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THE JUNE BUG: A STUDY OF HYSTERICAL CONTAGION

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Preface

Collective behavior is a term used by sociologists and psychologists to refer to a rather large number of different kinds of phenomena. Crowd behavior, panics, fads, crazes are all forms of collective behavior. The common definitive characteristic of such phenomena is a spontaneous response of a number of people in a situation in which there is no common cultural definition of what is appropriate. What occurs, therefore, is an emergent social form whose qualities can often be specified only after they develop. In most cases, the response is an active one, the collectivity *does* something with reference to some element in the situation—they lynch the prisoner, run from the fire, buy the presumed valuable commodity, and so on.

The form of collective behavior represented by the case analyzed in this volume is somewhat different from most other forms. It is generally called "hysterical contagion," and it consists of the dissemination within a collectivity of a symptom or set of symptoms for which no physical explanation can be found. In such cases, people get sick from "gas" but no gas can be found; they get "food poisoning" but no toxic element can be found in the food; or, as in our case, they suffer from "poisonous insect bites" but no poisonous insect can be found. The noteworthy phenomenon, therefore, is not an active response to some element in the situation; it is a passive experience. The actors do not *do* something so much as something happens *to* them. In fact, we are more likely to think of them as victims rather than actors.

Although it can safely be said in general that there have not been many empirical studies of collective behavior, it is even more true that studies of hysterical contagion are hard to find. This fact made the prospect of carrying out the study reported here both more exciting and more fearsome. There was little to go on, and there was even basis for doubting that anything of value *could* be done. Since

the study could be made only after the event occurred, and since there seemed to be little order to the series of events that constituted the contagion, the usual tools of behavioral science seemed less than adequate for the task. And yet the challenge of coming to grips with such an amorphous but significant social phenomenon is great, and we present the analysis of this single case in the hope that it will stimulate further research in this area of inquiry. It will be apparent that we have not solved all of the problems of such research, but if what follows indicates that these problems are worthy of continued systematic investigation, the effort will have been justified.

The book has been organized to reflect the kind of problem faced in undertaking this study. Part I consists of two chapters which report what was "given" at the time the study got under way. Chapter 1 provides an outline of the external facts of the epidemic as reported by the various mass media and as reconstructed during our initial contacts with officials who had been involved. It thus relates what we knew about this particular case when we planned the field work. Chapter 2 is a summary of what we saw as the most relevant ideas current in the literature at the time and represents the conceptual framework with which we approached the investigation. Part II reports the outcome of our efforts within this context. It consists of seven chapters which present our solutions to the problems of research design and analysis (Chapter 3), the findings relevant to the major dimensions investigated (Chapters 4 through 7), and summary and concluding statements (Chapters 8 and 9).

This book owes its existence to the generous support of two organizations. Funds from the Office of Naval Research (Group Psychology Branch) through Contract Nor 181 C11 (Project NR177-470) made it possible to act promptly when our suspicion of extensive hysterical contagion was aroused through local newspaper reports. A grant from the National Science Foundation (NSF GS-89) enabled us to carry out the study. We want to express here our special appreciation to the responsible administration, Luigi Petrullo of ONR and Robert L. Hall of NSF, for the flexibility and promptness with which they responded to our needs and without which it would not have been possible to take advantage of this unique situation for scientific purposes.

Like other research projects of this magnitude, this study owes much to help of colleagues, staff, and participants in the field. We recognize especially the contribution of Norman Miller in the early stages of the research, in the initial contacts with plant management, the design of the study and construction of the questionnaire.

We appreciate the excellent interviewing by the field staff of the National Opinion Research Center under the direction of Galen Gockel. The further processing of the data was aided materially by a group of research assistants at Duke: A. Clarke Davis, Carl Hirsch, Patricia B. Frazer, Robert H. Roth, and Frank D. Bean, assisted in coding by Frances Anderson and Mary Sargent. The computations were conducted by Duke University Computation Center which is supported by a grant from the National Science Foundation.

Mary L. Brehm undertook the demanding task of the final editing of the manuscript and preparation of the index, and we are grateful for this essential contribution. We also wish to acknowledge the skillful typing of the manuscript by Ann Boneau and Susan Wright.

We have also profited from comments on earlier versions of the manuscript by Arlene Daniels, Kurt Lang, and Guy E. Swanson.

Finally, we must acknowledge, by necessity anonymously, our gratitude to the workers and officials of Montana Mills (especially "Hiram L. Lamont," the personnel manager who cooperated so splendidly with the research group), to "Dr. Joseph R. John" of the Communicable Diseases Center who gave us valuable first-hand medical information, and to those associated with the various media of mass communication who have permitted us the use of their reports in order that the immediate impact of the incident can be portrayed.

A. C. K.

K. W. B.

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"If men define situations as real, they are real in their consequences."

W. I. Thomas

Part I

The social phenomenon analyzed in the following pages had the qualities which are characteristic of most cases of hysterical contagion. It was unexpected, it was newsworthy, it was disruptive, it was short-lived. The decision to study it could only be made after it was over. There was some real doubt about what had "really" happened. Even if we assumed, as we ultimately did, that the official statement of what had occurred was correct, there was no very clear basis for deciding what kinds of data to collect.

The two chapters which constitute Part I provide a picture of where we were when the study began. Chapter 1 presents selections from the news reports on the epidemic and a summary statement of the information originally available to us from official sources. It reconstructs the events of the epidemic as reported to the public and as seen by those who had investigated it before us. Chapter 2 turns to the problem of conceptualizing the epidemic in light of the literature on collective behavior and in a form that would make an empirical investigation possible. Thus, Part I reports what was "given" at the time the study began.

To the experienced researcher it will be obvious that the picture was not as well-organized as these two chapters suggest, however. They have been written, after all, after the study was completed, and they would be too cumbersome if they included *all* of the bits and pieces of evidence and ideas with which we were originally faced. Chapter 2 even refers to literature published after our field work ended. The attempt is made, however, to provide the reader

with a view of the *kinds* of information and ideas available at the time so that he may move with us through the process of design and execution of the study.

1

THE "BUG" AND THE EPIDEMIC

Word first reached the public on the six o'clock news. The report was brief, and an air of mystery was already evident:

Officials of Montana Mills [1] shut down their Strongsville plant this afternoon because of a mysterious sickness.

According to a report just in from Strongsville General Hospital, at least ten women and one man were admitted for treatment. Reports describe symptoms as severe nausea and a breaking out over the body.

Indications are that some kind of insect was in a shipment of cloth that arrived from England at the plant today. And at the moment the bug is blamed for the outbreak of sickness.

Later that night, on the eleven o'clock news, further details were supplied. Some of them varied from the original report, but the melodramatic tone remained:

During the past three weeks a number of the 200 employees have been stricken with a mysterious illness, apparently caused by an insect

[1] All of the names used in this report are fictitious. In no case, however, has this failure to disclose the identity of persons or places involved a distortion of the essential information about the incident or those involved in it.

bite. Today about ten women and one man were stricken. Several were admitted to the hospital for treatment and observation. Company officials say they are fumigating the building.

The plant is scheduled to reopen tomorrow morning at six o'clock.

This station learned tonight from a company employee that the small insect attacks the skin, the bites leaving a wound similar to a gnat bite. In about twenty minutes the victim is struck with severe nausea. The company doctor informed us tonight that an entomologist is studying the problem. A report is expected later this week.

It was a Wednesday night in June, 1962. There was not much else worth reporting that day in Strongsville, a relatively small city in the South. But such strange happenings would be newsworthy in any event. In fact, the story was soon picked up by the news services, and reports of the Strongsville epidemic were seen and heard throughout the country. Before it ended, the story became considerably more complex and the cast of players grew markedly. Some feeling for the event and the reactions to it can be derived from a brief sampling of the reports of the various news media over the next few days:

Thursday—The dressmaking division of Montana Mills opened for business this morning after a night of debugging. But the reason for a mysterious illness that has stricken 40 employees is unsolved. The outbreak of the illness had been confined to females in the sewing and stitching room at the plant, where 200 women work side by side, until yesterday when a man in the warehouse area suffered an attack. Reports that the insect might have entered the plant in an overseas shipment of cloth from England have been discounted. Last night the plant was fogged with Pyrethrum in an effort to rid the premises of a possible insect.

A plant physician treating the victims—all women except one—is quoted as saying that the diagnosis is still hanging fire, and that no one afflicted by the malady could give a specific description of the insects which many said they saw. A State College entomologist called into the case said that what caused the illness cannot be confirmed, but they are working on several theories.

Hiram L. Lamont, plant personnel director, said a number of women reported for work today, when the plant resumed operations, in a highly nervous state. At least six were treated by the company physician and sent home.

Dr. C. H. Foreman, Strongsville County Health Officer, reports tonight, however, that there is nothing present in the community to get

excited about. The investigation continues, but Dr. Foreman said, "We haven't been able to put our fingers on a thing." And, he emphasized, although some of the women employees at the plant have been very sick, nobody is seriously ill. The predominating symptom according to physicians and company officials is anxiety. Doctors are keeping several of the stricken women at the hospital for observation in an effort to diagnose the ailment. Dr. Foreman says the doctors have ruled out a virus—since none have fever—and food poisoning. All are in good condition.

