of the Depression years. The WPA hired many people, some of whom were instructed to conduct interviews and to produce oral histories. Among their tasks, these workers were to locate living Americans who had been born into slavery and were still surviving at that time, and to gather from them *verbatim* testimonies about their personal experiences, growing up as slaves in the United States.³ Over time, those oral histories turned out not only not to be marginal to the history of North American slavery, but at the very center of the tremendous change in the study of slavery that took shape in the post-Vietnam years. Until that time, it would not have been inaccurate to say that popular understanding of what slavery had been like in the U.S.A., at least among its white citizens, was based largely upon a farcical and tragically misleading novel, and the yet more ludicrous film that followed it, both entitled *Gone with the Wind*.

When as a graduate student I lived in a rural hamlet within the municipality of Santa Isabel, on the south coast of Puerto Rico, I got to know a former slave who lived there. He was at that time only four years older than I am right now. He was clearheaded, articulate and entirely sound of mind. As I listened to him, I realized that a project exactly like that of the WPA could still have been done, *then*, in Puerto Rico. Since slavery had ended there more than a decade later than in the U.S., there were still persons around with stories to tell. Nothing frustrated me more at the time than the realization that a serious *oral* history of Puerto Rican slavery would never be written. Had such a modest enterprise been completed, we would understand Puerto Rican slavery much better than we do now.

Equally exemplary oral history was collected by Studs Terkel, the American disc jockey turned writer, who was himself a product of WPA training. Terkel documented the personal experiences of thousands of Americans during the world Depression, experiencing World War II, and simply working, to mention only three of his remarkable studies (Terkel 1970, 1974, 1984). Terkel's work makes dramatically clear the benefits to be gained from collecting the lived experiences of the so-called "man in the street," with the care usually reserved for the self-serving narratives of dictators, athletes and movie stars that we know so well. When

carefully executed, oral histories surely demonstrate the method's effectiveness in recovering important data on the recent pasts of human societies. Of course, we know that every word may not be true in such cases. (There is, after all, no reason to expect every other human to be as perfectly honest as we are.)⁴

The story of life histories is a different one. But I think that these different sorts of history writing deserve to be drawn closer together. Though there are some methodological similarities between them, life history in the United States had a particular link with anthropology, my discipline, over a lengthy period. The life history is a kind of biography, but written down as autobiography, since we usually read the words of the central figure through an interlocutor or translator. Such a work purports to uncover some parts of a specific life for examination by the eyes and minds of readers. In practice life histories are quite diverse. The most moving, I believe, are those in which the reader can, so to speak, "listen" to the narrator speaking. At one time the life history was hardly ever written down by the subject herself, for many of the narrators were illiterate. But that is mostly an artifact of its early links with anthropology.

During half a century, roughly 1910-1960, anthropological life histories were almost all recorded in societies of the sort called preliterate or prescientific or, most often, primitive. This was the expectable outcome of anthropology's exclusive "concentration on the primitives because no other science would deal seriously with them" (Kroeber 1953:13). I am satisfied with Kroeber's assertion; no other science *would* deal seriously with the people called "primitive." Yet philosophers, sociologists, psychologists, historians, novelists and charlatans were prepared to tell the world what being "a primitive" was like—and to do so at the drop of a hat.

These culturally different "primitives" surely were peoples in whom no one else was seriously interested. Yet they were endlessly called to mind, verbally and even in print by persons who, though knowing little or nothing about them, would hold forth by invoking Zulus, Hottentots or Sioux, in support of some false argument about God, instinct, human nature, or some other subject.

Professional anthropology had grown up in an era when small, technically limited societies without states were thought to be models for earlier stages in the history of humanity. But instead of accepting that idea, anthropologists were the first scholars to undertake careful, lengthy, on-the-ground studies aimed at shedding light upon the social organization, everyday life and philosophical outlooks of those peoples. Such study has been highly enlightening; it supplanted considerable nonsense, much of it deadly, about "human nature," race and other subjects. But telling the truth about "primitives" was never all that anthropology

aspired to do.

In that early period the subjects of anthropological life histories were often local religious figures, so-called *shamans* or “medicine men.” Sometimes they were persons suffering from difficulties brought about by rapid social change—the same change that may have made them accessible to anthropological field workers. As such—and even though the speakers were often people tormented by their lives, captured between two different worlds—the books about them helped to make their readers aware of the great power of cultural difference. In North America, such studies offered at least a faint idea of the awful reality of Native American reservation life in the 1920s and 30s—a reality that I regret to say is not all that much better, even now.

