In the side room of a conference venue in Chandigarh, India, eleven researchers from four different countries sat together with the intention of establishing whether the two research groups they represented—the first an international one based in Sri Lanka, the second based in India—could develop new research together. They were attending the 2008 annual international gathering of toxicology researchers. All those gathered in the room shared an interest in how poisons work on the human body and how they might be dealt with once ingested. Their shared hope was to augment and improve research into a pressing medical and public health problem in South Asia: the alarming numbers of people who die from ingesting pesticides (for example, see Patel et al. 2012 for an Indian perspective).

The Sri Lanka–based Australian director of the South Asian Clinical Toxicology Research Collaboration (SACTRC) had learned about the Chennai-based group through mutual research interests and contacts. At the Chandigarh meeting, Salla was present as an observer, and she recorded how the researchers set about figuring each other out and looking for
common ground upon which they might, at some point in the future, start to work. They agreed they wanted to begin with something small to see whether they could work together around common interests and pooled resources. As the SACTRC director put it, “there is no point starting with something large like a multicenter trial to discover that things don’t work out. That would be a waste of everybody’s time.” As we will see, the reference to “things” covers far more than just the science of clinical trialing; here, it encompasses the host of intangible qualities that people bring to their relationships, such as integrity, politeness, respect, and tolerance.

During the meeting it was mostly the Sri Lankan group members who asked questions: What facilities were there in Chennai? How were poisoning patients managed? What poisoning types did they have, and what had they taken? How far did their patients have to travel to the hospital, and how quickly could they get there after the poisoning? What research interests did the Chennai group have? Did they have ethics committees in place for regulating their research? The Indian group received quite a grilling, and after a long discussion a timeline was agreed on for the researchers to explore further the potential for working together. The prospects of a future collaboration looked good. Mutual research ideas were exchanged by e-mail afterward, and several months later a group from Sri Lanka went to visit Chennai to firm up the arrangements. Upon meeting and visiting the Indian partners, the director commented to Salla that they were “a goldmine.” The Indian group had a functioning toxicology unit in a local hospital, their own poisoning ward, lots of patients, skilled staff, and good laboratory facilities.

In the encounter we have described, we might be reporting on nothing less than the birth of a collaboration—that is, the coming together of one group of researchers for a productive research engagement that could last many years. The shared concern with poisoning in the developing world gave an important telos or goal to their collaboration in the making. It not only provided them with a compelling justification for their meeting but also introduced a hoped-for mutuality. Given the seriousness of the issues at hand, the question was not so much: How could they collaborate? as it was: How could they not?

In recent years, there has been a move to bring biomedical research to bear on some of the developing world’s most pressing problems. The extent to which biomedical research has failed to address these problems was highlighted by the Global Forum for Health Research when it drew attention
to findings of the Commission on Health Research and Development (COHRED) produced in the 1990s. The COHRED report identified what it described as the “90/10 gap.” Put simply, less than 10 percent of the spending on health research went toward dealing with the health needs of over 90 percent of the world’s population. Since this startling statistic first gained currency, significant international efforts have been mobilized to address the health inequalities that it underscores. In recent times these endeavors have brought into sharper focus the diseases of poverty, such as tuberculosis, malaria, human immunodeficiency virus (also see Stevens 2004), and other less prominent or neglected diseases such as dengue and leishmaniasis, which blight the lives of many who live in low-income countries.

In commercial terms, these diseases have never been profitable to treat, so they have received little attention within mainstream biomedical research. The recent engagement with these and other problems endemic in the developing world has moved researchers beyond an earlier tradition of academic interest in tropical medicine and ushered in a broader interest in the inequalities of finance, gender, access to medicines, and the structural conditions under which diseases become “neglected” in the first place (Biehl and Petryna 2013; Farmer 1999, 2004; McGoey 2014).

These initiatives are currently subsumed under the broad heading of “global health,” and they have opened a new field of interest and action. Initiatives such as the Global Vaccine Alliance, International AIDS Vaccine Initiative, STOP TB, and the Roll Back Malaria Fund have been established to target particular conditions. Funding for such initiatives has been provided by governments and international nongovernmental organizations (NGOs) such as the World Health Organization (WHO). Funding has also come from charitable and philanthropic sources such as the Bill and Melinda Gates Foundation and the Wellcome Trust. However, as one of the researchers in our study pointed out, these funders combine their philanthropic overtures with a research business model, which carries pressure to generate results and drive tangible change. Many of these initiatives have had the effect of blurring the boundaries that previously separated commercial, privately funded research and academic, publicly funded research. In short, there are few examples of either entirely publicly funded studies or entirely privately run research, as many developing-world research initiatives now combine elements of both. Clearly demarcated public–private partnerships such as the Medicines for Malaria Venture or the International AIDS Vaccine Initiative
tend to operate with agendas that interweave commercial, industrial, humanitarian, nongovernmental, and governmental interests.

Increasing the amount and type of research performed in resource-poor settings has been one of the major objectives of COHRED and the subsequent attempts to help reduce the 90/10 gap (Haines, Kuruvilla, and Borchert 2004). In practice, a key mechanism to achieve this reduction has been the stimulation of international collaborations to address “local health needs” (Glickman et al. 2009; Mayhew et al. 2008; Simon et al. 2003). International collaboration in biomedical research is seen as a key opportunity for partnerships to develop that will bring about transfers of the knowledge, resources, and personnel needed to address gross inequalities in health research. It is within this broad program of action that the work of our second case study, SACTRC, might be situated.

