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Agents of Fertility: The Ford Foundations' Fertility Research Program Guided by its Biomedical Advisors

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I am most grateful to the RAC as they awarded me a generous grant to investigate the newly acquired archives of the Ford Foundation with respect to their Population Program.

During my archival research I investigated the following sources:

- 1) Oscar Harkavy's office files preserved in 27 boxes
- 2) Ford Foundation unpublished reports, on paper and on microfilm
- 3) Ford Foundation "logs" (unsuccessful grant applications) and grants; amongst these, I processed all cases and projects advised by A. Southam and E. Witschi

My stay in September to October 2013 focussed specifically on the Ford Foundation's efforts to play an important rôle in the field of population politics by building and maintaining a well supported and unique individual grant program from 1959 until the 1980s (cf. Hertz 1984).

This program focussed on support for physiological investigations into the reproductive organs in order to find new ways and means for contraceptive measures. My own interest was specifically on the first half of this period, 1959 to the mid-1970s, as it was in this period that the FF Population Program had the largest international impact (on the early Ford Foundation, cf. Walsh and Atwater 2012; Berelson, Anderson, and Harkavy 1965). For the PhD thesis more particularly, I researched the FF's investment in sterilization and laparoscopy.

I acknowledge most helpful support from Lucas Buresch for the Ford Foundation (hereafter FF) as well as Tom Rosenbaum and Beth Jaffe on Rockefeller Foundation related requests.

My research findings will help other historians to define what kind of rôle the FF played in fertility research in comparison to other global actors such as the Population Council and the Rockefeller Foundation, both run by the Rockefeller Family. In a more general perspective, this research helps to reveal the impact civilian and philanthropic funding had on political decisions and medical and health innovations in the field of birth control in the 1950s-70s. Research at the RAC supports my argument that specifically after World War II, contraceptive and neo-eugenic innovations and efforts were highly influenced by private foundations in the field who in turn were influenced by individual physicians and scientists.

First, I will outline the theory or rationale of reproductive research presented by the FF for their new Population Program. This will allow to trace the theory on the basis of which the advisors acted and on the basis of which Oscar Harkavy, head of the Population Program, justified his decisions. Second, I will describe the work of two of these advisors, and finally three significant projects in some more detail.

1. A Program and its Faces: What Did “Reproductive Physiology” Mean for the FF and How Did It Connect to Birth Control?

Oscar Harkavy, an economist by training (Harkavy 1995), chaired the FF Population Program. Harkavy had a crucial role in shaping the profile of the FF’s policy and ideology concerning reproduction and its control by medical means – a policy I termed, in my grant application, “Physiology for Contraception”. Reproductive physiology was neither a clear term at the time nor an established research field with its institutions. The term and the questions it addressed were unsolved for a long period of time.

Reproductive physiology was investigated both in medicine and in biology. In medicine, the active specialties were mainly gynecology and urology, and the more laboratory-based sub-disciplines of histology and pathology. In biology, researchers came mostly from embryology, endocrinology and institutes of physiology that dealt mostly with cells, nerves, fluids and tissue. The main questions were how all the reproductive organs in women and men functioned at any given time and what their important elements were. While biologists worked on the smaller structures, gynecologists and urologists investigated mainly entire organs such as the tubes, the ovaries, or the sperm ducts in men.

The Ford Foundation became convinced, after a few investigations, that its funding rôle should be that of boosting research on reproductive physiology (whatever the term covered) in order to access new knowledge to create new contraceptives of all possible kinds. In short, research into physiology should lead to better population control.

When Oscar Harkavy started this Population Program, he depended heavily on experts from medicine and the biomedical sciences to get an overview of current research and to help define what the FF should be aiming to fund. In order to progress quickly, he invited some of the most noted fertility and contraception researchers of the United States to present opinion papers on their topics. Their 1959 workshop was an important step in formulating the Population Program.

On 9 June 1959, the FF convened a meeting of experts at the FF headquarters in New York City^j. Invited to this seminal meeting were Willard Allen, Lewis L. Engel, Reuben G. Gustavsson, Allan T. Kenyon, C.N.H. Long, Warren O. Nelson, and Gregory Pincus. The most prominent participants were Engel from Harvard and Pincus from the Worcester Foundation who had both earlier attracted global attention to their research.