The first such professional life histories known to me were recorded by Alfred Kroeber. Those were very brief accounts of Native American experiences in their own wars. He published some first in 1906, then a more analytic look at others in 1945. Additional, much longer, anthropological life histories were written during the early years of Freudian psychology. In the 1930s, Freud’s case histories were being read by North American scholars, at the same time as the life histories recorded by anthropologists. Psychological case studies share some features with life histories, but I think that they are different because of the basic anthropological interest in culture, in lieu of personality. Yet some anthropologists, such as Paul Radin, George Devereux and again, Alfred Kroeber, were interested in Freudian psychology, while being early protagonists of the life history.

For several reasons, I think, the life history of my friend, Taso Zayas—mostly recorded in 1953, with some material gathered during a briefer visit in 1956—differed from many of those written down before it. At the time that Taso’s life history appeared, there existed to my knowledge no published life history of a rural proletarian in any language, anywhere in the world. Though there were literally millions of workers—sugarcane workers, for example—doing industrial wage labor in the world’s tropics, apparently no one had undertaken to ask a single one of them what his life had been like.⁵ Hence, so far as the knowledge of informed citizens anywhere on earth was concerned, such people and their lives were as unknown as the anthropologists’ African herders and Amazonian hunters.

Yet the difference between rural proletarians and “primitives” such as African herders is not what I want to draw to your attention, important though it is. At least as important in terms of intellectual history is that the study of such persons as Taso at the time was not viewed by many anthropologists as legitimately anthropological. It was as if anthropology’s original concentration on technically simpler peoples had

precluded our studying anybody else (Kroeber 1953). This was what had made Steward's "Puerto Rico Project" seem alarmingly deviant to some of my colleagues, though fortunately not to all. I found myself in Puerto Rico thanks to Julian Steward, who did not think anthropology should be limited to the study of one category of human being.

My first visit to Puerto Rico began in January 1948, and I stayed for more than eighteen months. I think I first met Taso in March 1948. He had been a cane worker all of his life. He was an unpaid volunteer political worker at the time for the sitting mayor of Sta. Isabel, a local figure in the Popular Party (PPD), Dn. Francisco "Pancho" Robledo. When I told Dn. Pancho that I wanted to live in Barrio Jauca 1°, he advised me to seek out Dn. Taso.

In the book, I describe my first encounter with Taso, and how deeply he impressed me. That was in 1948. I did not begin my work with him on his life history until 1953. My first stay in the community in which he lived began in April 1948 and lasted until August 1949. It had nothing to do with life history. I was collecting community data for the wholly different research goals of Steward's team project. We team members were expected to make thorough ethnographic studies of the communities we had chosen with particular attention to the "way of life"—meaning in this case the principal economic activity—of the people in that community. Steward's goal was to establish that anthropological methods could be effectively employed to carry out the study of a complex modern society. The result, not published until 1956, was *The People of Puerto Rico*, a book that included community studies, including my own, and considerable theoretical discussion.

I stress here that the work I did in Barrio Jauca in 1948-49 was unconnected to the study I would undertake in 1953. Of course, what I had already learned by the time Taso and I began to work on his life history played a part in what we then did together. When Taso agreed to my suggestion that we work on his life story in the spring of 1953, I knew him well already. But I also knew well the community in which he lived, from having lived there earlier for over a year. None of the life histories I am familiar with was preceded by such lengthy prior fieldwork. During that time I did many things that local people were accustomed to doing. I worked at three different tasks in the cane (*picar caña*, *llenar furgones*, *planchar hierro*) for a couple of weeks; played *bolita*, the illegal numbers game; drank rum with young male friends; hunted *pichi pichi* and crabs (*jueyes*) with the children; danced to the nonstop *vellonera* in the local bar; went fishing for *sierra* and hunting for *pulpo*, went to political rallies, and so on. I deeply enjoyed everything I was able to take part in, while I was there; I know my friends in the *barrio* could tell how contented I was.

My return to the *barrio* in 1953 meant a warm reunion with many

friends. But it also was the start of an entirely new and different fieldwork project. The first pages of the resulting book give a detailed account of how I came to the idea of working anew with Taso, in order to record his life. How that work actually began is not fully recorded there, however. The importance of the details only became clear to me long after the book came out, the outcome in part of several long dialogues about the book with other scholars. I was asked occasionally to talk about the work that Taso and I did together. It was in thinking about my reply to a question in such a presentation that I realized how much my original reactions to the life history project had changed over time. I published two articles, long after the book appeared, that dealt with my growing understanding of what we had done, in writing the life history. The first, was a short piece in Portuguese entitled “*Encontrando Taso, me descobrindo*” (“Finding Taso, discovering myself”), which appeared in 1984. The second, entitled “The sensation of moving while standing still,” came out five years later. In these papers I offered some reflections on the fate of the book, and on Taso’s central role as its subject, but also as my fellow author. I want to add a comment here.