The South Asian Clinical Toxicology Research Collaboration

SACTRC was established in 2004, a collaboration that grew out of less formalized cooperation going back to 1988 (Phillips et al. 1988). The research performed at that time resulted in publications that emphasized the particularly high suicide rate in Sri Lanka when compared with the region in general (see also Gunnell and Eddleston 2003; Ratnayeke 1996; Thalagala 2011). Out of these earlier forays into the causes and consequences of self-harm by poisoning, a network of researchers was formed that later became SACTRC.

The work of this group came to attention of Bob in 2003 when a scandal emerged in Sri Lanka amid allegations that a patient had died as a result of being in a clinical trial (see chapter 8; also see Simpson 2012). The case seemed to be particularly interesting as it brought into play a number of local groups with whom he was closely involved at the time. The impetus to follow up on this controversy in more detail became part of an application to the U.K.’s Economic and Social Research Council (ESRC), which eventually became the International Science and Bioethics Collaboration (ISBC) project. For the Sri Lankan element of this project, we were given access and full support for our work by the senior management of SACTRC. In 2008, Salla began a program of interviews and several months of observation in a hospital where several of SACTRC’s clinical trials were being conducted. During the ISBC research (2007–2010), both Salla and Bob built close working relationships
with people at different levels of the organization and have continued these relationships beyond the life of the original ISBC project.

The development of SACTRC began to be formalized in response to a grant call from the Wellcome Trust and the Australian National Health Medical Research Council. One of the aims of this call was to build research capacity in developing-world settings. The application was successful, and the network that was to become SACTRC was awarded a large program grant in 2004. Their objective was to build local capacity for handling poisoning admissions, and in so doing to reduce the high death rates from poisoning. The studies ranged from clinical trials to observational and pharmacokinetic studies. During Salla’s fieldwork, the group had a head office in Peradeniya and were performing studies in six hospitals across Sri Lanka. The studies were run by principal investigators who were a mix of Sri Lankan, Australian, and British researchers and were linked to universities in each of these countries. The group had eight PhD students, some of whom were local and others who were part of the diaspora and returning after the war. There were also numerous managers, coordinators, and clinical research assistants (CRAs), all of whom were Sri Lankan.

The group was also supported by an international network of collaborators across South Asia and the globe. The phase 2 clinical trial on paraquat poisoning that Salla followed was funded in part by Syngenta, the company that produced the herbicide. Although Syngenta contributed to the funding, the researchers were anxious to point out that the company did not have any role in the way the research was conducted or the interpretation of its results. These factors combined to give the Paraquat Poisoning Trial a character very different from the Joint Pain Trial. In this chapter, we give greater ethnographic specificity to these differences as a prelude to our discussion in chapters 5, 6, and 7 of conceptual and epistemic shifts in research practice.

**Hospital Relations and Frontline Researchers**

Access to patients for the Paraquat Poisoning Trial was negotiated via consulting physicians in charge of the hospital wards where the poisoning patients presented. To ease the way, senior researchers and principal investigators would visit the hospital’s senior staff to gain permission and establish good working relationships. At the time of our fieldwork, medical doctors
in Sri Lanka did not need to do research for their career to progress. However, due in part to the research-capacity building efforts of organizations like SACTRC, this picture had begun to change, and more doctors are becoming actively engaged in research activities. At the time, this picture was uneven: some doctors enthusiastically came forward as research partners, but others kept their distance.

Crucially, in the Sri Lankan context, if the physician in charge of a hospital ward did not agree to allow work in his or her ward, it was impossible for any trial work to proceed there. To recognize the considerable power wielded by the physicians in charge is to enter the distinctive and consequential set of hierarchical arrangements that feature in Sri Lankan hospitals. The following statement—in which a physician described his reservations about allowing his patients to be included in a Paraquat Poisoning Trial and why he subsequently changed his mind—shows how, in one instance, “access” to the patients in the ward was negotiated between researchers and the consultant physicians.

First, I didn’t allow research in my ward. I didn’t have exposure to trials although I would love to do them. The reason I didn’t allow the trial was, one, I didn’t think that the objectives of the study were valid. The argument was that with the lower solution of the pesticide there would be more survivors, and therefore more patients would occur who would need to prevent heart, lung, renal, and kidney failure. I didn’t see this as a clinician. I felt that it was an unproven drug on anecdotal evidence. Second, proposing something like this would give the patients and the relatives unnecessary distress [by taking samples] and hope that they might survive. Third, I also thought that pre-interns [who acted as frontline researchers] are young and immature persons. Patients and bystanders are unable to understand randomization and might think it is unfair. I presented these thoughts to the senior researchers who appreciated my opinion. Then a couple of months later I saw the pre-interns going around the hospital and felt guilty. Was I hindering valuable research? I heard that a few patients had actually survived, and so I contacted the researchers to ask if I could still join and joined the contract.

Concern that he might be “hindering valuable research” changed this consultant physician’s opinion on clinical trials on his ward, and his comments signal important shifts in the way that a senior physician sees power and dominion over the wards, staff, and patients. To allow “valuable research”
meant accepting the possible benefits not only to patients but also to staff. His change of mind suggests an important moment in the acceptance of research as a normative feature of life on a hospital ward.