When considering the “state of the field”, members of the workshop agreed that “attempts to control conception by steroids” needed additional scientific research. Furthermore, the participants agreed that “the state of present knowledge” did not allow to define “chemical and physical processes” during the “reproductive cycle”: reproductive physiology was not clearly understood. This program was completely in tune with research dynamics in the gynecological infertility community described two paragraphs above. Concerning possible avenues for FF research, it was considered obvious that the “list of the 1958 grants of the Population Council’s Medical Division” supplied a reliable catalog of the best institutes carrying out research on human reproduction. Members of the FF meeting looked with great expectations towards the Population Council’s budget for 1959 (on the interdependence between Pop Council and FF, cf. The Population Council 1965, 1978; Huang 2011; most recently Abrahamson 2013).

With regard to the papers given, G. Pincus described his Puerto Rican field trials and he was eager to state that the Pill was safe to take: “many of the reported side effects [...] diminish with time of use”. There were no “pathological side effects”. Under the heading of “Obstacles of Research”, it was discussed that the “principal obstacle is the lack of trained personnel”, especially MDs in comparison to PhDs. It was mainly PhDs in “biochemistry and physiology” who “carried” the field. The group identified a particular obstacle to clinical research by MDs in the liability of physicians for any “unforeseen adverse consequences” of experiments in the United States.

The meeting’s report underlined that organizations including the government spent \$900,000 per year on medical research geared towards population control, of which \$215,000 was provided by the Population Council. However, the FF deplored that the NIH, for instance, only supported research explicitly not related to birth control.

The FF program also identified “investigators overseas” as a possible group of grantees, especially with regard to the situation in the medical disciplines. In conclusion, the FF decided to support bio-medical efforts in a broad sense, be it MDs or PhDs, who worked on the physiology of reproduction, or more directly on birth control methods.

By 1969, the focus had shifted somewhat. Experience with working with MDs made the FF define the research to be carried out no longer as “physiology” but as “biology”. The “Foundation strategy” communicated in 1968ⁱⁱ declared: “We continue to assign highest priority to a greatly expanded worldwide program in reproductive biology directed toward the

development of radically new concepts in fertility control. Our estimate of \$ 150 million a year as an optimum level of support for biomedical research in fertility control is taken seriously by responsible government officials.” The FF would contribute to these efforts a budget of \$ 8 million in fiscal year 1969.

The emphasis on physiology in 1959 and on biology in 1968 shows that there was a gradual shift in understanding reproductive research within the FF. It is likely that over the years, the more biologically oriented projects looked more promising. Margaret Rossiter’s research at the RAC in 2012 (Rossiter 2012) identified a similar change of mind in Warren Weaver, the Rockefeller Foundation’s program director responsible for biomedical sciences and one of the main actors in defining “physiology” for the twentieth century. During his lifetime, Weaver became more favourably inclined towards funding projects in the history of science. Likewise, Harkavy shifted from the term “reproductive physiology”, in the 1950s much more medical than biological in definition, to the term “reproductive biology”.

2. Emil Witschi and Anna Southam as Key Advisors to the FF’s Population Policy Program Run by Oscar Harkavy

2.1 Emil Witschi and His Networks

It is within this framework that we need to understand the following case, the rejection of an MD’s project to set up a unit of reproductive physiology research in Tel Aviv in 1966-68.

Emil Witschi (1890-1971) was an advisor to the FF from 1962 until 1967. Even though his work as a scientist and embryologist has not been highlighted in histories of embryology so far (Laubichler and Maienschein 2009), his importance as a networker for the FF cannot be overstatedⁱⁱⁱ. Witschi did this FF work in addition to his laboratory obligations at the University of Iowa. His daily work and professional career were concerned with research on the development of the genital organs in the embryo, and the embryonic stages of development. For the former, he was an appreciated authority in the German speaking countries and contributed to medical handbooks.

However, his FF activity made a much bigger impact than his scientific research. When he was asked to rate proposals submitted to the FF for funding, Witschi relied mostly on notions

of popularity of the researcher in the international community, on his own respect for the researcher, tied to some extent to the first factor, and on personal friendships.

