NATIONAL ACADEMY OF SCIENCES

GERTY THERESA CORI

1896—1957

A Biographical Memoir by

JOSEPH LARNER

Biographical Memoir

Gerty T Cori

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August 8, 1896–October 26, 1957

BY JOSEPH LARNER

GERTY AND CARL CORI'S most significant contributions were the establishment of the cycle of carbohydrates known as "the Cori Cycle," the isolation of glucose 1-phosphate, and the discovery of phosphorylase and phosphoglucomutase. These discoveries established the enzymatic pathways of glycogenolysis and glycolysis.

In glycogen metabolism, Gerty Cori pioneered in the discovery of the debranching enzyme amylo-1,6-glucosidase and its use in the elucidation of glycogen structure by serial enzymatic degradation. This pioneering work led to the elucidation of the enzymatic defects in the glycogen storage diseases. Her studies, therefore, extended fundamental scientific discoveries into the clinical arena, most particularly in the field of pediatrics, her original area of clinical interest and specialization.

Gerty Theresa Radnitz was born on August 8, 1896, in Prague, at that time part of the Austro-Hungarian empire. Otto Radnitz, her father, was director general of a sugar refinery in Bohemia. Her mother's brother was professor of pediatrics at the University of Prague. Gerty studied at home until the age of ten, when she went to a girls' preparatory school, from which she graduated in 1912. In 1914, after passing her final examination (*matura*) at the Tetschen

Real Gymnasium, she enrolled as a medical student at the Carl Ferdinand University, the German university of Prague. There she met Carl Cori; they received M.D. degrees together in 1920 and were married in Vienna in August of the same year.

Carl described her as follows: "She was a fellow student, a young woman who had charm, vitality, intelligence, a sense of humor, and love of the outdoors—qualities which immediately attracted me." Their research together began as students and resulted in their first joint publication, on complement, in 1920.

The times were troubled. World War I had just ended, and the Austrian Empire had begun to disintegrate. Prague became the capital of the new country Czechoslovakia. Starvation and near starvation were widespread, and Gerty Cori developed symptoms of xerophthalmia—fortunately cured in time with an improved diet at her home in Prague.

During most of 1921, the Coris worked separately. Gerty was in pediatrics at Karolinen Kinderspital in Vienna under Professor W. Knoepfelmacher. Her research dealt with thyroid treatment for temperature regulation in a patient with congenital myxedema, and subsequently with studies in thyroidectomized rabbits. Several clinical research papers were published on hematological dyscrasias, including hemolytic crisis and essential thrombocytopenia.

Meanwhile Carl, also in Vienna, did laboratory work in the mornings and research at the University Pharmacological Institute in the afternoons. "My preceptor at the clinic in Vienna," he later wrote, "was a brilliant but amoral physician who was strongly antisemitic." Carl realized that because Gerty was a Jewess, their chances of obtaining academic positions in Europe were extremely slim, certainly an important factor motivating them to move to the United States.

In 1922 the Coris came to the United States—Gerty following Carl by about six months—and took up positions at the New York State Institute for the Study of Malignant Diseases (now Roswell Park). Gerty's first Buffalo publication, in 1923, compared thyroid extract and thyroxin on the rate of multiplication of paramecia, continuing her early interest in thyroid hormone action.

In Buffalo, the Coris' collaborative work rapidly became focused on carbohydrate metabolism in vivo and its hormonal regulation. In order to attack questions quantitatively, they developed careful methods for analyzing glucose, glycogen, lactic acid, and inorganic and organic phosphate. The Coris' studies on tumors in vivo confirmed the pathophysiological importance of Warburg's in vitro finding of increased tumor aerobic glycolysis, i.e., lactic acid formation. This early interest in lactic acid was of even greater importance in later work with epinephrine.

In 1923 and 1924, Gerty published a series of four papers alone on the influence of X-rays on the skin and on the metabolism of body organs. She was interested in the possible differential sensitivity to X-rays of stained versus unstained skin. One cannot help but wonder whether this early exposure to radiation might have contributed to her later fatal bone marrow disease.

In Buffalo the pattern of developing superior analytical methodologies with great attention to experimental detail, coupled with the quantitative framing of questions, became a hallmark of the Coris' research. In a series of three elegant papers, they presented quantitative, in vivo balance studies involving short-term (3-hour) epinephrine administration that showed a small increase (+37 mg) in liver glycogen in the face of a larger decrease in total (chiefly muscle) glycogen (–57 mg). (From studies in hepatectomized animals, muscle glycogen itself was already known not to

contribute directly to blood glucose.) Since epinephrine administration led to increased blood lactic acid, the increased liver glycogen most likely had come from lactic acid, a product of muscle glycogen breakdown carried by the blood from the muscles to the liver.