However, although consultant physicians were the immediate point of entry in negotiating research access in hospitals, they were not the end of the story. The CRAs who served as the main trial workers were typically medical interns who had completed their basic medical training. Usually they were waiting to be allocated hospital placements that would carry them on into the next stage of their training. Because they were medically trained, they were more qualified than the fieldworkers working in comparable international collaborations elsewhere. For example, at many African global health research sites—and particularly in those performing observational and public health research—the workers collecting data are often lower ranking health care staff, non-medically trained volunteers, or individuals chosen for their supposed knowledge of the community that is being recruited (Kingori 2013; Sambakunsi et al. 2015). The CRAs in the poisoning trial had to work closely and carefully with ward doctors as well as the consulting physicians.

The consulting physicians visited the wards twice a day. Although they acted as the primary gatekeeper to the patients, each physician also had about six junior doctors working under her or him, with whom CRAs had also to maintain good working relationships. At the ward, the primary clinical responsibility for the patients was with these “house officers” (junior doctors) who spent most of the day in the wards. They diagnosed the patients’ conditions, prescribed medicines, and followed the patients’ recovery. In addition to the junior doctors, everyday care work was performed by nurses, who took blood, gave out medicines, put on bandages, and, most relevant to the poisoning patients, administered their gastric lavage on admission. There were also attendants who helped with the work of lifting patients, cleaning up after patients who had vomited or excreted, and assisted with the difficult business of administering lavage.

Given the number of personnel and the challenging tasks they faced, messages about research did not always travel effectively from the senior to junior levels. The incorporation of a new category of health care professional whose raison d’être was primarily to perform research rather than therapy was both novel and challenging. The negotiations and clarifications that followed the arrival of these new personnel on the ward often led to tensions.
The nature of these tensions was evident in a conflict that Salla witnessed between junior ward doctors and the frontline researchers or CRAs.

Upon entering the ward, the doctors of the ward (five of them) were all at the doctors’ table and started bombarding questions at the CRAs. “We don’t know what you’re doing to the patient, there are no documents, and we are responsible! Sir [the consulting physician] does not know either, and he will chase you out. You don’t provide the patients with any counseling either! Is the paraquat test even valid? Only from the patient’s ‘bystander’ [usually a relative] we got to know that you are giving some injections.” The CRAs managed to hold their ground well, explaining that information about the trial should be in the patient’s records and that even they did not know in which study arm each participant was. “You have to tell us what you’re doing,” the junior doctors insisted.

In this particular instance, the information about a patient’s participation in the trial had gone missing from his notes—which were a bundle of papers and slips clipped together—and its absence raised suspicions among the junior doctors. They were afraid that something that they might subsequently be held responsible for was being done without their knowledge. The management of these relations defined the smoothness of the operations on a daily basis. A collegial and amicable relationship between the juniors was likely to result in a positive research climate in the ward and exchange of research findings. The CRAs, being the lowest in the pecking order, depended highly upon their confidence in conversing with those higher in the hierarchy and upon their clinical and research skills. An absence of communication could lead to situations like the one described here, where neither the senior researchers nor the consultant were available, which left the junior doctors and CRAs to sort things out as best they could.

At this point, questions of research ethics and governance seemed a long way off, and more prosaic issues of accountability and patient responsibility came to the fore. As many trial coordinators and CRAs were themselves quick to point out, grafting a trial onto the complex politics of day-to-day ward life might have begun with the agreement of the consulting physician, but it was likely to entail ongoing negotiations between junior doctors, CRAs, and other ward staff.

The CRAs, as we have already seen, were typically medical intern doctors using the year-long gap between the end of their studies and beginning
of their placement as productively as possible. This hiatus in trainees’ medical careers had arisen as a result of a backlog generated by the efforts to increase the number of doctors coming out of medical school. The need for a greater supply of medical students was in turn linked to the political conflicts of the early 1990s, which saw medical training disrupted due to the closure of many universities during the civil unrest and ongoing insurrection. Nowadays, the enforced gap year saw medical students keeping their hand in by gaining experience working as CRAs on clinical trials.

Although these interns were not fully qualified as doctors, CRA work gave them valuable experience and the opportunity to learn more about medical research. They were not in charge of patient care, but the CRA position meant that they could see research participant-patients on a daily basis in a hospital environment. Some of them even had the opportunity to carry out small observational studies for themselves. Their employment as research assistants in the toxicology collaboration usually lasted between six months and a year before they moved on to do their internships, after which they would go on to graduate as doctors.

From their position, the CRAs could explore the option of becoming a career researcher. However, research is not well recognized or supported as a career path in Sri Lanka, and there were strong pressures to pursue a more conventional career in medicine. The CRAs were also not quite so beholden to the senior researchers under whom they worked. Unlike junior doctors, if conflicts arose for CRAs they could leave without any great detriment to their career, if they wished. The repercussions for a junior doctor in a similar situation could result in a posting to an unfavorable location or the blocking of career progression.

In short, the CRAs were the key frontline researchers. They did most of the day-to-day data collection, and they were responsible for the negotiation of research ethics in practice. The CRAs worked either day or night shifts, with two CRAs working together during any shift. Where Salla did her fieldwork, there were six CRAs in total working on a three-week rotation: two people on a day shift, two on a night shift, and two on leave. During their shifts, they recruited new research participants, took them through consent procedures, and collected preliminary clinical data. In practice, the CRAs modified their version of the consent routine according to the patients’ understanding of the research and depending on which study the patient was being recruited for. Often the dialogue would be short, with the CRAs
explaining to the patient that what they had taken was toxic or lethal and simply asking for their agreement to participate in research. For this request, they would use the expression “Kemathida?” which translates as the rather passive “Do you like?” in the sense of “mind” or “accept” being involved in the research. (The recruitment procedures will be discussed in greater detail in chapter 7.)