The following studies of two proposals submitted to the FF will show how Witschi supported or turned down equally interesting research projects mostly on grounds of personal preference. This is an instance for demonstrating how seemingly objective rating approaches even of private foundations such as the FF depended on one advisor's decisions. It needs to be made clear that this system was successful in awarding highly important grants to selected individuals, for example Robert G. Edwards and Egon Diczfalusy (cf. Powledge 1985, 22-28, 62-64; Harkavy 1995, 124 note 8). What follows describes two cases in which the advisors' strategy of relying on their own networks failed to support the promising projects.

2.1.1 Witschi on De Watteville

The following project was supported by grant # 06400411^{iv}, based on Witschi's positive attitude towards the project leader, Hubert de Watteville in Geneva.

On July 17, 1964, Hubert de Watteville (1907-1984), chair of gynecology at Geneva, submitted a project proposal to the FF that would be approved and subsequently funded over a period of more than a decade to a total amount of \$ 748,515. The project investigated, in brief, how and why couples in Geneva use contraception. Similar questions were the subject of many statistical papers in the 1960s and as such, the project was entirely following the trend of the time.

However, from a methodological standpoint, the project description was rather weak even by contemporary 1960s standards, and a confusingly heterogeneous team consisting of "two sociologists, one demographer, one psychologist, one psychoanalyst, and one gynecologist [de Watteville]" did not obviously promise answers to the question of "opinions, attitudes and behavior of individuals, and of couples, [with] regard to the problems of sexuality (biological and emotional), of reproduction, and of the creation of a family." Moreover, the project was really outside Watteville's scope of gynecological competence. He was known for hormone research and never investigated the action of barrier methods of contraception or the IUD. Neither was he an expert in demographic research.

In the first three-year period of the project, “some thousand people who are in the reproductive age” drawn from the “population of Geneva” were to be investigated. First results were presented at Witschi’s 1966 Venice conference where a paper on the “Psychosociological aspects of contraceptive methods”^v was given in panel 10 on “Immediate and Long-Range Aims, Local and Religious Conditions or Problems: Evaluation of Recent Experiences”. In this panel, Watteville and his team joined illustrious figures such as Christopher Tietze from New York (on Tietze’s impact cf. Caulier 2012).

The Watteville team used the following methodology: a questionnaire was handed to participants with the request to return it completed. “At the same time, samples of each group will be investigated first by the psychologist and then by the psychiatrist.” Amongst other aspects, the group sought to “establish the conscious and unconscious motivations for the use of contraceptive technics [sic]”. The study aimed at classifying patients into types of personality liable to reject or accept contraceptives. Second, the study wanted to clarify whether patients who had received psychiatric treatment had a different attitude towards contraceptives than “the rest of the population”. A “psychiatric type of personality” should be defined and its behaviour should be assessed.

It can easily be seen that this project does not fit into the FF’s program profile to support either biomedical research on physiology of the uterus, ovary or tubes or the development of contraceptives for the use in population control. At best, Watteville’s study can be seen as a first step towards implementing new contraceptive measures in a given society. In that case, it is highly disputable whether Watteville’s group of patients “who have had psychiatric treatment” in comparison with patients without previous psychiatric treatment is a useful research sample for developing countries where neither the practice nor the access to psychological or psychiatric treatment is comparable to certain industrial countries. And even in industrial countries, the percentages of psychiatric treatment or identified psychiatric problems in a given population has varied considerably.

The FF’s commitment to Watteville’s research project can only be understood through acknowledging Witschi’s major role in supporting Watteville as an illustrious researcher and as a personal acquaintance.

2.1.2 Witschi on Halbrecht

By contrast, Witschi was very critical towards a biomedical project proposed by Isaac Halbrecht in Tel Aviv^{vi}, a gynecologist with a proven track record of research in infertility. Witschi's less favourable position towards Halbrecht resulted in this Israeli application being turned down.

Isaac Halbrecht (1906-2006), Head of the Department of Obstetrics and Gynecology at the University of Tel Aviv, wrote a first letter to Oscar Harkavy on June 18, 1964^{vii}. He referred to meeting Witschi on his recent visit to Tel Aviv and discussing with him the creation of a "combined Israeli-American project" of an Institute for the "Study of Reproductive Physiopathology" encompassing his Tel Aviv department and the Department of Obstetrics and Gynecology at SUNY, New York, headed by Lou Hellman. Halbrecht mentioned the support of global infertility doctors of the time for this project: Tesaro from Italy (who had organized the Second World Congress on Fertility and Sterility in 1956), Botella-Llusia from Madrid (one of the oldest endocrinologically trained gynecologists) and Louros from Athens. According to Halbrecht, these professors were immensely supportive of an Israeli center dedicated to "specific geographic and regional problems of Reproductive Pathology".