Friday—Two experts from the U.S. Public Health Service Communicable Disease Center arrived today in Strongsville to assist local health officials trying to determine the cause of the sudden outbreak of sickness which has hit employees of Montana Mills. These two physicians, along with Dr. C. H. Foreman and Dr. Daniel Gerard of the State Board of Health searched the plant today for the cause of the illnesses. Also on the premises were several plant officials, representatives of the plant's insurance company, two State College entomologists, representatives of the Strongsville Exterminating Company, and an engineer from the State Board of Health. All theories are being investigated, including the possibility that the air conditioning system in the plant could have been at fault. A thorough search was made of the vast textile plant, one of the most modern in the South, and several specimens were collected with the aid of a vacuum cleaner. The total catch consisted of one black ant, a housefly, a couple of gnats, a small variety of beetle—none with an attraction for human flesh—and one mite (a chigger) that could cause a reaction.

Nine persons remained hospitalized last night and two more have been treated in the hospital emergency room. There were unconfirmed reports this morning that at least four more persons have been hospitalized. About fifty persons have been affected.

We talked with one lady who suffered symptoms today—Mrs. Wilma Evans, a 29-year old resident of Pottsville. She said she felt a bite on her leg, then felt dizzy about thirty minutes later. She said her left arm became numb, and she felt weak all over. "I broke out in a cold sweat," Mrs. Evans said. But she could not find any evidence of a bite.

Dr. Foreman said that the cause of the illness is still unknown. The two Public Health Service doctors are returning to the Communicable Disease Center tonight with several specimens found today. The specimens are identified as small insects about the size of a mite.

Baffled physicians are pursuing a theory of mass hysteria in the search for the cause of the mysterious sickness. Medical spokesmen say

the symptoms are "far beyond what should be, because of fear of the unknown." Dr. Joseph R. John, one of the specialists from the Communicable Disease Center said, "We never heard of anything like this before."

However, Dr. Foreman says there is no cause for alarm. Both he and plant officials point out that only one percent of the employees have been affected and that, they say, is just about normal for a textile plant where nearly 1,000 employees work.

Saturday—Today Dr. C. H. Foreman and the experts from the U.S. Public Health Service conferred on the findings of the laboratory tests on several insects taken from the Montana Mills plant to be analyzed at the Communicable Disease Center. Among the specimens examined was a small chigger-like bug, known as a bird mite, which could be the insect causing the skin abrasion found on many of the afflicted employees. But, Dr. Foreman emphasizes that they are only saying the insect *could* be causing the extreme symptoms in evidence by most of the patients—that is, the stomach pains and dizziness. All the physicians feel that a great deal of anxiety enters the picture. They also feel that the press has played the "mystery malady" angle too much, thus increasing the anxiety in the plant and in other areas of the state.

Said Dr. Foreman: "We don't question that some of these people have been bitten . . . but certainly there is a great deal of anxiety. Fear has ballooned it out of proportion."

The physicians have advised the company to fill the building with a residual spray that would kill off the bird mite. Officials said they would take this precaution over the weekend. But the investigation continues, say the experts, because nothing specific has been found as yet.

There was one more case of the unknown sickness at the plant today. A woman employee was sent home after she complained of being bitten by a bug, causing her to feel weak and dizzy. There are still three women from the plant bedded down at the Strongsville General Hospital who were stricken last Wednesday. Others went home yesterday.

Sunday—Nervous disorder, publicity and lastly a bug's bite caused the outbreak of a "very real" and mysterious sickness at Montana Mills. That's the opinion of physicians who carried out extensive investigations. The illness was characterized by nervousness, nausea, weakness, numbness and insect bites.

They summarized it like this: The sickness was definitely real but related to overtones of anxiety and nervousness. They hesitated to use the word "hysteria." The least important factor was the bite of an insect.

Insect bites were found, but only one bird mite was found. Dr. John said it could have come in on somebody's clothing or in their hair. Dr. Foreman said the bite may have been a stimulant to incite the sickness—but it was not the cause.

Monday—Business was back to normal at Strongsville's Montana Mills plant today. The elusive bug apparently is a thing of the past. And, according to all the experts the rashes and other ailments which caused the trouble can be traced to a bug all right—but a mental one rather than one which crawls or flies.

After exterminators spent all day yesterday completely spraying the building inside and out, Dr. C. H. Foreman, plant officials, and two experts from the Communicable Disease Center voiced the opinion that most, if not all, of the fifty-seven employees afflicted were victims of nothing more than extreme anxiety. They said the illness could not be blamed on any insect.

In any event, as one exterminator put it: "Whatever has been here ain't here now."

And so it ended. No more cases were reported after Monday, and the plant returned to normal operations—though a sense of uneasiness was noticeable for some time afterward. It had come and gone in less than a week. A large number of people had been affected, and an impressive array of technical personnel had been brought in to help cope with it.

But what *was* "it"? The experts seemed to reach the conclusion, in effect, that it was "nothing," just anxiety. This, to them, seemed to be the only possible conclusion since they could find no evidence that any kind of insect was present which could have caused the symptoms recorded. They recommended the last spraying of the plant, we are told, simply as a means of reassuring everyone that there was nothing to worry about—if there ever was. Certainly, the newsmen accepted the experts' interpretation.

The experts were not very successful in convincing the women involved in the epidemic, however. We interviewed many of them two months after the whole thing was over, and even at that late date, most of them said they thought an insect had caused the symptoms, and very few suggested that the insect theory was not at least partially correct. Not only did they *believe* in the insect theory; many of them felt they had very impressive evidence of its validity.

In some cases, this consisted of having actually *seen* the insect responsible for their symptoms:

Well, I got bit, I caught the bug on my arm and I showed it to my supervisor, and we took it in the office. They put it in a jar. It looked like a little black gnat to me. It made me sick and I went home. They gave me a shot. I was out of work a day.

Well, they said it was my imagination, but it couldn't have been. I felt something bite me on my leg and when I scratched my leg, the little white bug came up under my fingernail. I got weak in the legs and got sick.

We even found some of those who had not been bitten who insisted that they had seen the insects which had bitten other women in their section of the plant:

I could see places on their arms and neck where little black bugs had bitten. We could see the bugs hop away. Then I would see the people being carried out.

These are rather graphic descriptions. It is difficult to charge that all of these women simply imagined they saw these insects. In fact, of course, the experts would make no such charge. They would say, as they did during their investigations, that there were certainly insects in the plant, and it is very likely that some of those affected were actually bitten by these insects. But, they would insist, the insects alone could not possibly have caused the dramatic reactions exhibited by the victims.

Here again the testimony of the victims would seem to contradict this type of interpretation. Many of them were very certain that there was a direct connection between the bite and the symptoms:

I was working and I felt a sting on my arm and I looked down and had a place on it. I scratched it, and my arm began to get numb. I got nauseated and they took me to the first aid room.

I was bit by the bug and it was such a sudden and sharp bite, you really didn't know what happened. It felt like a pin sticking me. Then the pain went down and up my arm and into my neck. I walked about six steps, and my legs started getting weak, and I just passed out.

And again we find women who were not themselves bitten who offer similar testimony:

Well, I think they got bit, 'cause I saw the bites on them. And I don't think anybody is going to pass out on that hard floor and vomit unless they did get bit.

The girl who rides with me got bit and had a place on her arm. She was real sick all night.

There were two different kinds of bites. One was a hole and it would bleed, and the other one looked more like a sting. The ones who had the hole were sicker than the ones with the sting-like bite, and they had to go to the hospital.

That is actually what I believe it was—an insect bite. I could see the girls as they fell over. I saw one girl's head draw back. She had convulsions. I saw the large areas where they were bitten, and the area got right hard.

There is little doubt in the minds of these women that there *was* a bug and that it *was* responsible for the symptoms from which the victims suffered. The experts, of course, would point out that, although many of the women were undoubtedly bitten, the reactions were too extreme and can only be explained by recourse to other factors, factors which they tended to lump together as "anxiety" or "nervousness." But such factors were not acceptable to most of the women. To them such an explanation meant the experts were saying they were "crazy," or were "putting on," and it is understandable that those who were bitten resisted this interpretation:

They brought this big doctor here, and he said we were crazy. But we *did* get bit.

I've got two little places on my arm where something bit me. They told us it was our imagination, but I don't think it was. One girl had convulsions so bad we all had to help hold her on the bed. She even foamed at the mouth.

I got bit on my finger. It left a stinger in my finger. They said it was all imagination, but they are crazy. I can understand that, though,

because it would have been bad for the company if people thought it was an insect.

Similar rejections of the psychological interpretation were heard from those who had not been bitten:

Some said it was imagination, but I think it was a bug that came in the material.

Well, the only thing I know is that they say they got bug bit. I think something bit them. I don't think they were going on like that for nothing.

I feel that if there was not somethin', the girls would not have been sick like they were. I don't think people should make remarks about someone unless they really know about their sickness.