The first night Taso and I sat down together in 1953, I asked him to tell me about his life, but not, I now believe, in a constructive or precise way. I had come to work with him, and I thought that I knew what I wanted to find out. But I had not given enough serious thought to how best to do it. After a short inconclusive conversation, he asked if he could think about it, and the session ended. On the next night he brought me a statement he had written, on cheap ruled paper torn from a child’s school notebook. Chapter 3 is devoted to this and a second such statement, elicited from him three years later, when we worked together once more to update or advance his story.

Keep in mind that Taso had only three years’ schooling at most; he probably had not ever before written anything longer than a hundred words; and he was beginning to have trouble with his eyes. Yet what I think important about his written statements, especially the first of them, is that more than anything else, they gave shape to the interviews that I failed to supply with my questions. In my 1989 paper I make plain that on that second night, Taso proved that he was a better social scientist than I was. I also talk there about what kind of informant he was, and his rare ability to view subjects as if he were looking at them from the outside. And yet he was very much a man of his own society, an insider with an ethnographic eye, as it were.

When *Worker in the Cane* appeared in print in 1960, I was a newly promoted associate professor. The book was barely noticed. But a couple of reviewers dismissed it because, they said, I was a friend of my informant, and personal friendship could interfere with my work. The 1960s

were still in what I would call a “scientific” mode. I mean by this that often social scientists, in their search for objectivity, viewed the expression of emotion as a scientifically disturbing aspect of behavior. When exhibited by a scientist, it could be a deviation that got in the way of the cold truth, particularly in a case like this one. It seemed that ideally, we were to remain aloof, impartial, outside (or perhaps above) our human subjects, so that our findings could be trusted as objective.

And so in the 1990s, almost thirty years later, I was amused to see that my discipline had been engulfed by proponents of feeling (Mintz 2000). The new critics found, not altogether unjustly, that anthropology's older “objectivist” approach to fellow human beings was cold and unfeeling.

The 1990s were also a time when anthropologists were becoming afraid to do fieldwork. It was not because it was hard or uncomfortable or at times even dangerous to do fieldwork—fears my generation knew well, and usually admitted. But in the 1990s those had become old fashioned fears, not even meriting acknowledgment. The new fear was that anthropologists might be thought to be taking advantage of the people they worked with in the field. Ethnographers, we were being told, had been racists and subalterns of imperialism, exploiters of the people among whom they had lived and worked. Some of this was surely true. But such criticisms were used to nullify the validity of anthropology itself, not a finding that followed logically from the criticisms.

In my 1989 paper, I suggested that while *Worker in the Cane* was criticized in 1960 because my informant was my friend, by 1989 the same book might fall short because I came from the imperialistic power, while my friend was from the colony. I think it fair to say that both such criticisms are wholly legitimate if posed as questions. But in this instance both criticisms can be aimed at the same book; and whatever my errors, not a word of the book itself has changed since it was published. What had changed was the perspective from which the book's deficiencies might be pondered.

The translation into Spanish was admirably realized by Yvette Torres Rivera and published by Ediciones Huracán in 1982. It was a truly happy occasion for me and for Taso, who came to Río Piedras with scores of his relatives to co-sign the book with me at a reception. But it may also be a comment on the times to realize that no one in Puerto Rico had thought of bringing out a Spanish translation in the course of twenty-two years, even though most of Taso's original story had in fact been recorded in Spanish. When the book did become available in translation, it enjoyed attention particularly in Puerto Rico.

The book had not changed; the times had. Anthropology's intellectuals, swept up by a wave of postmodern feeling, urged social scientists

fiercely to warm up to their own humanity if they wanted to make sense of the lives that others lived. They wanted anthropologists to look at *themselves*. In contrast, with the passing of time (and of one sort of political ferocity), *Worker in the Cane* was enabled to become for its Spanish readers what it had in fact always and only been: one man's story and a piece of Puerto Rican history.

The English original has remained in print, unchanged, for more than half a century now. In that time, it appears that a great many people have read it. Occasionally I hear from someone about it, even now. For example, a few years ago I received an unexpected phone call from a Puerto Rican lady who had somehow come upon my phone number. She claimed to be closely related to one of the book's more scandalous female figures, and she confirmed for me material that I heard about that lively lady, now long gone. A confirmation even some forty years after the fact is still a confirmation; I welcomed it, a little skeptically.

