

August 27, 1942

Memorandum to field collaborators  
From D.S. Thomas

In a memorandum dated May 12, 1942, I outlined some general questions bearing on our research and promised to plan more detailed procedures "as soon as the status of the investigation is actually settled." It is now apparent that it is unlikely that the "status" of an investigation of this sort will ever be actually "settled" (even from the administrative standpoint), and that the situation which you are observing will continue to be one of rapid and unpredictable change, with new and important problems emerging for the duration of the study, and that the application of accepted techniques of recording will continue to present great, but by no means insurmountable, difficulties.

The situation in which you are involved and about which you are making records is an enforced mass migration. We have a good background of knowledge and a body of concepts about voluntary migration, both mass and individual, concerning the role of economic forces which have acted to "push" and to "pull" people from one environment to another; the age, sex, and to some extent, mental and physical selectivity of the process; the goals that migrants hoped to achieve and the relative success or failure, under given conditions, of goal attainment; the conflict between the values of the culture patterns of the old and the new environment; disorganization and reorganization of behavior following the change, etc. Much of this background of knowledge, and many of the concepts we have developed are quite irrelevant to the present situation; "selective" factors were practically inoperative and individual wants were completely disregarded. In common with voluntary migration, but greatly accentuated we do, however, have the same problems of culture conflict and of social and personal disorganization and reorganization. On the culture-conflict side, you have the question, in the first instance, of "Japanese" versus "American" ways of behaving. One of your problems is to set up criteria by which the incidence, change, modification, and persistence of these conflicting ways can be recorded. Superimposed on this dichotomy are various important differentials, which have been noted in the earlier memorandum, for example, regional, urban-rural, age, sex, family status, occupational class, social class, etc. On the disorganization-reorganization side, you have, in the first instance, the question of determining the ~~extent~~ train of experience, the wants of the individual, the ~~extent~~ means of satisfying, and the success or failure in satisfying these wants prior to evacuation. You then have the problem of determining the extent to which the crisis involved in evacuation broke the established line of experience and thwarted or satisfied these wants, or whether, as frequently occurs in a serious crisis, there is a change of life-direction, a substitution of new ideals, which may lead to a readjustment on a higher level of efficiency. Finally, you have the problem of following through the subsequent modification of wants and behavior in the relocation process.

How will you get and organize this information? First, on the basis of your early field notes, systematic reports on Tule Lake and Gila, respectively, should be prepared immediately, according to the following simplified outline of the Shibutani-Najima-Shibutani report on Tanforan:

This report was organized under ~~the~~ three main headings: Administrative Organization, Social Organization, and Social Maladjustments. Under the first of these topics are included analyses of policing, housing and mess divisions, maintenance division, service divisions of various sorts, the economic system and rules and regulations. Under the second, Social Organization, are included analyses of the following basic social institutions:

Economic institutions: spending money, working, beginnings of private enterprise, substitutes for the money economy, etc.



The family: marriage, courtship, parent-child relationships

Religion: organizations, rituals, interrelationships

Recreation: daily routine, spontaneous groups

Political groups: self-government, internal intrigues, types of leadership, relations with administration

Education

Social Stratification

Under the third heading, that is, Social Maladjustments, are included conflicts between groups and individuals, sex problems, theft, and other forms of delinquency, destitution, and personality problems.

Second, and again on the basis of your field notes, a descriptive chronology of specific events and crises in the community should be prepared. Third, still on the basis of your notes, describe, annotate, and analyze the composition of specific social groupings which you are able to follow. Fourth, keep (and collect from others, if possible) diaries indicating the train of experience of yourselves and other individuals, and you will prepare, where possible, life histories and case records of other individuals.

As to detailed procedure, I cannot emphasize strongly enough the necessity of writing up your field notes daily. The unique opportunity which this situation offers is the obtaining of a socio-historical record on the spot and at the time the events are occurring. I realize that the situation makes it impossible for you to do more than jot down key words and "clues" during the course of the day. These key words must, however, be expanded into narrative form before they get "cold." You are not automatons. You are, yourselves, being modified daily by the experiences ~~as~~ you are going through. The chances of distortion of your records and of the introduction of bias into your field notes are greatly enhanced if much time elapses between recording and writing up. So -- the first point in procedure must be a daily write-up of field notes. The second point is an immediate collection and organization of the data for a preliminary report on the structure of the community. The third point is a summary once a week, or at the very most once every two weeks, under the categories of your "structural" report, of changes noted in respect to each of the categories you are considering.