Not all of the women we talked with were so certain about the matter, however. Some of them, even some of them who were bitten, expressed doubt about the "real cause" of the epidemic. Yet in almost all of these cases their doubt was less than complete. They did not *fully* reject the significance of the insect as a cause. They viewed it as a cause of greater significance than the experts did:

I don't know, but I strongly believe that they were bitten by something. Some of it might have been hysterical, but I don't think all of it was.

A lot of them believed it was hysteria, but I don't think all of it could have been. It seemed like those that took medicine got sicker than the others. The medicine may have made them sicker.

I do think some of them did get bit, but not all of the ones that got sick were bitten. When you get frightened, you get sick.

Some really got bit and some really got sick. I think they got sick because they already had a virus.

I think some people really were bitten. Then other people who saw bites on themselves really got sick, maybe from fear or maybe from the bite.

My heavens! I've been told so many things, I don't know what to tell you. They said it was bug bites, but I didn't see any bugs. Some said it was nerves. Some of those women were deathly sick. They were passing out and they were taking them out of there like flies. I don't know what happened to me. I guess it was my nerves, but when I saw all that happening, I just got scared and I clocked out of there and came home.

There were the full-fledged skeptics also, of course. Some of those we interviewed completely rejected the idea that an insect was a significant cause of the epidemic. They had little sympathy for those affected and were sometimes quite sarcastic:

I think they got hysterical. I think it was all hysteria. I think all of them worked long hours and when you do that a lot of things will bother you. You know the power of suggestion. This one got sick, so this one said, "Well, I'm sick too."

I think it was an epidemic of panic for several different reasons. Most of the people in my department were new hands. Seems like it would have happened to old hands also. Some was "panic" and some was "put-on act." The mind plays tricks on us. Some did it for attention. . . . I got a lot of bites, but they did not make me sick. Although my body fights off germs easily, if it had been anything strong enough to affect that many people, surely I would have become ill too.

They said they only found one chigger. He shore had some visiting to do if he bit all those people.

But the complete skeptics were few in number. Even among those who had not been reported as bitten, less than 10 percent fully rejected the role of the insect bite, and almost half subscribed to a pure insect theory. The rest either thought that "some of it" was something else or simply expressed puzzlement. Of course, very few of those who had been bitten expressed any doubt, although a few thought that perhaps fear or excitement may have been involved as well. Thus, whatever the official "scientific definition," the clearly dominant "social definition" was that there was a rash of insect bites, and, although some other factors may have entered in too, the bites did make a lot of people sick.

Yet, in spite of the undeniably impressive symptoms the women had (so impressive that a great array of professional talent was mobilized to cope with the problem), and the strength of the women's

belief in the reality of "the bug," no acceptable medical explanation could be found. We are left with the assessment voiced by one of the medical experts called in: "We must conclude that the outbreak was almost exclusively psychogenic in nature. There were probably no more bites at the plant during that week than in any other normal period at that plant or at other Strongsville plants."

If we accept this expert opinion, and we seem to have no alternative in a case so well studied as this one, we must view this epidemic as an almost classical case of what has been called "hysterical contagion." In the next chapter we will discuss this general type of phenomenon and attempt to tie up the several possible conceptualizations of such contagion with the basic information we have about the situation at Montana Mills. It will suffice for present purposes, however, simply to indicate that by "hysterical contagion" we mean the dissemination of a set of symptoms among a population in a situation in which no manifest basis for the symptoms may be established.

Since some of our discussion of the dynamics of the dissemination of such symptoms will make reference to the setting of the epidemic, it is well at this point to put the subjective and sometimes emotional comments just reported into a more objective context. It may easily be seen that the mass media accounts of the epidemic were less than fully consistent, at least in part because their early reports were based on "unofficial sources." A comparison between their news releases, in any event, and the more rigorously controlled data collected by the several experts who studied the case makes evident a number of inaccuracies. The victims themselves, though willing reporters, may also be unreliable sources. Briefly, then, we will present an outline of the basic facts of the epidemic as they may now be reconstructed before turning to the matter of conceptualization and analysis of the dynamics of the contagion.

A SCHEMATIC OVERVIEW

Montana Mills is a rather large plant employing 965 workers. A subsidiary of a large northern concern, it was relatively new in the area, the sprawling one-story building being only two years old. It is an unusual company in that it carries out all operations from spinning

raw fibers into thread to the manufacture and distribution of finished women's clothing. Thus, there are several different departments separated in the various parts of the one large building. The newness of the plant is reflected in its attractive appearance and cleanliness, the new equipment, and the presence of air conditioning and piped-in music. It is a union-organized plant, although it is not (and by state law cannot be) a closed shop. Wages are higher than those paid in other similar mills in the area. Many of the workers receive a piece rate in addition to their base rate which makes a relatively high income possible for many.

The distribution of the workers by sex, department, and shift is significant for our purposes. The data are reported in Table 1.1. The plant works three shifts. The vast majority of the workers (728) worked on the first shift (8:00 A.M. to 4:30 P.M.) at the time of the

TABLE 1.1
Distribution of Workers and Affected Cases by Shift, Department, and Sex

	Number of Workers	Number of Affected Cases	Percentage of Workers Affected
Total	965	62	6.4
Women	674	59	8.8
Men	291	3	1.0
First Shift	728	59	8.1
Women	576	58	10.1
Dressmaking departments	490	58	11.8
Other departments	86	0	0.0
Men	152	1	0.7
Dressmaking departments	—	—	—
Other departments	152	1	0.7
Second and Third Shifts	237	3	1.3
Women	98	1	1.0
Dressmaking departments	—	—	—
Other departments	98	1	1.0
Men	139	2	1.4
Dressmaking departments	—	—	—
Other departments	139	2	1.4

incident. Of those on the first shift, only about one-fifth were men, whereas the majority (59 percent) of those on the second and third shifts were men. This difference was largely due to the fact that the dressmaking operations were carried out only during the first shift, and these operations were carried out by women.

Because of the newness of the plant, its personnel were all relatively new to their specific jobs. Perhaps more important, the plant was still in a state of tentative organization. As the size of the operation grew, people and machines were moved, and the organization of supervision and the flow of work were altered. This was accentuated in the dressmaking departments due to seasonal changes and the introduction of new lines. At the same time, plans were being made for the building of another plant in another town, and the managers of the Strongsville plant were responsible for this planning as well.

The rapid growth of the operation had caused serious difficulties in the area of personnel management. These difficulties were compounded by the fact that the Personnel Manager was also the Production Manager and a major participant in the planning of the new plant. It is unlikely that any one man could have fully performed all of these duties. In any event, the personnel records of the company at the time of the epidemic were almost nonexistent. There was no easily accessible list of employees besides the current payroll. There were no organized records of such basic information as age, sex, race, marital status, and work experience. There were no well-established channels of communication between management and the workers.

Finally, it must be noted that June is a month of peak production in this plant. Given the seasonal nature of the basic product, women's clothing, time is of the essence, and June is a crucial month in the production of the fall line. Because of this, much overtime was worked by those in the dressmaking departments. Since there was only a single shift of these personnel, overtime was organizationally more possible in these departments than in those which worked on a three-shift basis. Normally the machines in the dressmaking departments stood idle after the first shift, but during the rush season the first-shift workers could be asked to stay longer hours to get

the work out. It is also noteworthy that the seasonal nature of the business routinely led to slack periods after these production peaks which involved the layoff or cutting back in hours worked by most of the women in the dressmaking departments. In a business which consistently faced peaks and valleys, then, the epidemic occurred at a peak of production.

It is difficult to specify when the epidemic began. As some of the news releases indicated, workers had complained of insect bites for several weeks before the first group of serious cases brought the situation to the attention of the newsmen. In fact, sections of the plant in the dressmaking departments had been sprayed three times with Malathion the week prior to the outbreak. In spite of this, the complaints continued. Most of the complaints in this earlier period, however, did not lead to medical attention. Somewhat arbitrarily, therefore, we have followed the lead of the medical investigators in using the period from the Friday prior to the Wednesday outbreak through the Monday following that Wednesday as the period of investigation. This is a period of 11 days which includes two weekends.

Within this 11-day period 62 plant employees were known to have been seen by physicians either at the plant or outside. Of these persons, 57 visited the physicians specifically because of bites and associated symptomatology. Most of the other 5 had other assorted complaints which, while not specifically defined as due to insect bites, could be defined as associated with the epidemic. These included nervousness, a burning on the calf, fainting, numbness in an extremity, feeling "like a balloon ready to burst," and an inability to turn the head. Since the epidemic was presumably a function of factors other than insect bites, all of these 62 cases must be considered as "affected cases," even though some errors of classification seem almost certain.

Of the 62 cases, 59 were women (see Table 1.1). Of the 3 men affected, 2 worked on the third shift and 1 worked on the first shift. Of the women, all but 1 worked on the first shift, the 1 other case being a second-shift worker. Thus, about 9 percent of the female workers in the plant were affected compared with only 1 percent of the males. Also, 8 percent of the first-shift workers were affected

in contrast with just over 1 percent of those on the other shifts. Women on the first shift accounted for all but 4 of the cases, and all of these 58 cases were women in the dressmaking departments or in a cloth-mending department adjacent to the dressmaking area (see Figure 1.1). Thus, about 10 percent of the women on the first shift were affected and about 12 percent of the first-shift women in the dressmaking departments were affected.