In order to discuss the project further, Halbrecht asked Harkavy for an appointment in New York in July. On this request, Harkavy's advisor Witschi communicated to Harkavy that he would be "very much interested to hear from you after he [Halbrecht] has paid his visit" to the Ford Foundation^{viii}. Harkavy did not meet Halbrecht in person but instead sent Nicholson E. Eastman, an expert in childbirth and prenatal care, to talk to him. Obviously, Eastman was not entirely familiar with Halbrecht's gynecological specialty of infertility research.

Eastman reported back to his boss in a derogatory letter^{ix}. Eastman said he had spent "an hour or so talking to him". Eastman displayed his ignorance of the field of fertility research saying that Halbrecht had "apparently been appointed as Professor and Head of Department [...] in the new medical school being constructed in Tel Aviv" which "according to Dr. Halbrecht, will be a very fine medical institution". Halbrecht had been "extremely vague" about the nature of the "research in the physiology and pathology of reproduction" he would like to carry out. Moreover, Halbrecht had not stated how he wished to work with Lou Hellman at SUNY. Eastman reported that after Halbrecht had left, he had given Hellman a telephone call. Hellman "was just as hazy about Halbrecht's program as I am". Eastman also had concerns about Halbrecht's age which appeared to be "65 or so". Eastman had concluded the meeting by passing Halbrecht on to Witschi, telling him that "Tel Aviv lay in Emil Witschi's territory"

and that Halbrecht should explain to Witschi in more detail what exactly he wanted to investigate.

From Eastman's comments, it becomes clear that he did not know that the "physiology of reproduction" was rather clearly defined in gynecology even at that time: apart from the capacity of the tubes, the ovaries and the ovulation process were subjects of intense study in gynecology. It is therefore possible to imagine that Halbrecht saw no need to elaborate in more specific terms. Second, Halbrecht was a major figure in the field: he had been a friend of Isidor C. Rubin, the internationally recognized infertility expert at Mount Sinai, before Rubin's death in 1957, and had practiced insufflation, an early method for diagnosing tubal damage and for investigating the Fallopian tubes. Even though Halbrecht's department was new, he was not at all new to the field of reproductive physiology.

Halbrecht submitted a completed application form shortly after meeting Eastman, as required. He proposed four distinct research projects: first, investigating the side effects of the Pill and the different IUDs on "different ethnical and religious groups and social strata"; second, to investigate thrombosis as a result of the Pill (a new and controversial topic); third, cultures of placental cells for immunologic investigations (which was cutting edge research at the time; also carried out at Cambridge) and finally, the effects of atmospheric contamination from nuclear fission products on fertility (the effect of atmospheric pollution on the body as a much less dangerous form of atmospheric contamination is now being taken more seriously; cf. Valentino 2014).

In addition, Halbrecht wanted to include the "physiology of reproduction" in his teaching programs. "It is our intention to broaden as much as possible the teaching program of physiology of reproduction for the undergraduate medical students with special emphasis on the problems [of] population growth, birth control and to give the proper training to handle [these] problems." He intended to train postgraduates as "special teams which will serve as nuclei of birth control centers in Israel and in Asian-African countries."

Despite his elaborate description, Halbrecht's case did not progress as Witschi had already been put in a negative frame of mind by Eastman. For an evaluation, Witschi sent the project to his Swiss colleague R. Wenner, a physician at a minor hospital in Switzerland and not a protagonist of international infertility research.

In his report^x, Wenner wrote: “I studied the project seriously, reviewed some literature and – discussing with you – cleared some questions how the Ford Foundation likes to organize such big projects in general.” In contrast to Eastman, Wenner acknowledged that Halbrecht was an internationally known researcher of the calibre necessary to head a large research center. “But what annoys me is, that Dr. Halbrecht thinks, that he can spend 30 per cent of his time on the investigation program. From my point of view it seems impossible”: a head of department in charge of a “hospital” and with a teaching load could not allow himself to sacrifice this much time, according to Wenner.