The Coris demonstrated that 40 to 95 percent of D–lactate (the isomer formed in muscle), whether eaten or injected, was retained as liver glycogen. L–Lactate, the unnatural isomer, was absorbed but not essentially retained as liver glycogen. Careful control experiments excluded vasoconstriction and hypoxia as the causes of the lactic acid increase produced by epinephrine administration. Arterio-venous difference measurements demonstrated that increased lactic acid in the blood arose from bodily (chiefly muscle) sources. The Coris termed this hallmark accomplishment the "cycle of carbohydrates," later fittingly called "the Cori Cycle."

The Coris' close attention to detail in developing superior analytical methodology proved even more important in the analysis of hexose phosphates, the next intermediates they scrutinized. They had previously shown that only 40 percent of the glycogen lost from the body with epinephrine action could be accounted for as lactic acid. This provided the motivation for developing a procedure for analyzing hexose monophosphates, a rigorous method based on measuring both the reducing power and organic phosphate content of water-soluble barium salts precipitated with ethanol. The Coris performed both determinations in order to characterize the product more precisely.

In the first of two notable papers—which lists Gerty Cori as the first author, suggesting that it was she who was primarily responsible for developing the team's quantitative analytical methodology—the Coris described their methodology. In the second they showed that hexose mono-

phosphate increased with epinephrine administration but not with insulin or with glucose. Thus began the work on the biochemical basis for the formation of hexose monophosphate in glycogenolysis and the discovery of glucose 1-phosphate.

In 1931 Gerty and Carl moved to St. Louis. Their work focused on the action of epinephrine evoking glycogenolysis in muscle. They increasingly simplified their experimental systems, working first with the whole animal, then with isolated muscle preparations, then with minces, and finally with broken cell preparations. In Buffalo they had shown conclusively that epinephrine administration increased hexose monophosphate in muscle in fifteen to sixty minutes, with a decrease to basal concentrations in four hours. Further, they had demonstrated a decrease in inorganic phosphate under these conditions and estimated that the hexose monophosphate accumulation was sufficient to account for the missing glycogen not accounted for as lactate.

From 1933 to 1936 they produced a set of papers on hexose monophosphate formation in frog and rat muscle, particularly with epinephrine administration and electrical stimulation, chiefly done anaerobically. Carefully measuring lactic acid, inorganic phosphate, creatine phosphate, and ATP, they concluded that the increased hexose monophosphate derived from glycogen esterification with inorganic phosphate in a stoichiometric reaction. The increase in hexose monophosphate occurred with an equivalent decrease in inorganic phosphate, with no change in creatine phosphate or ATP. In a paper with A. H. Hegnauer, Gerty states: "Of the three exothermic chemical reactions known to occur in anaerobic muscle (lactic acid formation, splitting of phosphocreatine, and splitting of adenosine triphosphate), only the first one is accelerated by epinephrine."

The Coris focused less and less on lactate and more on hexose monophosphate. They also obtained important data through studies of the reverse reaction, i.e., epinephrine removal (or cessation of electrical stimulation) followed by recovery under aerobic conditions. Hexose monophosphate was shown to disappear three times more rapidly aerobically than anaerobically. Increases in inorganic phosphate were accompanied by a decrease in hexose monophosphate but with increases in glycogen and lactic acid. Aerobically, glycogen was a major product, but anaerobically lactic acid predominated.

The Cori experiment on frog muscle poisoned with iodoacetate proved key, for it showed that the loss of hexose monophosphate was the same in poisoned and in unpoisoned muscles. Aerobic resynthesis of glycogen from hexose monophosphate, therefore, occurred directly, without first being converted to lactic acid.

Glucose 1-phosphate was first isolated from washed, minced frog muscle, incubated in inorganic phosphate buffer, in the presence of adenylic acid (1936). In the full publication of this work (1937), the Coris recorded that rabbit muscle was extracted with water, dialyzed extensively against water, and kept refrigerated under toluene. Phosphate buffer, glycogen, and adenylic acid were then added to the extract. The reaction mixture was incubated thirty minutes at 25°C, deproteinized, and $Ba(OH)_2$ added to alkaline pH. The procedure developed for hexose phosphate analysis—namely, precipitation with alcohol—was then followed. The reducing power before acid hydrolysis represented the hexose 6-phosphate. The reducing power after acid hydrolysis represented the new hexose 1-phosphate. The researchers obtained about 500 mg of barium glucose 1-phosphate from 750 mg of glycogen.