Of the 62 cases, 50 occurred on the Wednesday and Thursday referred to in the news reports. Five occurred on Tuesday, and 4 occurred on Friday of that week. The only earlier case was on the previous Friday, and 1 case occurred each on the Saturday and Monday following the first news release on the epidemic. Thus, within the 11-day period, 95 percent of the cases reported occurred in 4 consecutive days with 80 percent of them occurring on 2 consecutive days.

As can be seen in Figure 1.1, all but 1 of the cases which occurred on the first shift and all but 4 of the total number of cases were located in one large area of the plant. This represented less than half of the total area of the plant, but it contained about two-thirds of the first-shift workers. This area was concerned with the cutting, sewing, mending, pressing, inspecting, and packing of women's clothing. Except for supervisory personnel, all of the workers in this area were women. There was no second or third shift in these departments. The rest of the plant was concerned with the preparation of the cloth, the storage, shipping and receiving of goods, and the office work. A much larger proportion (over half) of the first-shift workers in these other sections were men.

Whatever our approach to this phenomenon, there are thus several basic facts which must be taken into account. The epidemic occurred during the peak production season. It came and went rather quickly, lasting for all practical purposes a week or less. It rose rapidly to a peak and just as rapidly disappeared. Those who were affected were almost all women, and almost all of them worked on the day shift. All but a very few cases were found in one functionally and spatially separate section of the plant. Whatever "it" was, it struck first-shift women in the dressmaking departments more consistently than anyone else. And, most important of all, "it" could not be explained in any "normal" way.

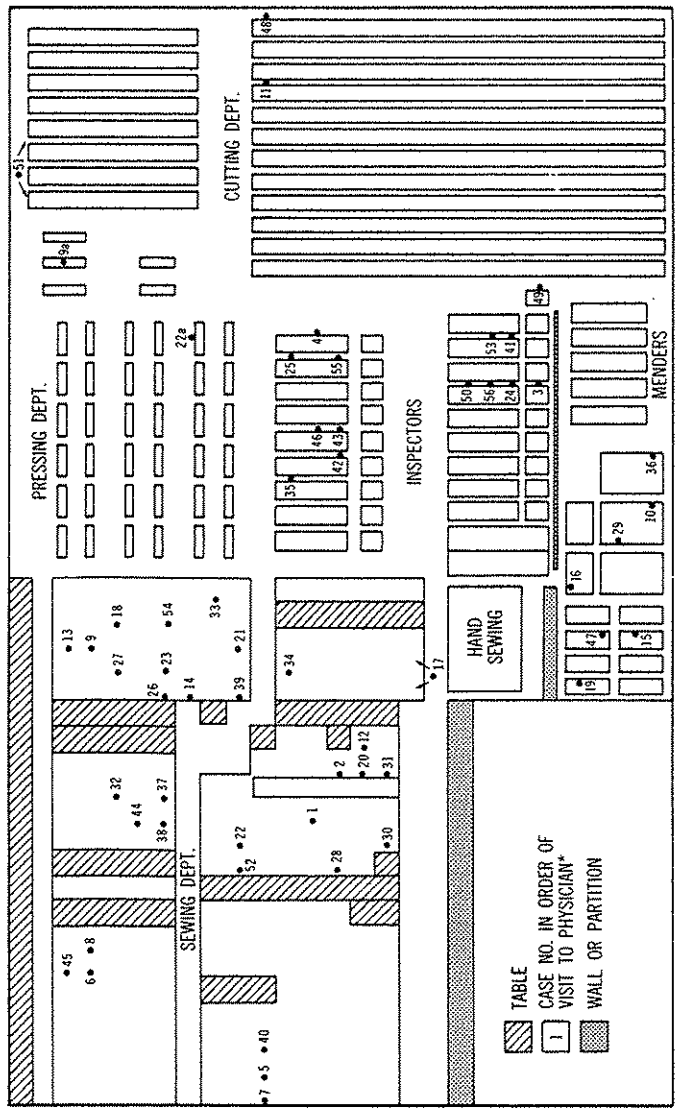

FIGURE 1.1. Diagram of Location of 58 of 62 Cases at Montana Mills

* Cases not in the "population at risk": 9a and 22a, Negro pressers; cases outside the sewing room: one each after cases 19 and 30 and two cases following 52.

THE RESEARCH PROBLEM

Given these facts, the "explanation" of the experts seems to make sense. However, to say that the epidemic "was mostly hysteria" or "almost exclusively psychogenic in nature" does not really explain the event. It simply labels it. It puts it in a category of events which, by definition, are not the usual business of the kinds of experts who investigated the case. In point of fact, it is not possible to define with any confidence whose business such phenomena are. There have been very few systematic attempts to investigate them, and, as we will see in the next chapter, the attempts to conceptualize them are at best variable and unsatisfying. The authors decided that, if this kind of event was anyone's business, it ought to be the business of the social psychologist.

As social psychologists, we felt that the experts' explanation basically posed our problem. If this was a case of hysterical contagion, if this epidemic was "almost exclusively psychogenic in nature," we needed to raise a whole series of questions about it. Why should such an epidemic occur at this time and place? What was there about this plant, these women, this period of time that brought about such an event? Why were only *some* of the people in the plant affected? Can we conceive of "susceptibility" and "immunity" to such epidemics much as we do with respect to epidemics of the flu or smallpox? If this was an epidemic, rather than a series of individual cases, what was the medium (or media) of transmission of the symptoms? If this was hysterical contagion, what can we say about the process of contagion? Is this whole business just so very mysterious that nothing short of a full psychoanalysis of all of the people in the plant will provide insight into its dynamics? What contribution can social psychology make to an understanding of such events?

These were some of the questions we had in mind as we approached the study of this epidemic.[6] With no illusions about the probability of our being able to provide adequate answers for all such questions, we nonetheless felt we should use them as guides for our investigation. The rest of this volume reports the results of our endeavors.

2

COLLECTIVE BEHAVIOR AND HYSTERICAL CONTAGION

Once the basic facts of the epidemic of insect bites had become clear, it was evident that the careful medical investigation had with some certainty ruled out physically toxic elements in the situation as a major cause of the symptoms the workers had experienced. As a result, the event was made more directly relevant to the interests of the behavioral scientist. Here was a social event involving a delimited population of persons who interacted with each other, an event which was inexplicable through reference to the physical realm alone. But even as a social event, it was an unusual one. Most social events studied by behavioral scientists involve behavior which is in some rather direct manner associated with the characteristics of the situation. Such characteristics might be physical or cultural, but the usual form of explanation involves reference to situational conditions and makes the implied argument that the actors behaved as they did because of a logical connection between these conditions and their behavior. In the present case, however, there was no obvious logical connection between any of the facts of the case as we knew them and the fact that a large number of people got sick from the bite of an insect which did not exist.

Although such a state of affairs is in one sense a disadvantage because it gives the researcher less to build on, it has certain advantages which we saw as very attractive. If we are correct in defining this as a case of hysterical contagion, what we must argue is that the behavior in question is almost completely explicable in social and psychological terms. Some of the women got sick and went to the doctor, which is a physical fact, but that sickness cannot be understood by reference to the physical context. Certain modes of behavior evolved "as if" the physical reality they faced was of a particular type, but we know that this definition of reality was factually wrong. Thus, if such an event can be understood at all, it must be by reference to the characteristics of the social and psychological contexts of behavior, not the physical. In fact, such an explanation seems to be wholly dependent on social and psychological factors.

The fascinating and unsettling thing about such phenomena as fads, crazes, and rumors is their seeming disconnectedness from the points of reference usually used in explanations of behavior. They seem inevitably to lead to outcomes which the participants do not seek, outcomes which they would presumably carefully avoid if they could foresee them. They seem to involve behavior which is founded on unrealistic, false, or illogical assumptions. Because of this, they appear to be terribly disorderly, and most behavioral scientists have chosen to avoid them, or to refer to them only to make textbooks more colorful. Yet, such events come as close as any to being "pure behavioral events" in that they exhibit the social and psychological processes at work in relatively unconstrained conditions.

Most social responses to external reality can be understood quite adequately by reference either to the demonstrable qualities of the physical realm or to the traditional cultural system of the group in question. One can usually point out that a group took defensive action because there was a tangible threat in the vicinity. This is not only true of orderly behavior such as building stockades, but it is also true of some forms of spontaneous collective behavior such as panic in the face of a theater fire. When such a tangible threat cannot be located by the outside observer, the usual explanation is presented in terms of the culture patterns of the group. For instance, we explain behavior in reference to the threat of spirits and demons by

pointing to the fact that certain people have a whole structure of such beliefs involving interpretations of natural events. We say simply that it is part of their culture. But in many instances of collective behavior such reference to the physical or cultural contexts does not provide an adequate explanation. Not only was there no evidence of a toxic insect (or any other physical source of sickness) at Montana Mills, there was also no evidence of an established culture pattern calling for the need to defend oneself against this kind of peril. In the face of such an epidemic, therefore, one's basic question is why such an event should occur at all.