The trainees who took on the CRA roles for the Paraquat Poisoning Trial were typically drawn from all over the island, and the work on the trial brought them together in lodgings. For many unmarried young people in Sri Lanka, living away from their parents like this is unheard of. Mindful of this fact and also keen to provide a sense of community for the CRAs, SACTRC paid for a house (which became known as the Study House) to provide a place where all the CRAs could live while on site. At the same time, the group lodging allowed the managers to help the CRAs in more pastoral ways and to have oversight of the CRA team. This work-focused domestic community had a mother figure in the form of a housemaid who cooked the meals and did household chores at the Study House. Despite the fact that young and unmarried men and women were cohabiting—an arrangement that normally would be frowned upon—the parents of the CRAs were prepared to allow this. They were confident that the young people were being supervised and that they were gaining good work experience and a salary that they would not otherwise have had.

Above the CRAs in the organizational hierarchy were the research coordinators. These individuals were mostly science or pharmacy graduates employed on longer-term contracts. Their role was to prepare the compounds to be tested, to perform the quality control monitoring of the data, and generally to oversee the work of the junior CRAs. Many of the coordinators described their employment and personal experience in very positive terms; they found themselves working in what they felt to be a good support network with opportunities to develop their own projects. In line with SACTRC’s original capacity-building aspirations, the coordinators could also see future research possibilities emerging from their current employment in the form of further research work or study, either in Sri Lanka or abroad.

The research team had a small office near the hospital that served as their headquarters. From here, the CRAs would report for work and walk to the wards where the trials were being run. The wards were divided into men’s and women’s sections. If poisoning patients were brought to the hospital after
5:00 p.m., they were channeled to a receiving ward for preliminary assessment. If a longer stay was needed, they would be moved the next day to other wards. At night, the receiving ward became especially busy. There was a shortage of beds, which caused it to become overcrowded. At the entry to each ward, there was a book with the names of the people who were in the ward and their conditions, and it was from this book that the CRAs checked to see if any new poisoning patients had been admitted in their absence. There were usually between one and five new patients in each ward per day.

In this set up, the junior CRAs were quite clear about the purpose and objectives of the research they were undertaking, and they invariably expressed it in terms of helping patients. Moreover, they would link the idea of helping with the Hippocratic Oath that they would have recited as medical students, perhaps in their graduation ceremonies only months before. As we will explore in more detail in chapter 7, however, encountering the extreme suffering of patients who have ingested poison is extremely challenging. The vague exhortations to “do no harm” provided only the most general of ethical pointers when junior staff found themselves caught between a desperately ill person and their role as primary recruiters and monitors of subjects for a clinical trial.

In short, the CRAs found themselves in an ambiguous role. As researchers, they were primarily observers, but they would often find themselves witnessing distressing scenes and being beseeched by family members to actively intervene and help the patient. Helping was not their role—their priorities lay elsewhere—so scenes of CRAs offering whatever consolation and support they could to the families tended to take place when more senior researchers and management were not around.

Research as a Lifestyle

Among the SACTRC team were also a number of doctoral students engaged in hospital-based clinical research. The majority of these PhD students were either Sri Lankan or of Sri Lankan descent, and they were funded by the Australian National Medical Research Council/Wellcome Trust research-capacity building grant.

One of the doctoral students admitted that he knew nothing about research before joining the PhD program. He had graduated from medical
school ahead of his cohort and then traveled around the world working as a
doctor and surfing, his favorite sport. Although he was of Sri Lankan de-
scent, he only knew the country from his parents’ stories—they had fled to
the United Kingdom and subsequently to New Zealand during the conflict
that engulfed Sri Lanka in the 1980s. When the opportunity arose for him to
do research in Sri Lanka, he saw it as an opportunity to learn about his
roots while further developing his passion for medicine. He said, “In aca-
demia, doing a PhD is like a big intellectual thing, but in medicine you are
seen as the . . .” He made a funny face and screwed his finger on his temple,
indicating that it was seen as foolish for a host of reasons ranging from
financial to ethical. “Why bother?” he continued. “You could have a good
position, and drive a jeep. It’s one thing to treat patients . . . [and another] to
do research on them.”

Many of the research staff were weighing the choice between a well-paid
and respected medical practice and a research career that was likely to be
uncertain and difficult. One PhD student described the engagement with
research in biomedicine as a “lifestyle” rather than a job, conveying the way
in which it demanded both a particular attitude and a consuming commit-
ment:

I could be earning a high salary at one of the private hospitals and not wor-
rying about these things. I end up having to deal with a lot of the CRAs’ con-
cerns. One really needs to love doing research; otherwise, it is not worth it. I
get whipped in the back by my superiors about my thesis; they are very thor-
ough and ask for little, little details. It’s a choice between having a lush life-
style, or finishing articles in the evening. SACTRC is not a job, it’s a lifestyle,
and it is an ongoing thing; you’re never quite out of work and out of the loop.

A third student, Sri Lankan by birth, went to complete his PhD in Aus-
tralia and ended up staying there, returning to full-time medical practice.
As Timmermans (2010) has described in the U.S. context, research provided
a pathway for career advancement among the younger doctors. In the case
of the Australian émigré, the benefits of local medical training were lost to
Sri Lanka en route to employment in Australia.