Another instance: around 2000, I had dinner in Baltimore with a young man and his mother who were quite closely related to the Zayas family. At the time a medical student at Harvard, the young man had sought me out, he told me, because he had read the book when he was an undergraduate. I knew his paternal genealogy better than he did, and was overcome by the enormous social distance he had already marked off in his life. The grandchild of Taso's nephew, Lalo, and the great-grandchild of Taso's sister, Tomasa, and her common-law spouse Cornelio, this man has since gone on to a medical career. I learned a year or two after our dinner that he had visited Jauca, perhaps partly because I had urged him to do so. His relatives there told me it was in some ways a saddening visit, though. He spoke no Spanish, and when he showed up they could barely communicate with him, or he with them.

Finally, not so long ago while I was getting a physical exam at The Johns Hopkins Hospital, a young medical resident taking down information about me nearly fell off his chair when I gave him my name. Astonished, he asked me, "But are you *the* Sidney Mintz?" to which I could only reply "Is there another one?" It turned out, of course, that he had read the book as a UPR undergraduate. In short, half a century after it first came out, the book has fared well, I think.

There is more, of course, that could be said about Taso and the life history itself, particularly with regard to Taso's unanticipated but huge intellectual contribution to its writing. But I think it better to say something about the community in which he lived, for the part that this larger setting played in shaping Taso's character and outlook.

To do so, I ask that you to try to think of Puerto Rico in 1948, not in terms of my age, or the then still-recent end of World War II, but rather as a society with a thriving and powerful plantation economy that was

still unchallenged and dominant on Puerto Rico's south coast. At times it was as if the island were drowning in sugarcane. The Serrallés family of Ponce had its own airport, and indeed its own castle, atop *El vigía*. But not far away, extinct 19th century sugar haciendas still dotted the coast, some with their *almacenes* quite intact.

In Barrio Jauca 1°, inside Colonia Destino, its land the former location of Hacienda Capó, people still lived in the tiny boxlike houses that composed a settlement called *Verdún*, no doubt built and named at the time of that terrible battle in the first World War. Because they lived on plantation land and within its power, those people were called by an ancient term: *agregados*. Some of them carried the same surnames that I had dug out with my friend Charlie Rosario's help, from the Santa Isabel municipal archives and the records of slave baptisms after 1863.

By 1948, those who had escaped to the ramshackle houses they built on the acre of land the government had bought for them nearby, were no longer *agregados*; they were *independizados*. Taso lived there; but his land history had been different. Taso said that after his mother died, but while he was still a boy, Hacendado Dn. Pastor Díaz, the Spanish-born owner of one of the two haciendas in the Barrio, had stolen the plot that the house rested on. When Taso and Elí began to live together, they didn't move into the house where he was born and where his sister Tomasa had lived and died, but into a small shack next to Elí's mother. Taso explained to me that it was his older brother who knew the circumstances under which Dn. Pastor had laid claim to the land on which their house stood; but the brother was unwilling to contest Dn. Pastor's claim. When Taso and Elí wanted to move into that house, Dn. Pastor told them that the land there was his, and Taso would have to move the house elsewhere. Reluctantly—I think angrily—Taso complied. A few years later, Dn. Pastor became a mayoral candidate in Sta Isabel. And that was why, Taso told me, he had gone into politics. He got special satisfaction out of being able to help to defeat Dn. Pastor.

When I lived in Jauca, I could see that the Barrio had advanced only slowly into the 20th century. There were still standpipes along the road. Though it was painful to watch little girls struggling each day with the heavy cans of water, those standpipes were in fact a recent and enormous step forward. Taso showed me an uncovered and abandoned well, close to the beach, that had been the Barrio's only water source until about 1945. Piped water did not replace the standpipes until nearly 1955. By 1948, electricity was available in two of the Barrio's three settlements. And by then the company stores were dying out; one more source of planter power was struck down. The corporation *vales* which had served as money on plantations during the preceding half century were gone at last. And because it allows me to make a different sort of point, I mention

that in 1948, *everybody* played *bolita*.