There evolved a belief in a mysterious insect at Montana Mills, and the belief became very widespread within the population. Even more unusual was the fact that a sizeable number of these people actually experienced the bite of this imaginary insect and got sufficiently ill to require medical treatment. If there was no such insect, why was it "invented"? Given the fact that it was invented, why did so many people believe in its existence? And even given such a belief, what could lead to the experience of a bite from such an imaginary insect, an experience so real that sickness followed? If we strip away any possible reference to physical causality by accepting the investigators' report, and if we acknowledge that in Strongsville in June, 1962, such a phenomenon was defined as very unusual (and thus not "required" by cultural forces), what basis is there for a possible explanation? If this was a "pure behavioral event," what social or psychological factors are capable of helping us understand it? It is the task of this chapter to report what answers we were originally able to find to such questions.

The setting was almost ideal for the investigation of such an event. The epidemic had occurred within a delimited population, and we had potential access to all of the people in the population, both the victims and those not affected. This was unique in that most studies of epidemics focus almost completely on the victims. This, together with the fact that there was no evidence of physical causality, made this investigation much different from any carried out previously. In addition, we had knowledge of all the affected cases within the population including both their symptoms and when they were affected. Comparisons could thus be made between the affected and the unaffected and among those affected according

to their symptoms or when they became sick. If this was indeed a case of contagion, social influence must have played some role in it, and knowing the sequence of cases should help to trace the channels through which the epidemic spread. Thus, not only was the kind of event of particular interest to behavioral scientists, but the particular setting and the opportunity it provided for following up leads to an understanding of such pure behavioral events was close to ideal.

There are very few such cases reported in the professional literature. The case that seems most similar is one reported by Schuler and Parenton (1943) in which a number of Louisiana high school girls became ill over a period of several weeks, and no legitimate physical cause of the illness could be determined. The authors speculate that there was a process of interpersonal influence involved and that the symptoms represented a means of escape from a distasteful situation and/or an attention-getting device. Another case which involved physical symptoms was that of "the phantom anaesthetist" reported by Johnson (1945). In this case, a number of women complained of being "gassed" by a mysterious prowler. The cases were distributed over a wide area, however, and the only known source of influence which might have tied them together was the newspaper. These two cases are particularly relevant since there was no known physical cause of the symptoms which the victims suffered. However, there was little information available on the victims and almost no basis for comparing them with those not affected by the malady.

In most other related studies, the lack of physical factors cannot be assumed. Some very interesting data, for instance, come from two studies by Imboden and his associates. In one study (1959) a comparison was made between patients with chronic brucellosis and those who had recovered from chronic and acute brucellosis. Medical evidence shows that chronic brucellosis is not a physical disease but a convenient way for patients to legitimize their desire to persist longer in the sick role, and therefore differences in personal characteristics could be expected between these kinds of patients. In the second study (1961), personality inventories were administered to a large sample of people before a winter in which there was an outbreak of influenza. Personality data differentiated between the chronic and recovered cases of brucellosis and between influenza

victims who recovered quickly and those who did not. In both cases, those who recovered slowly or not at all were found to be "depression-prone." The difficulty in both cases, however, is that the patients had actually been sick with a medically recognized disease. Thus, although there were good systematic data available in these two studies, the relationship between the somatic and psychosomatic aspects of the cases is difficult to determine. Also, there was little attention paid to the difference between patients and those who did not get sick. These are common characteristics of studies reported in the medical literature.

In searching for answers to our preliminary questions, therefore, it became evident that there was little previous work which was directly relevant. In fact, there has not been any study conducted which is really comparable to this one. Similar events have been reported in the public press and in various professional journals, but in all cases the reports are too limited to permit a consideration of the kinds of questions we wished to raise. Either there was no systematic information about the victims, or there was no comparison of victims with those not affected, or it was not possible to rule out effects from physical causes. In practically all studies there was no delimitation of the population which could have been affected (what the epidemiologist calls the "population at risk") and it was not possible to compare the different groups within this population. The possibility of doing so made Montana Mills unique from our point of view.

In fact, most reports have not been very explicit. The logic has often been that since no physical explanation could be found, the problem "must have been" due to psychological causes, although evidence of these causes is seldom provided. Because of the dearth of directly relevant studies, we found it necessary (and profitable) to make reference to a somewhat broader body of literature than might at first have seemed relevant. We will report the findings of this review with reference to the major problems which guided the search.

DEFINING HYSTERICAL CONTAGION

If we are going to refer to the Strongsville epidemic as a case of hysterical contagion, our first task is to define the terms. Probably the most straightforward definition of contagion available is that

offered by Grosser, Polansky, and Lippitt (1951, p. 115). They refer to "behavioral contagion" and define it as "a social interaction in which a 'recipient's' behavior changes to become 'more like' that of another person, and where this change has occurred in a social interaction in which the 'initiator' (other person) has not communicated intent to influence the behavior of the recipient." The critical point made in this definition (and in the study from which it is taken) is that behavioral contagion differs from the usual concept of social influence in that in contagion there is no indication that the initiator intends to influence the other person(s). Also, in contagion the behavior of the initiator and recipient are always similar, whereas in other kinds of social influence the recipient of influence may be influenced to do something quite different from the behavior of the initiator.

However, in most discussions of contagion, more is implied than simple unintentional interpersonal influence to reproduce a form of behavior. There is usually an indication that contagion involves a more rapid and widespread dissemination of an act than would be implied by the definition provided by Grosser and his associates. In fact, the term "contagion" is not normally used in descriptions of dyadic relationships, which is the type of experimental situation these investigators studied.[1] Contagion, then, will be used here to refer to the spread of a behavior within a collectivity[2] in which the spread is rather rapid and in the nature of a geometric progression.

But why do we call this a case of *hysterical* contagion? We should note to begin with that the term "hysterical" is not used here in the technical clinical sense. We do not assume that all of the persons affected by the epidemic were "hysterics" as a clinician would use the term. We find the term highly appropriate for several other reasons, however. First of all, the persons affected by the epidemic generally were hysterical in the more common sense, which refers

[1] The study reported by Grosser and his associates was only one part of a larger research program, however, other parts of which dealt with larger groups.

[2] We use the relatively nonspecific term "collectivity" here because we do not want at this point to limit our discussion to any particular kind of social entity. The degree of organization of the individuals in the collectivity is, for the present purposes, irrelevant. It will become clear later in our discussion, however, that the structure of the social relations is a significant datum for any analysis of contagion.

to highly emotional behavior. Second, the symptoms exhibited by most of those affected were closely allied to those found in many cases of clinical hysteria. Third, the epidemic fits very well the pattern of belief and behavior discussed under this heading by earlier students of collective behavior. This pattern involves the spread of physiological symptoms which are attributed to a fearfully mysterious and very threatening source.

A case of hysterical contagion, therefore, is one in which a set of experiences or behaviors which are heavily laden with the emotion of fear of a mysterious force are disseminated through a collectivity. The type of behavior that forms the manifest content of the case may vary widely from one example to the next, but all are indicative of fear, and all are inexplicable in terms of the usual standards of mechanical, chemical, or physiological causality.

THE CONDITIONS UNDER WHICH HYSTERICAL CONTAGION DEVELOPS

It is difficult if not impossible to identify *in advance* those situations in which hysterical contagion may occur. We will need to review some of the discussions on the matter in order to show that this statement is justified and to suggest why this is so.

In their chapter "Societal Conditions and Collective Behavior," Turner and Killian (1957, p. 21) note the importance of social organization, normative order, and communication to the carrying out of routine functions in a society, and they observe:

> Just as routine social behavior may be explained on the basis of these characteristics of social organization, collective behavior must be viewed as arising from changes in them. It has its roots in changes in these fundamental conditions for social cohesion and integrated group action.

Blumer's (1951) classical discussion of the field of collective behavior defines it as that kind of behavior which evolves in situations not adequately dealt with or controlled by the norms. In the Langs' (1961, pp. 16–19) discussion of the relationship between "collective dynamics and social structure" they make reference to "gaps" in the social structure and refer to these as "areas of chronic stress." Such gaps occur because of the existence of areas of discretion left

by normative definitions, internal cleavages on some issue, tendencies toward alienation from social demands, and so on.

All of these discussions suggest that such conditions place people under pressure and present them with problems for which there are no fully acceptable solutions. In such situations forms of behavior evolve which are responses to strain. Some of these responses are directly problem oriented and are organized attempts to cope with the unstructured stressful situation. Others are more in the nature of collective expressions of frustration and discontent. They are expressive rather than instrumental, need fulfilling but not directly focused on the source of the problem. In most of the literature on collective behavior, hysterical contagion is thought to be this latter type of response.