For many, the choice of a not-so-lucrative, not-so-high status career was
justified in terms of humanitarianism and a rhetoric of alleviating the suf-
ferring made all too visible in global health projects. One of the PhD students
said that when he had first started research in Sri Lanka, he was struck by all the things that were wrong with medical services, ranging from malpractice to ethics: “You can’t just be research-minded. For all the things wrong there is always something else that is worse. It is complicated to develop one area when others would remain in the state that they are. Can it be effective when other areas are so poor? But rather than critiquing, to change the system from outside, it should be worked from within.” Pointing to the overall systemic structures, the experience of Sri Lankan hospitals led this student to make attempts to move beyond a simple scientific hypothesis and extend his research in novel, multidisciplinary directions that include policy, advocacy, and knowledge transfer structures.

For others, the challenging nature of the work translated into something akin to medical machismo. Working at the sharp end of global health inequalities was not just a humanitarian duty but also an adventure, which required one to be able to handle the “toughness” of the setup. Working in these settings not only imposed physical demands but also required emotional discipline. As one of the SACTRC principal investigators summed up the situation,

Different individuals have different emotional approaches to the work that they do here. [PhD student X] really struggled here. He’d get really emotional and intense about all things that went wrong and say, “How can you just stand beside and look and not do anything?” [Researcher Y] had a bit of that as well, and he was very junior at the time . . . My take on this is that you can’t get too involved, that is like the helicopter approach. You can’t just save one and then go away; it’s not a sustainable approach. I am very critical of “helicopter research.” Some foreigner comes to have a bit of action and tell how things should be done and flies away. I think we think that if the project does not help people, there is no point doing it. Like in aid, giving money is not going to help them to improve medical care. I hate that missionary doctor approach. I believe in sustainable change; we can do that when you make a change in practice.

In settings where the local medical capacity is deficient, doing research can be seen as detached and frivolous, drawing the researchers away from the immediacy of administering help to distressed patients. But, as this researcher makes clear, being able to manage the feelings that such intense environments generate was part of the emotional work that their jobs required. Too much
feeling and the researcher might become too involved with the immediate rather than the bigger picture—which in this case meant the promise of sustainable, evidence-based solutions.

However, despite the goals of capacity building, career development, and sustainable interventions that the collaborative approach created, the research funding cycle rendered their work precarious and unpredictable. The director of the group regularly bemoaned the “even bigger” picture in which they all worked. He said, “While creating capacity, the project may still be developing dependency on the international research circuit.” The uncertainties and fluctuations of global research funding thus might lead to an inability to develop long-term strategies and continuity for the projects and staff. This precariousness meant that the researchers were always worried about their careers and prospects, and they often felt pressured to publish as a way of showing that they were achieving their targets.

In this section, we have introduced the organization SACTRC and discussed how, through international collaboration, it worked to introduce an ideology of research practice consistent with broader attempts to address global health inequalities. Achieving this aim involved a subtle blend of distance (from the immediacy and intensity of the presenting problems) and closeness to local context (to ensure that knowledge and skills were passed on and appropriately embedded in local practice). Here, the work of researchers from outside the country was to generate an infrastructure—material, social, and intellectual—that would direct the benefits of research into rather than out of the country. To get this formulation the wrong way around was to risk carrying out the aforementioned “helicopter research”—an unethical and unsustainable approach that would smack of neocolonialism.

However, an appropriate local infrastructure is but part of the story. In the next section, we turn to a closer consideration of the relations that made up the collaborative endeavor.

Trust

One question that Salla regularly put to the researchers was, What makes a good collaboration? The director of SACTRC answered the question in the following way: “I guess, firstly, some sort of agreed common purpose or goal among the people who are involved. It’s certainly very important because I
think it underpins one of the other things, which is that if you can agree on common purpose and goals then it’s a lot easier to develop high levels of trust about what all the players’ motives are.” Significantly, in this view trust follows common purpose and not the other way around. This view was echoed by one of the SACTRC managers, who also placed clarity of purpose high on his list of explanatory variables: “You have to have a common goal and object. You have to identify the right people. Each member needs to have their task, their role in the project. Good communication needs to be the primary thing, and all things should be addressed up front. You need to find the right people who have the interest from our joint protocol, to give authorship acknowledgement and to work together.” With clear purpose, the personnel and other aspects of the trial would fall into place. As the director also opined:

To have a good collaboration you need to have a high level of trust, because in clinical research things happen. You can plan as much as you like, and you try to communicate things at various levels, but because of the complexity it’s inevitable that there will be bumps. When you have those sorts of bumps you need to have trust so that you can have a situation where others can say, “Look, I’m pissed off.” Everyone’s trying to move ahead with the very best of intentions, and there has to be this common purpose, so you need that level of trust.

The clear-cut teleology of the trial, it would appear, was replaced by a rather more contingent working out of relationships, which fell back on trust and people’s ability to suspend judgment and action long enough for alternative and perhaps more collaborative readings of other’s behavior. In this respect, trust was seen as key to the social fabric that held the team together, particularly when difficulties were encountered.