In this paper, the chemical synthesis of glucose 1-phos-

phate was also ascribed chiefly due to the work of Sidney Colowick. The chemical properties, furthermore, including the acid dissociation constants, were carefully determined for both natural and synthetic compounds and shown to be identical. Very little can be added today to the chemical properties described by the researchers of the Cori lab, and the paper represents another milestone in enzymological research. It also included brief references to enzymological studies that would play such a large role in the Coris' later work, such as hydrolysis of the glucose 1-phosphate by an intestinal phosphatase and, in a note, the enzymatic conversion of glucose 1-phosphate to glucose 6-phosphate (Embden ester) in the presence of Mg^{2+}.

The years 1938 and 1939 were seminal, for, following their isolation of glucose 1-phosphate, the Coris shifted the emphasis of their work toward enzymology. Of ten papers published during that period, Gerty Cori was first author on seven, Carl on two, and Sidney Colowick on one.

One paper they wrote with Sidney Colowick studied the "migration" of the phosphate group of glucose 1-phosphate to the 6 position. Again, rabbit muscle extracts were prepared, exhaustively dialyzed, and even electrodialized to remove the Mg^{2+} required for the reaction. Among the series of metals studied, Mn^{2+} was found to be even more effective than Mg^{2+}. Mannose 1-phosphate and galactose 1-phosphate, synthesized by Sidney Colowick, were shown not to be converted to the corresponding 6 phosphates by the enzyme now named phosphoglucomutase. This was in keeping with the phosphoglyceromutase terminology previously used by Meyerhof and Kiessling. The muscle extracts contained no detectable phosphatase activity, but the conversion of glucose 6-phosphate to fructose 6-phosphate was established and the enzyme termed "phosphohexoisomerase."

A major error was the researchers' failure to recognize phosphoglucomutase as an equilibrium reaction. This was undoubtedly due to the presence of the isomerase, which distorted the mutase equilibrium. As noted in an addendum, no effect of insulin (Zn free) was detected either on the mutase reaction or on the formation of glucose 1-phosphate from glycogen. (This was in response to an earlier report by Lehmann describing an inhibitory effect of Zn insulin on the mutase reaction.)

A subsequent paper described the properties of the enzyme catalyzing the formation of glucose 1-phosphate from glycogen. This catalytic activity was now named phosphorylase.

With Gerhard Schmidt, Gerty and Carl began to examine the physiologic significance of their discovery of glucose 1-phosphate. They turned their attention to the liver—the organ responsible for the formation of blood glucose. It had been presumed that blood glucose was formed in the liver by the enzyme diastase (amylase). An alternate pathway via the action of phosphorylase and glucose 6-phosphatase had already been proposed by Gerty and Carl.

With Gerhard Schmidt, they now demonstrated the presence in liver of phosphorylase and phosphatase in the face of very weak amylase activity. Phosphorylase and phosphatase were separated from each other by alumina adsorption. By ammonium sulfate fractionation, phosphorylase was obtained free of mutase and phosphatase. This enzyme preparation catalyzed the formation of a polysaccharide in the test tube, from glucose 1-phosphate, which stained brown with iodine and was indistinguishable from glycogen. Adenylic acid was necessary for the phosphorylase reaction to proceed in either the degradative or synthetic direction.

The researchers further demonstrated with an extract from muscle the synthesis of glycogen in the test tube.

Here the synthesized polysaccharide stained blue with iodine, thus more closely resembling starch. Once again they had produced a hallmark paper.

In a brief but very important note, Gerty and Carl reported the activating effect of glycogen itself on the synthesis of glycogen from glucose 1-phosphate. Careful correlation of experimental results revealed an important difference when glycogen synthesis was carried out with phosphorylase preparations from liver and from other tissues. With preparations from other tissues, a lag period of variable length for glycogen synthesis was always observed. In contrast, with liver preparations, no lag period was observed. (No lag was observed with any enzyme preparation when the reaction was run toward glycogen degradation.) Since liver phosphorylase preparations always contained glycogen, while little or no glycogen was detected in the other enzyme preparations, the researchers decided to study the effect of adding glycogen during the lag period. Added glycogen abolished the lag period, and, Gerty and Carl reasoned, "one may conclude that this enzyme, which synthesizes a high molecular weight compound—glycogen, requires the presence of a minute amount of this compound in order to start activity." Thus began the concept of glycogen synthesis upon a preexisting primer.

Once again, the final publication by Gerty and Carl was an elegant description of the enzyme kinetics. Michaelis constants were determined for glucose 1-phosphate, for adenylic acid, and for glycogen with enzyme preparations from both brain and muscle. The reaction equilibrium was measured as a function of pH, and the reaction order was determined. It was further shown that glucose inhibits the reaction competitively with glucose 1-phosphate.