It is of the utmost importance that you send copies of your field notes and summaries to the central office frequently. Field notes should be received weekly, summaries twice a month. Up to the present, I have been overburdened with administrative detail and have been able to devote only a small fraction of my time to analysis and critique. We are now out of the woods on the administrative side, and I shall do my part in sending you a running critique of your records at least twice a month. This technique has worked every well to the limited extent that I have been able to use it (i.e. in regard to Hoshiyama's and Hayashi's reports from Tanforan). It is the only way in which we can be sure we shall have an organized and integrated body of material. You do your part and I guarantee that members of the central staff will do theirs!

If you can so arrange it, I will provide stenographic assistance for you in the field. I realize fully that you are, or will be, overburdened with the details of your camp jobs. I realize also that full participation in community life is

essential not only for obtaining the kind of observations we need but also for avoiding suspicion as to the nature of your activities. Nevertheless, you should make every effort to be put on a part-time basis on the community job, and, as you know, we have been assured of administrative cooperation in this matter.

For your information, the formal status of the study is, at present, the following: WRA has approved Tule Lake and Gila. The situation as respects Manzanar (where Miss Okazaki is located) and Poston (where Miss Tsuchiyama is located) has not yet been completely clarified. Nor have we any plans for the relocation center to which the remainder of our Tanforan observers will be moved towards the end of September. The Tanforan study will continue, by correspondence, with the full approval of the Army and we hope shortly to integrate the reports that have been received with the preliminary Shibutani-Najima-Shibutani report. A partially completed report on Santa Anita is also at hand; and a comparable report on Tulare can be built up from Sakoda's field notes.

Our research assistant, Billigmeier, has been approved for residence at Tule Lake as a teacher; and our research assistant, Spencer, is now resident at Gila and will later, if further approved, spend periods at other Centers, particularly Tule Lake. We hope that Billigmeier can build up (in collaboration with Dr. Jacoby and with the statistical office of WRA) the formal statistical and ecological side of the study at Tule Lake.



UNIVERSITY OF CALIFORNIA  
Evacuation and Resettlement Study

Study  
207 Giannini Hall  
Berkeley, California

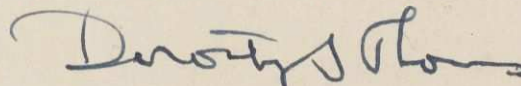
February 23, 1943

To all Members of the Evacuation and Resettlement Study:

As you know, there have been several items in the newspapers during the last week relative to a Senate Investigating Committee inquiring into the conditions of the Relocation Centers. We have no indication whatsoever that this Committee is interested in the data we have. Several collaborators, however, have raised questions about the possibility of our research data being examined and sections subjected to interpretations out of their context. This note is being written for two purposes.

In the first place, I want to assure you that I will continue to do my utmost to protect our material from non-scientific use. In this effort we shall have the full backing of the University.

In the second place, you are instructed to refer all inquiries about your work to this office. If Senate Committee members, or members of other investigating groups, examine the Relocation Centers, it is not unlikely that collaborators of this study will be questioned. You are instructed to show the utmost courtesy and cooperation to these questioners, but (a) to inform them that, as is the case, all material has been sent to the central office; (b) that as one of a group of collaborators, you are in no position to discuss the significant larger aspects of the study; and (c) that Dr. Dorothy Thomas, Director of the Study, and Morton Grodzins, her administrative assistant, are the only persons to whom inquiries may profitably be directed.



Dorothy Swaine Thomas



Please return this

October 28, 1943

This is  
the famous  
"Chicago letter"  
sent after Morton  
visit to Chicago.  
During that visit  
he initiated an  
unauthorized  
"representation" of  
the study

TO: THE CHICAGO STAFF

I have had a rather long talk with Morton in which he reported on the various difficulties now confronting the Chicago office. Some of these difficulties, e.g. the fact that you have to change offices for the third time, are of the sort that we can do relatively little about, though you all may be sure that I will try to arrange matters so that you people can have suitable and undisturbed working quarters. However, you must understand that we are the "guests" of the University of Chicago, and almost uninvited guests at that. Redfield is very sympathetic to our work and wants to help us as much as he can. Nevertheless, if moves must be made to accommodate Redfield's own staff, it is obvious that we will have to suffer in the process. I repeat that I will do as much as I possibly can, and that I will emphasize to him the fact that our study is a continuing one.