Of the different research projects, Wenner found the first one on investigating side effects of the Pill and IUDs in different ethnic groups very interesting because in Israel, “in a rather small area there live isolated, homogenic groups of different human races”. The second one seemed dubious: “the causality between thromboembolic complications and the administration of oral contraceptive drugs is certainly not definitely solved.” Concluding, Wenner said Halbrecht’s statement on the expenses for laboratory equipment were not detailed enough and that the points most interesting were the ones investigating side-effects in different ethnic groups and to assist with the “postgraduate training of a whole country’s medical staff”.

Harkavy’s assistant Nicholson Eastman wrote to Harkavy giving indications on how to decide about his projects. He repeated that Halbrecht “was in to see me six months or so ago” and that he had “impressed me as a foggy thinker” who had “no ability to carry out notions that he had in mind”. On 18 December he wrote “I would say in brief that the Halbrecht proposal is very very bad while the C[r]ooke proposal is very very good.”^{xi}

Both proposals had been sent to Witschi’s friend and colleague Diczfalusy in Sweden. Diczfalusy was a potentially biased referee as he was one of the main researchers on estrogen and female hormones himself, notably supported by a massive FF grant since 1962 (on Diczfalusy’s research and biography, cf. Benagiano and Merialdi 2011; one of his most influential papers is Diczfalusy et al. 1964). It can be supposed that Diczfalusy would not have wanted anybody to criticize the use or investigation of female hormones, a project Halbrecht proposed when suggesting to investigate the thrombolytic agencies of the Pill. Witschi wrote to Harkavy on December 24, 1964, Harkavy “may be interested” to read Wenner’s evaluation of Halbrecht’s proposed project. According to Witschi, Wenner had tried “to be fair” despite the “shortcomings” also detected by Witschi’s friend Diczfalusy^{xii}. On

January 5 1965, Halbrecht received the disappointing message that after having been studied by “the Foundation’s staff and expert consultants”, “we shall be unable to accede to your request.”^{xiii}

Two years later, in September 1966^{xiv}, Halbrecht presented a more precisely formulated research project. He wrote to Witschi: “The application on which we are now working would be completely different from the one I have forwarded to you some time ago, and at any rate will be much more diversified”: a cluster of colleagues at different institutions should take part. The idea was to “integrate the already existing nuclei of research laboratories [...], to enlarge them substantially and to make them fit for teaching, training, and, mostly clinical research purposes [...] in the field of Reproductive physiology.” Halbrecht said he would contribute with his own laboratory on fetal development and reproductive physiology, Lunenfeld’s laboratory would carry out clinical endocrinology and the Tel Hashomer Hospital would take care of clinical genetics. Rabau, an internationally known investigator of infertility, would contribute on the physiology of pregnancy and labor. Halbrecht reassured Witschi that all participants had “discussed the details of combining our efforts” in a “central Institute of Physio-pathology” which could have international ties to “institutes which are being established by your and other foundations”.

Again, Halbrecht’s application was turned down. In his rejection letter, Witschi argued that despite the FF’s annual budget of \$ 8.2 million, the program could not “aim at building up entire institutes or even organize comprehensive university programs like the one that you are proposing”. Twenty members of FF staff had discussed the scope of projects they wished to fund at a recent meeting and decided against Halbrecht’s kind of project.

According to Witschi, Halbrecht’s proposal did not match the FF’s Population Program whose aim was to “give first priority to projects which show promise of contributing most directly toward its major aim, that is control of population growth.^{xv}” It has become clear that Halbrecht’s proposal was much closer to this aim than Watteville’s psychological investigations into the contraceptive habits of Genevan adults, a project that was funded simultaneously and generously.

In this letter, Witschi hinted that “now we have under consideration a request by Dr. Lunenfeld. There is no telling yet about a possible acceptance but in view of the fact that right now about four times as many requests are under consideration as can possibly [be] accepted you will understand why in New York we came to the conclusion that submitting a project as

you have outlined certainly cannot be encouraged.” Lunenfeld was one of Witschi’s acquaintances and had been invited to speak at the Venice 1966 conference that Witschi was simultaneously organizing.