Trust is what enabled allowances to be made and relationships to continue into the future despite what at any particular point might have seemed good reasons not to continue the relationship—what were referred to as the “bumps” in the road. In considering the work that the notion of trust does in relationships, as opposed to the more elementary question of what it is, Alberto Corsín Jiménez suggested that under conditions of doubt and anxiety, trust has come play the role of placeholder for robust knowledge, the knowledge based on the certainties of evidence, audit, and accountability (Corsín Jiménez 2011). Trust, he suggests, is an “engine of epistemic distance-
compression: where knowledge, responsibility and mutuality collapse into an identical social form” (Corsín Jiménez 2011, 178). According to this metaphor, trust might serve as the “engine,” and the “social form” that it drives looks very much like the collaborations that SACTRC was trying to establish.

However, what is of interest in the context of these collaborations is their international character. “International” here signals particular kinds of distance to be compressed—and very different readings of knowledge, mutuality, and responsibility come into play. Consequently, the bumps that threaten ongoing trust may be of rather different scale and form when working internationally and depending on where one is within the collaborative assemblage.

One of the areas in which we encountered frequent misreadings of collaborative intent was in relation to the management of hierarchy. In professional encounters, Sri Lankans are rarely expressive of their feelings. Confrontation is generally avoided, with conflicts sublimated and managed through compliance and passivity (for example, see Chapin 2014, 103). With these sensitivities in mind, good collaborations were seen as ones where a shared vision was underpinned by efforts to anticipate the local sensibilities. The ideology of collaboration as enshrined in earlier visions of the scientific norm was one in which there were ideals of openness and accessibility underwritten by presumptions of a flat and flexible community of co-working scientists. In Sri Lankan medical circles, on the other hand, relations are typically hierarchical and segmented, with high levels of loyalty presumed to exist between seniors and their junior staff.

Given such steep power gradients, confrontations were best avoided, and there was a good deal of compliance, even when juniors felt doubt or a lack of confidence in the actions of seniors. Suspicion and jealousy also are common, along with the idea that corruption is rife beyond the immediate world in which a person operates. Foreigners, in particular, are treated with caution. This is hardly surprising given that for most local doctors and administrators these international collaborations would often introduce levels of resources that could prove very disruptive when released into local organizational hierarchies.

Collaborations also take on organizational forms that local staff may find difficult to read. For example, SACTRC presented itself to local audiences as a “collaboration” rather than an institution, and, moreover, as one that es-
chewed hierarchy and presented itself as a loose confederation made up of little more than the relationships it comprised. This way of presenting SAC-TRC created problems for how power and authority were read. Against this backdrop, attempts by the SACTRC management to work in a spirit of open communication with local hierarchies could be a cause of confusion. For example, the junior and frontline researchers at times expressed unease at the lack of clarity regarding the structures, responsibilities, and authority in their work.

In institutional settings that are often conservative and bound by convention and protocol, this foregrounding of people and relationships rather than organizational form was troublingly radical. Thus, despite the attempts to establish flat and fuzzy ways of operating, the subtleties of working across divisions of labor were not without tensions and were apt to be reconfigured by local researchers into the more familiar hierarchal mode.

Authorship and Publication

A story related by the SACTRC director captures some of the sensitivities that underlay what, on the face of it, was a clearly defined and perfectly amicable relationship between a consultant and the research team. It concerned publication both as an index of efficacy of collaboration and as a marker of academic prestige. In an interview, he described the background to the misunderstanding:

When we had a joint research meeting where we mostly discussed intervention type stuff, [we] asked if one of the consultants [physicians] was interested in interventional studies, and he said, “No, no, no.” I said, “That’s fine, it’s good that we know you don’t want to do that. You are in charge.” The study that we did in his ward—with his blessing but not direct involvement—went on without him, findings were published, and he was acknowledged in the publications. But nonetheless really what evolved out of all of this was that he felt that we didn’t value him and that we ignored him, and he felt that he had not been acknowledged enough. Part of this was because probably he had not even looked at the papers we’d sent him where we’d acknowledged him. Because of this, he decided to punish us. He was not going to allow any research in his ward at all; now he was really in charge. We spent a number of meetings trying to understand what the issue was. It took maybe seven or eight
months for him to finally come out and say what the issue for him was. I decided to write a short but simple letter to say that I was very distressed that he’d felt that he hadn’t been acknowledged and to try and explain the process of research and that there weren’t ten papers out of that research which he wasn’t acknowledged in. There was only one, and he was acknowledged in it. Interestingly, a few days later he allowed us to come back into the fold.

This example illustrates similar issues about territories and power to those described earlier in relation to the physician uncertain about accepting CRAs onto his ward. Here, however, a misunderstanding about acknowledgment resulted in the closure of a trial site for several months. In a society that pays such careful attention to the observance of hierarchical etiquette, not being included as an author was taken as a significant rebuff.

Given the importance of patterns of authorship as providing “transactional traces of past collaborations” (Strathern 2012, 119) and the common desire to collaborate to gain access to publications, international opportunities, and recognition of one’s work, the ward physician’s response was hardly surprising. It suggests a pattern of sensibilities that do not match straightforwardly with those of the researchers. Publication was also an issue that frequently exercised members of SACTRC, as the director opined:

I think we try, when papers come out, to send copies to everyone who’s been involved in the paper, whether that’s an author or whether that’s people who are acknowledged in the paper. When you’re in the collaboration, you say, “Look, we’re going to acknowledge the work you do.” It depends on people’s understanding of that. If they’re co-authors on the paper, then they often know that the paper is in preparation. Normally you tell people that this is what we’re doing. Everyone’s work is acknowledged; these are authorship rules. If you want be involved as an author then there are certain things you have to do. People then have to understand that. Perhaps in the physician’s view, acknowledgment was that he would like a silver plaque. Perhaps that’s a good idea. People should have all got something they could stick up on the wall. That’s a very Asian tradition. From an Australian perspective, I think it’s very nice but unnecessary to have that level of acknowledgment. But at the end of the day if you’ve already got 130 publications it’s not really an issue, but if you’ve got two, it is . . . I’m guessing on the spectrum of complete trust to complete suspicion, you know, probably a certificate would be very useful to make the relationship concrete.
The director’s further reflections on the importance of acknowledgments and transparency in recognizing everybody’s role in publication indicate the cultural mismatches that may occur in how acknowledgment is enacted.