Difficulty number two, budget: You all must realize that we are in a very poor financial state. Because of the possibility that Morton will be drafted, we have acquired a full-time person to help him. Other unexpected financial burdens have been the higher-than-anticipated expense of the Washington trip, stenographic costs at Chicago and other places, Spencer's Tule Lake trip, etc., etc. The only way that we can recoup these losses, so to speak, is for Morton to be drafted! But I don't think you want me to refuse to ask for a deferment for him on these grounds. We are economizing to the limit here.

For Chicago, I understand that (1) we will continue to supply you with office materials and with incidental expense money; (2) the absolute maximum for stenographic aid will be \$110.00 monthly; (3) the absolute maximum for typewriter rentals is three (one for Charlie at home and two for the office). You must further understand that it may be necessary for us to cut these maxima later in the fiscal year, that is, we may all end up typing our own stuff on our own typewriters. Therefore, if you can get by on less than the maxima stated above, now, the less the cut may have to be later on. Further, I hope that you will arrange matters so that there will be an equitable division among you in the use of the stenographic help. Obviously, you cannot arbitrarily divide Louise up among you, hour for hour, but some stated arrangement should be made. In cases where deadlines are being met, e.g. when Charlie is finishing a case, the person meeting the deadline should have a temporary priority. This, I think, I can leave to your own good judgments.

Difficulty number three, methodological problems: This, of course, is the most important problem of all, and it is one that I look forward to discussing in detail with you next month. Morton reports that you feel that you are in the dark about the "larger purposes" of this study, and that you need more precise direction from me. I can't say that I think Morton did much to dispel your confusion or to present my point of view. Morton now feels (1) that a talk we had this morning has clarified many things for him, (2) that I should similarly try to clarify matters for you, and (3) that the discussion you had with him in Chicago was justified in that it cleared the air and made me aware of what was troubling you (and him).

Since the problems that face each of you differ to a considerable extent, although there is a common core, I shall, first, take up the question of the "larger purposes" of the study, then proceed to the question of the division of labor which I have planned for the Chicago staff, and the individual disturbances that seem to have resulted because of this division.

First, as to the "larger" purposes of the study. The primary purpose is not at all mysterious. A minority group was, in a period of crisis, forcibly uprooted en masse, and forcibly concentrated in camps. Later, part of the group was forcibly segregated, and the other part was permitted to disperse and resettle. The purpose of the study then is to collect, organize and analyze "relevant" data on (a) the nature of the restrictions that were imposed on the group; (b) how the persons affected behaved under these restrictions and after the restrictions were removed.

Your major difficulty, as I see it, is an unwillingness to accept the essential simplicity of our project. This problem would never occur to a natural scientist, who patiently adds his brick to the foundations that have already been built, and is content in so doing, and who repeats and checks on his own and other peoples' experiments and observations. The difficulty is not even characteristic of all social scientists; anthropologists and geographers, in particular, are outstanding in their willingness to collect and systematize data, unhampered by over-elaborate theories. Some psychologists are more inclined towards speculation; many of the economists have brought their "science" to a state of utter incomprehensibility by their unrealistic and fanciful theories; and sociologists are, perhaps, the worst offenders of all.

It is, I think, very significant that none of the anthropologists on the study have had the shattering perturbations that some of you seem to be going through in regard to