When one seeks for explanations of why one form of collective behavior occurs rather than another, the answers are not very forceful. The dominant emphasis, however, is on the level of specificity of the source of difficulty or frustration and the degree of organization evolved to cope with it. It is usual to note the variation in organization of the various forms, ranging from such diffuse phenomena as fads and crazes to well-structured social movements. This variation in level of organization also generally parallels differences in the relevance of the behavior to the original source of frustration. Fads and crazes, which are wholly unorganized, are usually seen as a kind of mechanism for "draining off" the sense of tension without actually coping at all with the source of that tension. Social movements are, in contrast, usually more directly relevant to the source of dissatisfaction and thus more clearly a mechanism for problem solving.

The different forms of collective behavior, therefore, call our attention to the importance of preexisting and emergent forms of social structure in the collectivity. They also illustrate the importance of "a sense of problem" in the collectivity. These may be seen as differences in the clarity of ends (the goals to be sought, the definition of the problem) and differences in the availability of means for achieving those ends. As Turner and Killian (1957, p. 31) state in their discussion of mass hysteria in nineteenth-century Japan:

> It is of particular significance that these instances of "mass hysteria" do not necessarily constitute true revolutionary or reform movements, but

> are primarily of an expressive nature, a means of giving vent to repressed feelings. Frustration and deprivation do not always lead quickly and surely to revolt; a revolutionary goal and an organization are needed.

In the absence of a goal (however vaguely defined) and a social structure for attempting to attain that goal, the frustration associated with the "gaps" in the social structure will generally be expressed in either individual aberrant behavior or some kind of collective emotional expression.

In one of the most ambitious attempts to date to organize and systematize our knowledge of collective behavior, Neil Smelser (1963) discussed hysterical contagion as a preliminary stage in the development of panic. A central concept in all of his analysis is the "generalized belief." He says (p. 8) that

> . . . collective behavior is guided by various kinds of beliefs—assessments of the situation, wishes, and expectations. These beliefs differ, however, from those which guide other types of behavior. They involve a belief in the existence of extraordinary forces—threats, conspiracies, etc. —which are at work in the universe. They also involve an assessment of the extraordinary consequences which will follow if the collective attempt to constitute social action is successful. The beliefs on which collective behavior is based (we shall call them *generalized beliefs*) are thus akin to magical beliefs.

He sees generalized beliefs as the basis for all forms of collective behavior. They are the beliefs that prepare the participants for action, and they "arise when structural strain is not manageable within the existing frame-work of action." They form one essential part of the etiology of collective behavior, and their form and effect on the collectivity vary with the other conditions.

Cases such as the one we find in the insect epidemic, according to Smelser, are based on many of the same conditions as lead to panic. Both involve an hysterical belief which he defines (p. 84) as one "empowering an ambiguous element in the environment with a generalized power to threaten or destroy." Both involve conditions of strain, and both involve the possibility of communication among the participants.

There are a number of cases described in the literature which seem to be of this type and which clearly involve hysterical beliefs but in which panic does not occur (Schuler and Parenton, 1943;

Johnson, 1945; Medalia and Larsen, 1958). In each of these cases, Smelser says, one or more of the necessary conditions for panic was missing. There are some cases which are very similar to our own in that in addition to an hysterical belief and the report of an experience, there is evidence of the availability (and the presumed use) of channels of communication among those involved and the presence of physiological symptoms in those affected.[3] In these cases, the only factors which Smelser says are needed for panic which do not seem to be present are limited and diminishing avenues of escape and mobilization for flight.

If we accept his account of such events, therefore, we would say that cases of hysterical contagion occur in stressful situations in which there are channels of communication among those experiencing the stress. The hysterical contagion can develop, however, only if an hysterical belief is present (or evolves) and a precipitating event occurs to heighten the sense of immediate and tangible threat.[4]

We find that no one has very good advice on how we might *predict* when such cases of hysterical contagion (or panic) will occur. Condition of strain can presumably be observed in advance, but Smelser, along with all other students of such phenomena, indicates that "many different kinds of strain may give rise to one type of belief (and thus a particular type of collective behavior), and one kind of strain may give rise to many different types of belief" (p. 83). Thus, the kind of belief that develops is not easily predicted, and the precipitating event is basically unpredictable also. In most cases, it may be possible to perceive "reasonable" connections between the

[3] In the case discussed by Schuler and Parenton (1943) there are many parallels to the present case both with respect to symptoms and with respect to the possibility of communication among the participants. In the case discussed by Johnson (1945) the parallel is limited to the kind of symptoms, although the media of mass communications undoubtedly served some of the same functions as interpersonal communication presumably served in our case.

[4] With regard to the role of the precipitating event, Smelser (1963, pp. 146–147) says: "Most panics [and cases of hysterical contagion] are preceded by a build-up of what some observers have called a 'tense psychological state of mind'. . . . The place of a precipitating factor . . . is that it 'confirms' the generalized suspicions and uneasiness of anxious people. . . . Anxiety alone does not produce panic; it must be transformed into fear of a specific threatening agent."

situational characteristics, the hysterical belief, and the precipitating event *after* the fact, but any a priori statement about these relationships is not easily made.

Much will depend, of course, on the temporal relationship between the development of the hysterical belief and the precipitating event. Should the belief exist before the event, only certain events will be capable of being interpreted as relevant to the belief and thus as representing the kind of threat the belief validates. Should the event occur first, however, almost unlimited possibilities exist for the development of a belief which gives the necessary meaning to the event. We are thus left with a considerable amount of ambiguity with respect to this aspect of the etiology of hysterical contagion.

Equally ambiguous is the reason why some beliefs and events lead to cases of hysterical contagion and others do not. Examples of "precipitating events" and "hysterical beliefs" which do not lead to hysterical contagion must exist, although their existence is difficult to demonstrate because of their failure to lead to something. About all that can be said is that *some* events and *some* beliefs seems to "strike a chord" or "fit the situation" better than others, but this is certainly of little help, especially since which ones "strike a chord" can only be seen in retrospect. Even at that, some interpretations of the relationship between the situation and the precipitating event are based on rather involved reasoning. We cannot help agreeing with Brown (1954, p. 843) when he complains:

> We should like, first of all, to know why this kind of behavior "contages," rather than reasonable, cooperative action. In the second place, it is clear that even irrational and emotional behavior does not always diffuse through a collectivity. Under what conditions will it do so? The answer to this has too often been that contagion of emotion will occur in the mob, with the mob defined as a collectivity manifesting contagion. Something better than this must be found.

Smelser's analysis helps by suggesting the kinds of situations in which *some kind* of feeling or hysterical belief may "contage," but it does not offer much assistance in determining *what kind* of event or belief will have this effect in any given situation.

We can thus offer even provisional answers to only part of the

questions relevant to a definition of the conditions under which hysterical contagion occurs. The most general statement that can be made is that it is likely to occur when a number of people are in a stressful situation for which no traditional forms of solution appear appropriate and within which develops a belief in an ambiguous threatening force. There are likely also to be channels of communication among the persons involved but no organized attempt to deal with the source of strain and no general mobilization for flight. Greater specificity as to what conditions lead to this form of collective behavior does not seem possible at present.[5]

THE CONNECTION BETWEEN THESE CONDITIONS AND PHYSICAL SYMPTOMS

It is one thing to note that various forms of collective behavior tend to occur when particular sets of conditions are found, but we need to move on to an even more refined question relevant to the case we are studying. Given that certain general conditions are the necessary if not the sufficient conditions for the development of a case of hysterical contagion, how are these conditions related to what actually occurs? If some kind of "imaginary" physical experience is the hallmark of hysterical contagion, what is there about these conditions which makes such an experience come about?

[5] Brown (1965) has built on Smelser's analysis through a game theoretical treatment of collective behavior. He is able to show that, given a certain strength of communication and trust, mass behavior can be rational from the point of view of the individual participating, even if irrational from the point of view of the observer. Although his discussion provides an elegant analytic tool it would be difficult to use it to predict behavior in concrete situations and seems particularly deficient in treating an outbreak of hysterical symptoms which do not have any direct value even to the individual concerned (in contrast to panic behavior which may be of value to the first person to get out the door). In spite of these cautions it is striking how similar his definition of "crowd behavior" is to our analysis in the previous paragraph. "The basic situation is one of communication among persons having a similar conflict of mind, between an impulse that is socialized and one that is not, with the physical possibility of acting out the unsocialized impulse. . . . The outburst will occur because the total set of conditions produces a payoff matrix such that each person can act on his unsocialized impulse and be free of punishment or guilt" (p. 760).

To cope with such questions we need to make reference to a different part of the professional literature. For the most part, students of collective behavior have been largely concerned with the social aspects of their subject matter and have not dealt with questions relating to the specific content of the contagious experiences involved. To the extent that concern for content has been exhibited, it has been by way of noting that the symptoms were undoubtedly "symbolic" in some way of the "real" source of difficulty. Although this is a beginning and the place for us to start, it does not really help a great deal since the rules for determining the connection between symbol and reality are not specified.