Lunenfeld was a young gynaecologist at the Weizmann Institute in Rehovoth (see Calder 1959 and Flowers 1975 on this institute) and Director of its Institute of Endocrinology from 1961. Incidentally, he had obtained his MD at Geneva, in Hubert de Watteville’s department. The FF decided to support Lunenfeld’s unit from 21 December 1965 for 15 years with a grant of \$ 2,530,000^{xvi} (cf. Hertz 1984, 119). When Halbrecht had applied for a grant, he had been a direct rival of Lunenfeld’s proposal which came from a well-established research center. However, at the time, Halbrecht was the established physician with the standing to head a multidisciplinary center whereas Lunenfeld was still relatively young. Two years after the FF turned down his application, he was not too “foggy” a thinker (as Eastman had supposed) to organize the Sixth World Congress on Fertility and Sterility in Tel Aviv in 1968 (cf. Zondek and Halbrecht 1970).

Analyzing this specific instance of a proposal turned down, Halbrecht’s case exemplifies to what extent funding from the FF relied on their own advisors’ preferences, and also on possible connections within New York or the United States: the State University of New York was not the FF’s preferred institution (Columbia University was, where Anna Southam was based). In addition, the support from within SUNY for Halbrecht was not strong enough to outweigh the FF’s preferences.

Most importantly, the Halbrecht study shows how power plays in international research influenced referees. Diczfalusy who wrote the second evaluation had his own stake in the game: he was an important contributor to hormone research around the Pill, even though he only participated in Pill research some years later. He certainly did not want to support studies showing potential harmful side effects (Halbrecht’s project 1) or even a possible thrombolytic agency of female sex hormones.

In conclusion, one can argue that in many cases, the FF Population Program lacked an unbiased neutral evaluation system. From today’s point of view, Halbrecht’s research projects were ahead of their time in most respects and would certainly have contributed to high scale advanced international research had they been carried out and funded well. Halbrecht’s idea of training postgraduates and even undergraduates in what he termed “reproductive physiology” was revolutionary for 1960s medical education. From today’s point of view, the

FF missed out on an exciting project. Halbrecht's case shows how a proposal could be turned down despite his using the right key words of "reproductive physiology", "birth control" and "population control".

Witschi's argument that Halbrecht's project was not close enough to the aim of performing population control is all the more questionable when considering how far away Watteville's Swiss psychology project was from contributing to achieving population control in developing countries. When Lunenfeld was funded, the FF enacted the theory described in the 1968 memorandum to support rather biological research than medical research.

2.2 Anna L. Southam and her Impact on the Ford Foundation's Funding Patterns

Anna L. Southam (1915-1996) was the FF's second medical/scientific advisor and supported some projects in her own sphere of interest just like Witschi did. She started this position in 1965 and in 1968, Southam broadened the scope of her work becoming responsible for projects in Asia, together with Lyle Saunders.^{xvii}

It is noteworthy here that she is one of the few female fertility researchers, alongside Sophia Kleegman also based at New York and Elizabeth Palmer (wife of the eminent Raoul Palmer) in Paris, who were part of the international community of infertility experts in the 1960s.

Besides being a physician at the Columbia College of Physicians and Surgeons, she also worked as a medical advisor to the FF. At Columbia, Southam was based in Howard C. Taylor's (1900-1985) department who advised the Population Council in clinical matters. One could therefore say that the Columbia College of Physicians and Surgeons/ Department of Obstetrics and Gynecology advised the Rockefeller Foundation and the Ford Foundation. A detailed study of the intricate relations between Columbia and these two or more philanthropic organisations as well as their impact on population control programs is still missing.

Southam had made a name for herself, together with her Columbia colleague C. Lee Buxton (later head of department at Yale), as co-author of a 1958 book on infertility (Buxton and Southam 1958) in which both displayed their interest in a specific form of infertility investigation in women, namely culdoscopy. Culdoscopy is an endoscopic technique of viewing the contents of the abdomen and, more specifically, the internal genital organs such

as the tubes and ovaries, through a rigid optic tube called the culdoscope. The culdoscope, an instrument which is no longer in use, was a rigid endoscope developed in the 1940s that was inserted through a small incision in the vagina next to the uterus. The physician inserted the culdoscope through the vagina; this vaginal endoscope thus served to look at the tubes and ovaries without surgically having to open the abdomen.