the "ultimate purpose" of the study. They have, of course, had other troubles, but they take it for granted that what we want is a record of the way of life of a people in a specified situation. Their troubles have been of another sort, namely, that anthropological observation has, historically, been of a static sort, whereas we are faced with a highly dynamic situation. Nevertheless, we have a more useful pattern for our own study in the work of the anthropologists than in that of any other group except the "case history" or "documentary" school of sociologists. The anthropologists' patient and cumulative analyses of groups of primitive peoples have contributed greatly to an understanding of human society. It is deplorable that sociologists have lagged so far behind in organizing data on modern society. This lag, I fear, will never be overcome until a great deal of the grandiose theorizing that sociologists teach is actually unlearned, and until the greater part of the sociologists is willing to follow that minority in their group which has dug deep into data, and got their individual hands very dirty in the process. I am naming no names, but one of the Chicago sociologists for whom you and I share a real admiration, and who is known for his penetrating insights, elaborate theorizing, and devastating criticism of empirical studies, admitted to me that he had neither the techniques nor the patience to set up or carry through any empirical studies whatsoever on his own. Nor, may I say, are his theories formulated in such a way as to lead to any possibility of empirical "verification." This sort of thing, of course, is what has put sociology in such bad repute among those in better-ordered disciplines. Einstein, who is as near a "pure" theorist as one could find, in the sense that he does not himself carry out experiments, has, nevertheless, formulated his theories on the basis of past experiments and has emphatically and repeatedly stated that if experiments and observations carried out to "verify" his theories fail to produce such verification, then the theory and not the "facts" must be wrong.

At this point, you would be quite justified in accusing me of begging the question. I gave a simple formulation of our project, but blithely passed over a crucial point, namely, what facts are "relevant" to the project. Here, we must certainly be selective, and our selection will necessarily be arbitrary. In the first place, we are studying a group that already had a history, but whose history had remained largely unrecorded. We therefore, have to attempt some reconstruction of the background of the Japanese in America prior to evacuation. In the outlines in which I have had a hand, which are based on the individual as a unit, I have emphasized demography, family, education, economics, interpersonal relations, associations. All of these are oriented around connections with the majority group and connections with the Japanese community. Part of this reconstruction

will come, then, through our individual case histories of both resettlers and of those remaining in the projects. Part of it will come from analyses of organizations (Japanese Association, JACL), religious organizations, gambling rings, mutual aid societies, and so on, these analyses to be prepared by persons who were participants in pre-evacuation Japanese communities, and who can document their reports.

In the second place, we are studying a group which was forcibly concentrated in government-controlled camps. We want a history of the progress of this group throughout their period of confinement in these camps. We have developed outlines ad nauseum to cover the main aspects of life within the camps. These outlines differ in many details, but have a common core: Administrative organization and operation, political organization, economic organization, religious organization, communication, protective institutions, food, clothing and shelter; forms of collective behavior; social stratification (including caste, class, and generation). Of these, by all odds, the most important are the political, which in its ramifications cuts across most of the other headings and includes social conflict; forms of collective behavior; and social stratification. We, obviously, cannot get every little detail about every topic, but we are aiming at a history of several projects, in broad outline, with elaboration of the major crises that occurred, and of solved and unsolved conflicts.

In the third place, we are studying that part of the group which is relocating. With much mental agony, we developed outlines for recording significant factors in the adjustment of resettlers and in the growth development and possible decline of associations.

I don't believe it is necessary for me to recapitulate the details of our plans. You might refresh your memories by reading the annual report, and going over again the outlines (in your files) that we worked out in May and June. As far as I can see, the outline for individuals is working out admirably. I must point out to you, however, that none of you has made any effort to test out the group outline or to propose any better approach to the study of associations (or any approach whatsoever for that matter). This is a matter of grave concern to me, for I certainly do not want to spend my limited time in Chicago spinning out new outlines, which are immediately filed and forgotten, while old outlines remain untested.

Now, as to the division of labor which I have suggested. Togo seems happy, Morton reports, at the prospect of working up his pre-evacuation material on the JACL, etc., along the lines suggested above. It is hoped that he will continue to attend the staff meetings regularly and to give the other members of the staff the benefit of his wide contacts and keen insights. Since he has such a limited amount of time to give



to the study, however, we are not asking him to continue to write up his notes on the cases with which he has contact. If, however, Togo finds it possible to dictate a hasty running account of "high spots" and "leads", this will be appreciated.