There seem to be two approaches to this problem, one from the behavioral science side, the other from the medical side. Behavioral scientists have noted that there is always some kind of belief pattern which gives meaning to the physical experience. The hysterical belief Smelser refers to serves to objectify the source of a pervasive sense of discomfort and tension. It gives meaning to the dissatisfaction associated with the sources of strain. The belief serves this function because the strain involved is either too general to permit its identification or it is so closely associated with highly valued aspects of the people's lives that its identification would be too threatening. If some external source of threat is thus "invented," it can solve that part of the problem the people face. Since such a belief is motivated by strong needs for understanding and relief from tension, it is likely to be adopted avidly. Belief may then influence perception so that what is "imaginary" from the point of view of the outside observer can be "proven" by those involved through what they view as incontrovertible evidence. Perhaps the most impressive evidence that can be presented is the physical effect of the source of threat, an effect that seemingly can be explained only through reference to the imaginary threat.

But this only tells us how the belief and the symptoms can function to reinforce each other. Certainly no one would claim that the victims of such epidemics as the one we studied "figure out" such a relationship and then "decide" to get sick in order to prove that a frightening and toxic element really is among them. If it is not a conscious process, however, there are at least two further

questions to raise. How does it happen that the new source of threat gets invented at all? What makes the people sick if the threat is imaginary?

There is little to be said in answer to the first of these questions. The connection between the experience of strain and the hysterical belief seems to be purely a matter of chance. Undoubtedly such an hysterical belief is invented by one or more individuals in the situation as a function of their own unique personalities and experiences. The precipitating event is likely to be the behavior of such a person based on the new belief. The belief is then adopted by others because of its functional significance in the situation. It seems almost certain that such beliefs are invented quite frequently in any population, but it is less common that the belief and a sense of strain that can easily be explained by it occur together. It would be very difficult, however, to demonstrate that this is so.

Medical investigators have become increasingly concerned with the fact that illness is not simply transmitted by physical vectors and organic injuries. This renewal of interest in medical ecology has been given an eloquent expression by Rene Dubos in his book *Man Adapting* (1965). He summarizes the section on determinants of microbial disease as follows:

> It is clear, in conclusion, that the type of relationship existing at any given time between hosts and their parasites is the outcome of many different factors, including past racial genetic experience, evolutionary adaptation through genetic changes and immunological processes, and transient disturbances in the external and internal environments. In the classical infections of exogenous origin, the determining event of the disease is exposure to the infective micro-organism. In endogenous microbial disease, the immediate cause is the environmental factor that upsets the biological equilibrium normally existing between the host and the microbial agents (persisters) (pp. 194–195).

This statement expresses the view that part of the etiology of any disease is the contagion mechanism which we have been deriving from the theories of social psychologists. The studies of disease we have discussed earlier in this chapter (pp. 22–23) are attempts to establish such relationships. On further search it becomes disappointing, however, how little definitive work has been done on any social conditions of illness. Dubos again states the case:

> Successful physicians of all times have known that no disease state can be understood without considering the patient as a whole. But while this general truth is given lip service in medical schools, it is not taught with the thoroughness which comes from conviction, and it has not generated many research programs (p. 441).

Because of this paucity of concentrated research efforts on sociomedical problems we shall restrict ourselves to a critical discussion of a symptom which occurred prominently at Montana Mills, namely fainting.

Engel (1962a) has discussed the impact of psychological stress on the individual and notes that often such stress is adequately handled at the intrapsychic level. This he refers to as the "psychologically compensated state." However, in the "psychologically decompensated state," in which intrapsychic coping breaks down, "psychological stress acts to mobilize biological systems for the defense and protection of the body" (p. 367). Under these conditions, Engel points out,

> . . . not only do affects become felt but also the neural system discharging into the body may be activated, meaning in essence that bodily systems are being mobilized to assure needs and to cope with stresses that cannot be handled through intrapsychic processes and controlled behavior.
>
> This, then, constitutes the most general and the most common avenue whereby somatic changes are brought about in response to psychological stress. On the psychological side this involves the experiencing of both signal-scanning affects of unpleasure and drive-discharge affects, felt as states of diffuse unpleasure, such as discomfort, distress or tension, or as their more definitive cognitive states, such as anxiety, fear, guilt, badness, shame, discouragement, anger, helplessness, or hopelessness. What is felt includes not only such vague to precise mental content (the cognitive part), but also the awareness of physiological changes, such as palpitation, sweating, flushing, muscle tension or "butterflies in the stomach" . . . The body, no longer buffered against the vicissitudes of the environment by an effectively operating mental apparatus, now is alerted to anticipate damage or exhaustion (pp. 383–384).

These physiological changes are exactly the kinds of symptoms found in all cases of hysterical contagion. In the most severe form, such physiological responses to psychological stress can lead to fainting, particularly in situations where the body has prepared for flight

from a threatening danger but the individual is inhibited from carrying out the actual behavior of flight (Engel, 1962a, p. 390; Engel, 1962b).

What we find here, therefore, is not only an explanation of the connection between the experience of psychological tension and physiological symptoms. We find also a basis for understanding why an external "cause," which forms the core of the hysterical belief, would be so easily accepted. Not only does it place the explanation of the experienced discomfort in a socially acceptable etiological framework, it also provides a "cause" whose presumed "effects" correspond closely with the actual experience of those who are under stress.

A very interesting set of findings relevant to this matter has been reported by Schachter (1964) who has investigated the relationship between the cognitive and physiological elements of emotion. In a series of experiments he manipulated these two elements and was able to demonstrate that either could be influenced by the manipulation of the other. His significant theoretical contribution from our point of view is reflected in the following statement:

> The key cognitive assumption underlying the human experiments described is that "given a state of physiological arousal for which an individual has no immediate explanation, he will label this state and describe his feelings in terms of the cognitions available to him." Obviously, this proposition implies that a drive exists to evaluate, understand, and label ambiguous bodily states. . . . Given a new, strange, or ambiguous bodily state, pressures will act on the individual to decide exactly what it is that he feels and to decide how he will label these feelings (pp. 76–77).

From this perspective, what occurs in cases of hysterical contagion is that physiological symptoms, which occur largely as a result of unresolved psychological stress, are explained (and thus responded to) in terms of a newly invented label which is provided by someone in the situation. To the extent that the new label "makes sense" and is thus easily accepted, it will be adopted readily by all those who have the experience but have been unable to conceptualize it satisfactorily. Schachter's work demonstrates that different labels can be successfully given to what must be assumed to be the same physiological experience and that the experience will correspond with

the label in the view of those having the experience. The kind of physiological arousal which Engel describes, then, is evidently capable of a rather wide range of cognitive interpretations. For a case of hysterical contagion to occur, what is needed is a combination of a number of people who are so aroused and a belief in a credible source of such discomfort. Evidently the arousal is present beforehand and what "contages" is the new belief which gives meaning to the sense of arousal. One would also assume, however, that as the credibility of the new belief increases, relatively minor physiological cues might suffice to suggest a connection between one's experience and the source of threat.

THE PATTERN FOLLOWED BY HYSTERICAL CONTAGION

If one is interested in mapping the flow of hysterical contagion, a number of different kinds of questions might be raised. For instance: What is the rate of diffusion of the behavior? What kinds of people are likely to be involved? What is the channel through which dissemination occurs? What relationship does the contagion have to the characteristics of the collectivity and/or the larger social system? Suggestions have been made regarding all of these matters, and we will present a brief summary of them here.

As we noted in Chapter 1, the spread of the insect bite symptoms accelerated rapidly and just as rapidly diminished. The vast majority of the cases occurred on two days with a few days of build-up before and a few days of drop-off after. This is a familiar pattern, and it has been found in other such cases. We seem generally to find a kind of "snowballing" effect which evidently soon leads to a state of satiation. However, agencies of social control usually enter in to limit the spread of the behavior before any "natural satiation" occurs. Some of the rapid drop-off, then, is undoubtedly a function of outside forces being introduced into the situation. The pattern of rapid rise and rapid fall, however, is very common.

When we ask: "What kinds of people are affected?" the answer is far less simple. The easiest answer to this question is that only people in the situation are affected, but even this is not as clearly true as might be expected. Or, one might at least wonder how we

decide who is "in the situation." Were those outside the dressmaking departments at Montana Mills in the situation or not? Another deceptively simple answer is to say that only people who have been exposed to the pre-contagion strain would be affected. But we have already seen that it is generally conceded that any number of sources of strain might lead to the "same" kind of hysterical behavior.[6] Thus, participation in an hysterical epidemic such as we have studied may somehow satisfy the various needs of persons under very different kinds of strain.

Most of the discussion of the kinds of participants, or the variations in susceptibility to contagion, however, focus rather directly on the more enduring personal and social characteristics of individuals. Some have focused almost exclusively on a personality type and claimed that participation in a whole range of types of collective behavior is a function of meeting the needs of such a personality (Hoffer, 1958). Also a common assumption is that women are more susceptible than men to the kind of contagion we are studying. Others assume that low intelligence is a good predictor. Many discussions have revolved around the questions of whether there is a trait of "general susceptibility" to contagion. We would agree with the general conclusion of the Langs (1961, Ch. 10) that there is good reason to doubt the adequacy of any of these sweeping generalizations.