Despite being an alternative to open surgery, enthusiasm for this tool was minimal in the gynecological community interested in infertility: the cumbersome procedure demanded from the woman a so-called knee-chest-position i.e. being on all fours on the operating table. The technique was internationally known after its inventor Decker had described it in the widely read *American Journal of Obstetrics and Gynecology* (Decker and Cherry 1944). There were a few vocal proponents of the technique in New York where culdoscopy had been developed and at Hopkins (cf. TeLinde 1948; Decker 1952). Anna Southam believed it had a promising future.

It is in this perspective that one needs to see her and the FF's support of a project of operative culdoscopy, culdoscopy used to sterilize women for population control. Starting in 1973, the FF poured hundreds of thousands of dollars into a project of "operative culdoscopy" for female sterilization as a contraceptive method at the University of the West Indies in collaboration with the State University of Florida^{xviii}. After one decade, both the head of the project and the FF had to acknowledge that they had failed to develop a viable technique of performing sterilization on large numbers of women. The FF also learned that the travel budget for sending staff from Jamaica to Florida had mainly been used for beach holidays.

More interesting than the description of a failed project and the money spent on dubious procedures, is the broader context in which Southam and the described technology operated. The year the project started, 1973, was a point in time when a very different technique of endoscopic surgery had already proven useful in sterilizing hundreds of women in a day, if needed and wanted, for the purpose of population control: laparoscopy.

Laparoscopy is a method in which a rigid endoscope is inserted through an incision below the navel. As an abdominal technique, it has the advantage that the abdominal organs are better visible. The woman can lie on her back rather than being on all fours. Laparoscopy in gynecology had been developed at the same time as culdoscopy, in the 1940s, yet until the late 1960s it had been used in Europe only (widely read books are Frangenheim 1972; Steptoe 1968). From 1967 onwards, the technique was used in the United States. It came with

instruments to coagulate and thereby sterilize the tubes, and a handbook that showed how to do it (Stephoe 1968). The method revolutionized population control in developing countries, especially India (a history of the implementation of the technique in India is yet to be written but hints towards it can be found in Connelly 2008).

By 1973, a number of plastic and metal clips were available for tubal sterilization in case the surgeon wanted to avoid, or did not have available the necessary electrical current, to perform coagulation of the Fallopian tubes. This had proven effective in developing countries with poor operating conditions. By 1973, a professional organization called the American Association of Gynecological Laparoscopists (AAGL^{xix}) had formed and hosted its second international meeting in the USA. Southam knew about all this, as did everybody else engaged in population control. Yet Southam chose, with the means at hand, to take a more political stand.

She chose to support research into an alternative technique, a technique that was not likely to be successful but one that she had helped to disseminate in the late 1950s. As a medical advisor to the FF, Southam supported a technique that was close to her heart: operative culdoscopy. It can be debated whether this was compliant with the FF's declared policy of seeking objective external advice. What is more interesting to the history of population policy is that foundation money was used with the intention to change current medical practice and to make political claims, however unrealistic they may have been. Southam used the FF, an agent much more powerful than herself, to have her say in the discussion about mass-sterilizations.

Southam was not against mass-sterilizations, therefore she went with the general flow of gynecologists interested in contraception in the 1960s. Southam wanted to have her own technique, funded through her initiative, implemented by her and, we can suppose, eventually to be named after her if successful. The University of the West Indies served as a risk-free test-area for that technique, and if it failed, this could always have been attributed to untrained personnel. Southam's favoured technique would have a chance to prove itself.

The case of culdoscopy in the West Indies shows two points very clearly: First, the institutional power the FF's advisors had to effectively approve or reject projects. Second, the fact that a woman in the generally male business of population control did not act in any way different from her male colleagues: she did not put forward any argument concerning the userfriendliness of her technique for women, she did not criticize mass-sterilizations and she

did not question the fact that few women were trained in the program supported by her. Southam was not a feminist, and in her pursuit of a technique of her own she mimicked her male counterparts. This finding counters a general trend in the historiography of women in science to describe women as victims of male agency. Southam was not a victim, she was as powerful as her male counterparts. However, she was not interested in strengthening the voice of other women, or of the women subjected to her culdoscopic operations.

Conclusion

The present report describes the role and impact of two biomedical advisors, Emil Witschi and Anna Southam, in detailed and representative case studies which constitute a small part of my research. Research at the Rockefeller Archive Center was the only way for me to access this highly valuable set of historical data.