to Frank has agreed to devote the major part of his time/writing up the Tule Lake report, but we have further agreed that he shall continue with a few case histories, building them up as he has time. I hope it is clear to you why we have asked for this diversion of Frank's main efforts. We have a limited staff and an even more limited budget. Even if our budget were larger, I would hesitate to add anyone to the staff, for experience has shown that there are very few people who are qualified to do the sort of job we want. Resettlement is progressing slowly and is touching a relatively small part of the evacuee group. We must, therefore, continue to emphasize the "unresettled." In my opinion, no one who has not been a participant observer (emphasis on participant) in a project can possibly do an adequate job of writing up the social history of a project in the terms I have suggested above. Tule Lake is now a "closed" project as far as we are concerned, although we shall continue to get some reports from informants, and its history must be written up before the notes become "cold." Frank is, obviously, highly qualified to do this overall job for Tule Lake up to the point of segregation, and I hope that he will see its importance. Jimmy, now in Minidoka, is preparing a detailed, analytical account of segregation at Tule Lake, and a follow-up of the Tuleans in Minidoka. Tamie is now in Cleveland, writing up a similar overall report on Poston, emphasizing in particular the political set-up and political developments. In the course of the year, Rosalie Hankey will try to pull together the threads in the Gila reports, which have already been partially prepared, and she, as well as Tamie, are getting invaluable advice from Richard Nishimoto.

The question arose, I believe, as to who would eventually pull all the threads of the reports of all the projects together. That is obviously my job, and I assure you that I shall attempt to do it in a way that will not disgrace the individual workers. I cannot do it adequately without a great deal of mulling over of the individual reports. It is a job that cannot be rushed. It certainly will not be ready for publication on the day of the armistice. It will require consultation with and criticism from all of you over quite a period. Your separate contributions will, I hope, develop into a series of publishable monographs.

Although Charlie seems to suffer occasional agonies of doubt, I believe that he realizes his case histories are meeting with universal approval, and that he should continue in the future exactly as he has in the past. W.I. has suggested

to Charlie that wherever possible a report on the life history should also be secured from a member of the family, or a close acquaintance of the subject, and I have pointed out that the very nature of the resettlement problem demands periodic follow-up of the cases he has started, for "resettlement" is a continuing process and the cases will not be closed for the duration. As the follow-ups become more pressing, Charlie must realize that he cannot hope to complete a new case every week. In fact, it is clear that he may have set far too ambitious a goal in quantity production even under existing circumstances. This I recognize is partly my fault, and is, in fact, directly attributable to the "sampling schedule" I set up. That schedule must be radically revised downward, and we now have more information at hand on which to revise it. Both statistical and documentary evidence on relocation makes it perfectly clear that it is those on the periphery of the Japanese group who are relocating ("assimilated" Nisei and "marginal" Nisei), and that the real core of the Japanese group has hardly been touched. This means that we shall find only occasional Issei and Kibei (unless they are "technical" rather than genuine cases, e.g., persons born in Japan but brought over as infants). I believe, therefore, that we can concentrate our resettlement phase on the Nisei group, trying to get a reasonable distribution as between the sexes and among the occupations. We will discuss the details of this when I get to Chicago. Meanwhile, do not let the sampling worry you, and do not let quantity production worry you.

The question was also raised, I believe, as to whether much of the material would not eventually come to rest in the files, without being "used." If by "used" you mean published, the answer is obviously "yes." To give an example: Charlie has produced an enormous amount of material. Naturally, all of it can't be published. All of it will be "used" in the sense that we will all draw on it for our generalizations. Part of it - probably a very appreciable part - will be published as his own contribution. I hope, for example, to publish quite a number of his cases in toto. How many, or even which ones, cannot be determined yet. Which ones, in fact, cannot be determined until we have the complete range at hand. The same applies to the contributions of all the other members of the staff. Hence another necessity for continued collection. It is true of all investigations, both in natural and in the social sciences, that the ultimate use or meaning of materials cannot be determined until the assembling stage is completed. And in this investigation, we are far indeed from reaching the end of the assembling and organizing stages.