The use of ghost writers in clinical research (Sismondo 2009) as well as the rejection of gift authorship has led the International Committee of Medical Journal Editors to draw up clear guidelines as to who should be acknowledged as an author. Including all who have contributed, ranging from junior and technical staff to project principal investigators, is an important objective of these guidelines. Those who have few papers and for whom building a curriculum vitae is crucial may feel upset if they feel that they are being used as facilitators and data collectors rather than being included as authors. Although this sort of critique was not directed at the international research in Sri Lanka, several researchers of Sri Lankan origin did mention that it was common in Sri Lankan academia at large for seniors to expect their juniors to do all the work—for which they rarely got credit. The importance of intellectual support from seniors was also underlined by another of the SACTRC managers:

Publication is one of the main parameters of success [of the collaboration]. We have had about twenty to twenty-five papers each year. This year we’ve already had eight, last year we had eighteen. That’s been a real success. Usually in Sri Lanka, supervisors take a long time to comment on papers and generally do not discuss things carefully to their students and provide intellectual support. Whereas, these supervisors make a change by putting their effort and sharing their experiences. Finally, we had funding flexibility and statistical support that allowed us to publish so much.

All the collaborators in SACTRC had agreed to work within accepted authorship rules, which included an early opportunity to be involved in research and papers in their area of interest. In the collaboration, authority was distributed based on effort, but there was also pressure to publish. Despite the collaboration being a nonprofit research body, its funders were seen to operate on a commercial output-driven logic of reward. Where publication and authorship were concerned, there were no discounts when it came to the demands of working in a low-resource, high-pressure setting. Those who in their role had found themselves spending a lot of time building the social infrastructure for the studies in the hospitals were at times criticized for their slow publication rate.
In sum, working to address global health inequalities through international collaborative ventures of the kind described here would appear to follow certain principles: sharing a vision, building on existing knowledge and experience, choosing partners with whom one can work, establishing clear roles and relationships, building trust and friendships, maintaining good commitment, and ensuring that collaborators are recognized in the outputs of the research. Yet the attempts to realize these principles by blending an open and democratic mode of working with local conditions presented some considerable challenges. We saw a wide spectrum of relations in play. These ranged from the thrill of working in difficult circumstances on mutually interesting and exciting research questions with companionate colleagues through to emotionally draining rounds of persuasion and diplomacy to ensure that access to the hospital wards was secured and maintained for the duration of a trial. Communication could easily falter, and mutual expectations would fail to be met.

The international and Sri Lankan collaborators were keenly aware of these difficulties, and they were perhaps unusual in the extent to which they incorporated an awareness of the issues into their practice. They were eager to challenge what they saw as the colonial legacy of tropical medicine, which cast the developing world as a place that is acted upon, rather than a place that performs the actions. Although they made an effort to eschew the hub–spoke, north–south, donor–recipient models of development by using collaborative work as their vehicle, the power relations still continued to be written into the modalities of collaboration in practice. Underlying the scientific aims of collaboration are a plethora of other assumptions, which may surface in day-to-day interactions. These interactions speak to the different scales of the research-as-development nexus, and they point to the importance of mundane, day-to-day relations in research practices. Here we see the importance of personal networking, securing employment for oneself and others, accessing training, and getting into positions to control the flow of resources to individuals and institutions.

The managers of the collaborations we studied were not unaware of these challenges, and they made attempts to rationalize them into short- and long-run strategies. Doing so required mindfulness of global research as part of a global assemblage of institutions and relevant research and health care practices as well as an ongoing evaluation of the relationship between local and international research cultures.
Collaboration in Practice and as Practice?

In this chapter and in chapter 3 we have provided a descriptive account of the running of two clinical trials. The trials we studied were both nested in socio-technical networks that used the same modality of knowledge production: randomizing a small number of patients as trial participants to see whether a particular drug worked. The trials shared a common methodology and scientific rationale, but we have shown how they were very different in terms of their purposes and the contexts in which they took place. They varied in the networks they tapped into locally and internationally, in who funded them and in what ways, in where they looked for ethics guidance and regulation, and in who they viewed as being the ultimate beneficiaries of the trials.

The poisoning trial was established with the aim of addressing a humanitarian crisis in Sri Lanka. It was broadly situated within concerns about the large discrepancies in health research funding directed at developing-world health problems, and it was initiated by international researchers with an interest in medicine in low-income settings. The research would generate important benefits in the form of academic publications and improved clinical guidelines for the treatment of poisoning admissions, which would help not only Sri Lanka but also other low-income countries facing similar problems.