I use the now over-familiar word “community” to describe Jauca, but people no longer spend time defining the term. It has particular meanings for me. I mention *bolita* simply to be able to exemplify one of my ways of defining community. When on Sunday morning the winning *legal* Dominican lottery number was announced, its last three digits were the winning number for the Puerto Rican *bolita*. And when the Dominican announcer would “sing” (*cantar*) the winning number, nearly everybody in Jauca knew instantly who had won locally. “*Ah, eso era de Dn. Diego!*” A shout would rise up on Sunday morning in the kitchens across the barrio. “*Escoge siempre el dos cuarenta seis —¡qué lindo e’ ese número!*” An amusing sidelight: after the number was announced, the announcer at the station in the Dominican Republic would play *La Borinqueña!*

There are other ways of suggesting what “community” meant. My conception of “community” is rather simple and ordinary, so let me state it quickly. Among other things, a community must be small enough so that *most people know most people*. The second point (to which there are important exceptions) is that people in a community are relatively immobile geographically. Third, such people are also relatively immobile economically. They are not enough alike to banish envy or hate or sorcery; yet they are closely enough tied to each other by what they do to live that they recognize how their fates are linked; often they interact along the threads of those entanglements. I am speaking about the group of people whom I came to know over more than a year’s time. To some degree their moral or ethical perspective was attuned to, if not influenced by, the other people in that community. To say it differently, the anonymity that typifies urban living—weakening the ethical values held and usually enforced in a community—was not available to Jauca’s inhabitants at that time, as an alternative to conformance.⁶

It was in that setting that Taso’s story stands out. Economic forces had made the lives of Jauca’s people much alike. But his luminous intelligence and articulateness set him apart, and this was apparent even to the eyes of an ignorant outsider like myself. My first fifteen months in Barrio Jauca gave me at least an outsider’s sense for the inside, such that I saw Taso as two different persons, at one and the same time—a male, rural, poorly educated proletarian Puerto Rican sugarcane worker in middle age; and the sensitive, smart, eloquent individual and warm father and husband that he, as an individual person, was. I have tried to make plain these two different images in what I wrote about him. I understand well anyone who says that what Taso and Elí had to say was more interesting than what I have to say. My reply is that is obvious, even to me. I would have had nothing at all to say if they had not accepted me. But they gave me the opportunity to bring their voices to the ears of others. I am proud

and happy that they let me do so.

Taso was a remarkable individual, but he lived an ordinary life. I mean that though while he stood out because of his intelligence and acumen, an articulate and sociable person whom Fate had treated harshly, the life he led had been strikingly ordinary. When we sat down together to record his life in 1953, he was only 45 years old, and he had lived only half of that life. He would remain in the same place at the same job with the same faith for many years more. I see nothing contradictory in my view that he was an extraordinary man who had lived an ordinary life. I think he would agree.

Notes

- ¹ This paper was delivered on February 22, 2012 as a lecture with the same title, at the University of Puerto Rico (Río Piedras) as part of the *Semana de la Historia Oral*, sponsored by the Instituto de Estudios del Caribe. I am grateful to the Instituto and its director, Dr. Humberto García Muñiz, for inviting me. My special thanks go to Prof. Juan Giusti for his valuable assistance in tracking down references.
- ² Works of this sort rely upon many speakers, giving voice to a broader perspective on the ways that past events were experienced and interpreted. They offer the opinions of many, in building their pictures of the past. Those who record them do not usually seek out leaders, such as presidents, generals and dictators, for study. Yet I do not think that this is the fundamental difference between oral history and other history. I think it is rather the expression of an underlying belief that history can be made by great men only when a large number of “ordinary” people support them. Berthold Brecht’s wonderful poem, *Fragen eines lesenden Arbeiters*, “*Questions of a worker who reads*,” captures this sentiment perfectly.
- ³ Readers unfamiliar with this body of work should consult Rawick 1972. George Rawick transcribed and published 20 volumes of these materials. The first volume was his *From Sundown to Sunup*—an illuminating introduction to the 19 volumes that followed it.
- ⁴ A recent book by Rebecca Scott and Jean Hébrard (2012), *Freedom Papers: An Atlantic Odyssey in the Age of Emancipation*, calls out for careful consideration of the place of oral history in history. Its painstaking description of two centuries in the life of one family begins in the Haitian Revolution and ends with the murder of one of its members in 1945, in the Nazi death camp at Ravensbrück. This

history was documented minutely by both records and interviews.

- ⁵ There is one luminous exception to this assertion, McWilliams's great study of migrant agricultural laborers, *Factories in the Field* (1939).
- ⁶ During 1948, as the migration northward gathered momentum, desertions by migrants of their common-law wives in Jauca were multiplying. As a consequence, the parents of young girls in the *barrio* who had eloped were bringing to court their new consensual sons-in-law in order to secure civil marriages. That is a clear instance of the way that community was yielding ground to anonymity.

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