Tom seems to be most seriously upset about the progress of his work, about the indefiniteness of our purpose, and about deficiencies in direction. Regarding his productivity, he should certainly cease worrying about that. He had a great



deal of material to finish on Tule Lake. He took off time for this, by agreement, and accomplished what he set out to do. He took off time to go to St. Louis, again by agreement. He is now, I should say, ready to get his teeth into the Chicago situation. What seems to be stopping him is the feeling that I want him to "collect everything." This, I must say, is quite a gratuitous interpretation on his part. I suggest, therefore, that he dig out from the files the previously mentioned plan for the analysis of groups and associations, and start giving us a detailed account of the beginnings of Nisei society in Chicago. He has notes on half-a-dozen informal groups and cliques already. He can now begin a systematization of these notes, at which time he will find, as we all do, that there are many gaps to fill in, and he will know in which direction his field work must go in order to get the data to fill in these gaps. He has also, I infer from his notes, begun several case histories. He should endeavor to complete one or two of these in the near future. He, like Charlie, should stop worrying about quantitative production, but he should most decidedly endeavor to do a "finished job" on a few groups and a few cases rather than spreading himself and his fine abilities too thin over too great a variety of episodes, or worrying himself unduly about a theoretical superstructure or an objective-beyond-all-objectives.

I look forward to your reactions to this letter, and assure you that you need not pull your punches.

Sincerely yours,

Dorothy Swaine Thomas

November 5, 1943

TO THE CHICAGO STAFF:

I was delighted with your prompt and straightforward replies to my letter of October 28. In this note, I shall merely comment, briefly, on the main points you raised.

First, in regard to the need for a conceptual framework, which Frank particularly emphasizes: Our only difference here is one of emphasis. If we had had a body of knowledge out of which a sound conceptual framework could have been built, then I should have been all for it. But we had no such body of knowledge, for, as Frank points out, "an enforced mass migration was an entirely new thing for sociological knowledge." We had, therefore, to depend on "hunches" or "judgment" to get our start. And we have had to proceed from point to point, clearing up our vagueness as we go along, getting new hunches, estimating "significance." W.I.'s letter to Park, which is reproduced in Blumer's "Critique" shows that our experience is not unique. And a quotation from F.B. Wilson re statistical investigations applies with equal force to our own type of study: "A method is a dangerous thing unless its underlying philosophy is understood... Our aim should be, with care, to avoid, in the main, erroneous conclusions. In a mathematical or strictly logical discipline, the care is one of technique; but in a natural science and in statistics the care must extend not only over the technique but to the matter of judgment, as is necessarily the case in any problem of real life where the complications are great. Over-attention to technique may actually blind one to the dangers that lurk about on every side, like the gambler who ruins himself with his system carefully elaborated to beat the game. In the long run, it is clear thinking, experienced feeling and a patient poise, not automatic systems and methods, that win the strongholds of science." ("The Statistical Significance of Experimental Data," Science, August 10, 1923, p. 94).

I know of no method that will short-cut this process of "clear thinking and experienced judgment." This is what I tried to emphasize in my own way in my letter of October 28, and, on rereading it, I wonder where Tom got the idea that I was asking you to become "so concerned with method that we forget the central problem and seek only those things that are relatively easy to get." On the contrary, I was asking you, very definitely (1) to keep the simplicity of the main problem



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in mind, and (2) to avoid over-attention to a methodological or theoretical superstructure. The details of our plan call for continued experimentation in regard to what to observe and record. "Ease" of getting things has never been, and is not now, the criterion by which we select. On the other hand, "verifiability" is certainly one of the main tenets of any science, and we shall get nowhere at all unless we are willing to accept that tenet.

Frank raises an important point when he says "there must be certain phases of the individual report which are more important than others" and further that "it is frequently necessary to leave out whole areas of an individual's life....which might be of considerable importance if the problem were viewed differently." All that I ask is (1) that a common core of experiences be recorded, so that the problem can be viewed from the several standpoints which we have specified, and (2) that the individual research workers include an appraisal of the reasons why, in a given case, certain phases seem more important than others.

On sizing up all of your replies, I agree with Frank that there is no fundamental disagreement among us. There is, however, a great need for clarifying details of procedure. We will be in Chicago in little more than three weeks. I sent the "methodological section" of my letter to Tamie and Jimmy, who will also be present, in order to give them some background on the matter. I want to have a series of seminars while we are in Chicago, in which each of you, in turn, will outline (1) the problem on which you are now working, (2) what difficulties you are facing, and (3) specific questions that you want me to consider. I should like to have a brief sketch of (1) and (2) and as complete detail as possible on (3) in my hands prior to my arrival in Chicago.

Sincerely yours,

Dorothy Swaine Thomas

cc. Tamie Tsuchiyama  
Jimmy Sakoda