The specific challenges for collaboration that the team members faced had to do with social relations and working within a hierarchical health care system that had limited experience with clinical trials research. To initiate and maintain the necessary social relationships required the cultivation of trust, particularly given that the motives of outsiders were apt to be viewed with suspicion. Differences in cultural background were flattened out by the emphasis on a common purpose in terms of the scientific enterprise, which was often expressed in sentiments such as “We’re all scientists, and that’s what counts—if we get along and you’re good in what you do, we can collaborate.” Ideally, the researchers gained no concessions for being from a “developing country”—science and its scientists would be as good as anywhere in the world, and science was presumed to be the medium they all had in common. In theory, this left no space for prejudices based on ethnic background, cultural traditions, or nationality. The ways in which the people who were involved in the collaborations talked about their groups suggested that it did
not matter where someone was from so long as they wanted to work together, knew what they were doing, and got along well on an interpersonal level. These attitudes coincided with the research ethic that emphasized the broader benefits of the research and its broadly utilitarian purpose—seeking to gain the maximum value for the maximum number of people.

In the industry-sponsored trial, the dynamics were somewhat different. The initiative for developing the trial came from Sri Lankans, so the trial was embedded in local relationships in a very different way from the poisoning trial. In the industry-sponsored trial, the assimilation of international research into local medical settings appeared to be largely unproblematic. The researchers were working with the support of the established hierarchies rather than in opposition to them. As such, the financial means and mutual efforts were geared toward establishing an internationally recognized trial site that would thereby attract more international research. The research teams were intent on gaining experience with research ventures of the type approved by the U.S. Food and Drug Administration and to cultivate practical skills that would enable them to comply with the International Conference on Harmonization Good Clinical Practice Guidelines (ICH-GCP).

Performing trials at this level was used to show international and local onlookers that Sri Lanka was an attractive trial site from both operational and regulatory perspectives. This aspiration was realized by creating a distinctive space and time within the usual operations of a hectic public hospital ward. The pharmaceutical company’s motivation for the Joint Pain Trial was, in the longer term, profit-oriented as the drug was intended for distribution in international markets. The drugs, if approved, would be too expensive for local consumers and would ultimately benefit patients in richer parts of the world. However, in line with the trialists’ shorter-term intentions, the patients taking part in the trials derived immediate benefits from the study.

As in the poisoning trial, the relationships within the team were very relaxed, yet the functioning of the Joint Pain team was marked by adherence to roles and strict compliance with the protocol policed by outside agents. Local researchers were also mindful of structural imbalances between them and the international research community at large. They recognized the difficulties in raising levels of expertise in a context where first-class science education is in short supply, as are the resources needed to carry out world-class research. In other words, it was important for researchers not only to
advance the research but to work simultaneously to address deficits and imbalances along the way and collaboration was their pathway to this.

This strategy is consistent with our notion of first-order development described in the introduction—that is, one geared to setting up research units, hiring and training staff, gaining high-impact attention, attracting further funding, and improving the material settings of laboratories. The trial was treated as a resource-rich stepping stone toward developing other types of scientific research and collaborations. Consistent with this more pragmatic approach to capacity building was an ethical logic that might best be described as one of practical deontology—acting according to ethical expectations. In this case, the expectations were dictated by the protocol and by the ICH-GCP guidelines within which the protocol was framed.

In table 1, we summarize the similarities and differences between the two trials. There were considerable differences between the contexts in which the trials took place, but one common underlying feature for both was what one of our interviewees referred to as the “developing country factor.” Unlike research collaborations that take place in relatively affluent settings, those we have considered here had to work against a backdrop of inadequate resources and a vitiated health care system. Such inequalities posed challenging questions for collaboration in practice: Who controls the research agenda? How can salary differentials between local and international researchers be reconciled? How can the outcomes of research be rendered as local benefits rather than only helping external others? Overcoming these challenges was cited by researchers on both trials as practical and moral reasons for why

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<th>Features</th>
<th>Privately Funded</th>
<th>Publicly Funded</th>
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<tr>
<td>Knowledge</td>
<td>Proprietary</td>
<td>Humanitarian</td>
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<td>Direction of knowledge</td>
<td>Out</td>
<td>In</td>
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<td>Capacity-building aim</td>
<td>Build an international level group, enable further scientific research</td>
<td>Train local researchers and doctors</td>
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<td>Regulation</td>
<td>GCP guidelines</td>
<td>Research ethics</td>
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<td>Ethical logic</td>
<td>Deontology</td>
<td>Utilitarian</td>
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<td>Marketing aim</td>
<td>Primarily Western</td>
<td>Sri Lankan/Asian</td>
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international collaboration was important. Collaboration was a means to gain access to international networks, publication outlets, and funding in the future. But above all collaboration established a viable, sustainable local research culture that could address local problems more directly and as part of a wider development agenda. These objectives, moreover, were strongly underscored by the local ethics committees. In short, our interviewees did not look on collaboration merely as a practice but also as a value (Strathern 2011). Moreover, collaboration carried potentiality: it was strategic, aspirational, and forward looking. Hence, the rhetoric of collaboration began to align closely with that of development, reflecting hopes for a future in which there would be progress and improvement.

This alignment might appear to be self-evident in its effects, but it depended in practice on a great deal of work on the part of the researchers to reshape their conceptual language and ensure that the categories were clear. Without this second-order epistemic work, transnational research would not be operable in the diverse settings in which it lands. In the next three chapters we turn to the question of second-order development and the ways in which the running of the Joint Pain Trial and the Paraquat Poisoning Trial introduced conceptual and epistemic changes in local practice.