However, it may well be that some kinds of people, under some circumstances, have a greater potential for participation in the contagious spread of unusual behavior. The Langs note, for instance, that the socially inadequate and insecure person will presumably be more dependent upon social cues for an indication of his own adequacy. He should, therefore, be more susceptible to almost any form of social influence. In contrast, those they define as "high ego-

[6] This position is defended very cogently at a much more general level by George Devereux (1961, p. 236). He points out that participation in *any* social activity may serve many different individual needs. "The real point to be stressed is that *both organized and spontaneous social movements and processes are possible not because all individuals participating in them are identically . . . motivated, but because a variety of authentically subjective motives may seek and find ego syntonic outlet in the same collective activity.* This is equally true of spontaneous revolutionary movements and of extreme conformity."

defenders," who project their own intrapsychic conflicts into the world, will be strongly susceptible to some kinds of influence but not others. Similarly, there is some appeal in the idea put forward by several authors that those who are "socially marginal" might be more susceptible than those closely integrated into a normatively controlled social structure (Turner & Killian, 1957, pp. 103–112). Since such marginal persons are less constrained by group norms, participation in the process of hysterical contagion should be more probable for them. This should be so because such contagion, by definition, is predicated on an unusual definition of the situation, one which is often outside the established normative system.

This last point brings us to a consideration of the third of our four questions: What is the channel of dissemination of the contagion? It also reveals some of the contradictions and ambiguities in the literature relevant to hysterical contagion. Since many conceptualize contagion as the result of a process of interpersonal influence, presumably the interpersonal relationship is a basic medium of transmission of the hysterical behavior. In their studies of behavioral contagion, Polansky, Lippit, and Redl (1950) found that, among the children they studied, those in less secure social positions neither initiated nor were susceptible to contagious influence as much as the secure children. On the other hand, these insecure children were much more subject to direct social influence. These authors also suggested that the less secure child may join in such contagious activity but do so relatively late in the process, after it has become clearly accepted by the group. Such an analysis adds to our understanding of contagion, but it also emphasizes some of the real complexities of the process.

The same authors note a related complexity when they interpret some of their experimental findings as indicating that "impulsive" children will be more active in contagion in situations in which there are strong normative constraints but also a strong pressure on the group toward deviation from the norms. We find here the suggestion that the significance of one's social position for the transmission of contagion may vary depending on the kind of behavior being diffused and the kind of situation in which the collectivity finds itself. Many other bases for such an ambiguous view could also be presented. Because of the complexities involved in such a view, we

have found it difficult to state with any clarity a general relationship between social position and role in the transmission process. We thus postpone until Chapter 6 a more detailed discussion of the issues involved, and at this point we simply record that social position should presumably be an important factor in the transmission process.

Somewhat similarly, the question of what personality characteristics are most likely to lead to participation in a pattern of hysterical contagion can only be answered in a very tentative way. The answer must be given largely in terms of the relationship between the personal characteristics of the people involved and the kind of behavior which spreads through the population. One can more easily predict which kinds of people will be likely to be affected once one knows what kind of behavior those affected engage in and what kinds of beliefs legitimize that behavior.

All of this leads to the conclusion that there is undoubtedly some pattern of interaction among personality, social structure, and situational characteristics which influences how the diffusion of the pattern of behavior will occur. Although there seems to be general agreement that *some* kinds of personality characteristics and *some* kinds of social relationships may assume significance in *some* kinds of situations, it is difficult to specify with any degree of certainty what combinations are most likely to occur together.

Finally, we turn to our last question in this general area: What relationship does the contagion have to the characteristics of the collectivity and/or the larger social system? We again find some suggestions but little firm guidance. We have already noted the emphasis given to the role of strain or stressful conditions in the development of all kinds of collective behavior. The preceding discussion also suggests that level of organization of the collectivity may play an important part. These two factors are presumably related since a highly organized group will have fewer sources of strain of the type the Langs call "gaps in the social structure," and highly organized groups should have greater resources for coping with strain when it does occur. Earlier in this chapter we also noted the importance of external agencies of social control in the development and spread of contagion. The effectiveness of these agencies will be a function of the degree of organization of the larger system. There is reason to believe, therefore, that the degree and effectiveness of

organization of both the participants and the larger social system of which they are a part will be important variables in determining the pattern and extent of dissemination of contagious behavior.

APPLICATION OF CONCEPTS TO THE INSECT BITE EPIDEMIC

Following Smelser for the moment, we can see that the insect bite epidemic included all of the ingredients for panic except "limited and decreasing access to avenues of escape" and the final "mobilization for flight." "Strain" was present in the fact of overtime work, the lack of clear organization of the plant, and the probable conflict between work and home obligations for many of the female workers. One can assume that under these circumstances many of the workers had at least vague feelings of frustration and discontent in the work situation. It may very well be that the persistent complaint of insect bites prior to the epidemic was an expressive means of giving vent to these feelings. The bites were at least tangible sources of irritation which could be defined as unnecessary, whereas the overtime could be seen as both necessary and desirable (for the income it provided), and the lack of effective organization in the plant was too vague a source of irritation.

It is difficult to say whether the precipitating event preceded or followed the development of the generalized belief in this case. Since the plant had been sprayed twice just prior to the epidemic, and the bites had continued, there had undoubtedly begun to develop some notion that these were unusual insects. However, it seems unlikely that they were defined as the sinister, mysterious, and potent force they soon became. Therefore, a precipitating event must have occurred which confirmed the vague worries about "the bug" and provided a tangible focus for the ambiguous sense of frustration and anxiety which had evolved. This precipitating event was evidently the extreme reaction of the first case (or first few cases) in the epidemic. Suddenly there was tangible evidence of a serious threat. Suddenly anxiety was replaced by fear as a real danger was found.

But what "caused" the first case, or the first few cases? It is interesting to note that the first case was a young woman of 22

who complained of a bite on the Friday before the first big day of the epidemic. Soon afterwards, she fainted. Most noteworthy, this woman had fainted approximately five times during the past year, the last time being one and one-half months before she fainted in the plant. (She also fainted again later in the epidemic.) The second case, which occurred on the following Tuesday, was a young woman who worked near the first case. She said she had been bitten the previous week, but she did not report to the doctor until that Tuesday when she complained that she felt "like a balloon ready to burst." On the same day four other women reported to the doctor. The third case passed out soon after having been bitten that afternoon. This young woman had been under a physician's care for the past several months for "nervousness." The fourth case complained not of a bite but of a crawling sensation on her thigh, and she almost passed out. Late that afternoon the fifth and sixth cases occurred when two women became emotionally disturbed, and one of them fainted. The next morning the epidemic developed with a rush. Eleven women reported to the medical authorities before noon, and the contagion began to snowball.

It may very well be that the first few cases (or at least some of them) were responding to conditions which were not extraordinary in the plant context but which seemed to them to be extreme because of their presumably unusual personalities. Their reactions may have been appropriate to the situation, therefore, only in the very special sense that their personalities made them so. As they were observed by others, however, the behavior they exhibited appeared to indicate an extremely potent force at work. Undoubtedly some of those who observed these first few cases simply shrugged the whole thing off as the strange behavior of "some crazy women." But others were more impressed by what they saw, and the hysterical belief began to evolve and spread. As the number of cases grew, the validity of the belief was increasingly strengthened and more widely disseminated, until almost everyone believed in the reality and potency of "the bug."

Returning to Smelser's discussion again, we may ask why panic did not occur and why this hysterical belief did not give rise to "pell-mell flight" in the face of such a potent danger. There seem to be several possible answers, although our evidence is less than adequate to provide convincing support for them. First of all, there evidently *was* some flight from the danger. As one of the women indicated in the interview, "I just clocked out of there." Management told us that there was an increase in absenteeism during this period, but they had no adequate records of the extent of it. Second, management acted on the afternoon of the big Wednesday in a way that precluded flight: they sent the workers home. This did not remove the threat from the situation (cases began occurring immediately the next morning), but it removed the workers from the threat at least for a while. Third, the *possibility* of "clocking out" was always open to the workers, although admittedly difficult to utilize because of general work norms. The avenues of escape were not limited, nor were they diminishing. Fourth, from the onset of the epidemic, forces of social control were at work. Management took control by clearing the plant and calling in the exterminators. Other experts were soon on the scene telling the workers that there was no reason for fear. The press was hinting broadly from the beginning that it was simply a case of some hysterical women (although they also played up the mystery involved). These forces of control were far from fully effective, but they undoubtedly reduced the seriousness of the epidemic. All of these factors, then, kept the epidemic from becoming a case of panic.

The hysterical forces, however, were effective—an epidemic did occur. Given the balance between the forces impelling the workers toward hysterical contagion and the forces operating to dampen this effect, some became more clearly affected by the contagion than others. What basis do we have for predicting which ones were affected and which were not? This seemed to be the most reasonable question for us to use in guiding our research. We cannot in any convincing sense "predict" the epidemic itself since it had already occurred and we have only the single case to analyze.[7] We can illuminate the case by reference to what are presumably the dynamics of such events, but there is no basis for testing any tentative expectations about a single case such as this. We can, however, even after the event, investigate the applicability of expectations about

[7] We did, however, collect similar data from another clothing manufacturing plant in which no epidemic had occurred to determine how atypical our subjects were. See the Appendix for a discussion of these data.