The Ford Foundation was key to supporting many important researchers in the 1960s and 1970s yet instead of fairly conventional research, they could have supported many more groundbreaking projects. Their failure to do so was due to a primary reliance on a network of acquaintances for advice, and in quite a number of cases a lack of neutrality in the assessment of funding applications.

My findings can correct the FF's own historiography published in the journal *Contraception* in 1984. In his historical account, Roy Hertz idealized the evaluation process and wrote that "initial site visits by staff usually accompanied by selected consultants provided pertinent data concerning the physical and institutional setting for proposed projects." He admitted that "for the most part grant recipients were selected by staff for consideration" yet that "panels of staff-selected experts [...] convened for grant review" combined with the "expertise of the staff" yielded satisfactory outcomes in the end (Hertz 1984; 113, 131). My case studies present instances in which a broader network of advisors and experts would have helped fund more important projects.

ⁱ "Conference on the Current Status of Medical and Biological Research Related to the Population Problem. June 9, 1959 New York City", Report #007721, Unpublished Reports, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.

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- ⁱⁱ “International Division/Population/Budget for Fiscal 1969-73, April 1968. Foundation Strategy: Population Work”, Population Program Office Files of Oscar Harkavy (FA623), Box 19, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.
- ⁱⁱⁱ With projects such as the interdisciplinary conference bringing together everybody doing pioneering research on fertility and contraception in 1966, “Conference on the Physiology of Human Reproduction, Venice, May, 1966”, International Division/Population Program Office Files of Lyle Saunders (FA631), Box 1, Folder 16, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.
- ^{iv} “Research in Reproductive Physiology”, Grant file #PA64-411, grant to Hubert de Watteville, University of Geneva, 07/17/1964-02/28/1976, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.
- ^v J.H. Meylan, “The Psycho-Sociological Aspects of Contraceptive Methods”, abstract for “Conference on the Physiology of Human Reproduction, Venice, May, 1966”, International Division/Population Program Office Files of Lyle Saunders (FA631), Box 1, Folder 16, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.
- ^{vi} “Establishment of a Teaching Training & Research Unit of Physiology of Reproduction (I.G. Halbrecht)”, Log file #L64-843, grant proposal Tel Aviv University Medical School, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.
- ^{vii} Isaac Halbrecht to Oscar Harkavy, June 18, 1964, letter, Ford Foundation Archives, Log file #L64-843, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.
- ^{viii} Emil Witschi to Oscar Harkavy, June 29, 1964, letter, Ford Foundation Archives, Log file #L64-843, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.
- ^{ix} Nicholson Eastman to Oscar Harkavy, July 14, 1964, letter, Ford Foundation Archives, Log file #L64-843, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.
- ^x R. Wenner to Emil Witschi, December 09, 1964, letter, Ford Foundation Archives, Log file #L64-843, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.
- ^{xi} Nicholson Eastman to Oscar Harkavy, December 18, 1964, letter, Log file #L64-843, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.
- ^{xii} Emil Witschi to Oscar Harkavy, December 24, 1964, letter, Ford Foundation Archives, Log file #L64-843, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.
- ^{xiii} Richard Catalano (assistant to Oscar Harkavy) to Isaac Halbrecht, January 5, 1965, letter, Log file #L64-843, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.
- ^{xiv} Isaac Halbrecht to Emil Witschi, September 18, 1966, letter, Log file #L64-843, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.
- ^{xv} Emil Witschi to Isaac Halbrecht, October 31, 1966, letter, Log file #L64-843, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.

^{xvi} “Research in Reproductive Physiology at the Weizmann Institute of Science in Israel”, Grant file #PA66-078, grant to the Population Council for the Weizmann Institute, 12/21/1965-03/31/1980, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.

^{xvii} “Assignment of Dr. Anna Southam and Mr. Lyle Saunders as Program Advisors on Population Questions in Asia. May 6 1968.”, Report #010061, Unpublished Reports, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.

^{xviii} “Research & Training Program in Operative Culdoscopy & Human Reproduction”, Grant file #PA73-15, grant to the University of the West Indies, Ford Foundation Archive, Rockefeller Archive Center, Sleepy Hollow, New York.

^{xix} Their website is <https://www.aagl.org/service/about-aagl/> , accessed 02/20/2014.

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