As already mentioned, phosphorylase preparations from brain, heart, or liver synthesized glycogen that stained

brown with iodine, while phosphorylase preparations from muscle synthesized glycogen that stained blue with iodine. This extremely interesting observation eventually led to my subsequent work on the branching enzyme. In a paper published with Richard Bear, they compared the X-ray diffraction patterns of the two types of enzymatically synthesized glycogen with the pattern from plant starches and found that the pattern of the blue iodine-staining polysaccharide synthesized by the muscle phosphorylase was very similar to that from plant starches. The brown iodine-staining polysaccharide showed only a diffuse pattern, characteristic of amorphous material. With Zev Hassid, they demonstrated that the blue iodine-staining polysaccharide synthesized by the muscle enzyme was similar to the unbranched fraction of starch called "amylose." Digestion studies with B amylase, together with chemical methylation and hydrolysis studies, defined the synthetic polysaccharide as a 1,4-linked glucose polymer with an average chain length of 200.

Together with Earl Sutherland, Sidney Colowick, and Carl, Gerty established the chain of reactions from glucose 6-phosphate to glycogen, separating phosphorylase from mutase and isomerase. The researchers further corrected the previous failure to recognize the mutase equilibrium and quantified it. By precipitating the inorganic phosphate released by phosphorylase as $Ba_3(PO_4)_2$, they "pulled" the set of reactions—mutase and phosphorylase—toward glycogen synthesis against the unfavorable mutase equilibrium.

Gerty's experimental flair next became apparent in a note written with Arda Green and Carl describing the crystallization of muscle phosphorylase. The stage had been set by the prior fractionation of the enzyme, free of mutase and isomerase. Then in 1943, there appeared a definitive set of four papers on crystalline muscle phosphorylase.

In the first, Arda Green and Gerty Cori described the preparation and physical properties of phosphorylase, including the molecular weight. In the second, Gerty and Arda Green wrote on the prosthetic group with two forms of the enzyme, *a* and *b*, which were shown to be interconverted by a third enzyme termed "PR" to denote prosthetic-group removing. The prosthetic group removed, however, was thought to be adenylic acid, and this was later shown to be incorrect. The third paper—by Carl, Gerty, and Arda—described the kinetics of the reaction, while the fourth paper, by Gerty and Carl, dealt with the formation of glycogen. In it a new enzyme was described that permitted a conversion of the blue iodine-staining polysaccharide into the brown-staining glycogen. The new enzyme was thought to be a new phosphorylase that synthesized the 1,6 bond or an enzyme related to an amylase.

These are the papers on which I cut my scientific teeth, and—with the exception of the work on the PR enzyme—they remain classics in the field.

The crystallization of phosphorylase *a* from muscle and the recognition of a second *b* form, together with its subsequent crystallization by Carl and Gerty, initiated the era of control by covalent phosphorylation and the influence of allosteric effectors—since the two forms were recognized by their differential sensitivity to adenylic acid. The correct chemistry of the interconversion between the two forms *a* and *b* was subsequently clarified by Krebs and Fischer and by Sutherland and Rall and their collaborators. The Cori laboratory continued to crystallize additional glycolytic enzymes successfully. Gerty, with Carl and Milton Slein, crystallized from muscle extracts *d*-glyceraldehyde 3-phosphate dehydrogenase and, with John Taylor and Arda Green, aldolase.

While Carl, Earl Sutherland, and Theo Posternak pursued their interest in enzyme mechanisms (particularly study-

ing the mutase reaction), Gerty and I continued to study glycogen structure. The work on hormonal regulation by Carl, Win Price, and Sidney Colowick that had caused such a furor had been discontinued. This was later resumed along different lines by Mike Krahl, Joe Bornstein, Rollo Park, and their collaborators, who were studying insulin action, and by Sutherland, Rall, and their coworkers, studying epinephrine and glucagon action.

I arrived in St. Louis in 1949, having spent eighteen months in the Biochemistry Division of the Chemistry Department at the University of Illinois obtaining a master's degree in chemistry. Upon arrival I was assigned to work under Gerty Cori and immediately began a program of research.

At that time Gerty was particularly interested in the degradation of the 1,6 linkages, or branch points, in glycogen. Shlomo Hestrin, who had preceded me in St. Louis, found that highly recrystallized muscle phosphorylase degraded glycogen only partially (about 40 percent), whereas crude phosphorylase degraded glycogen completely. I was given the task of finding how the cruder enzyme bypassed or degraded the branch points.

Through the work of Allene Jeanes, Melvin Wolfrom, and their collaborators, the 1,6-linked disaccharide, isomaltose, had just become available. We had a small, precious sample, which we used as a standard in a paper chromatographic analysis of the products of enzymatic degradation of glycogen from crude or highly purified phosphorylase preparations. When the reaction mixtures were treated to remove the hexose phosphates, the analyses revealed the presence of free glucose only, and no isomaltose was found.

We proposed, therefore, a hydrolytic mechanism of cleavage of the 1,6 linkages, with the formation of free glucose as sole reaction product. The debranching enzyme was named amylo-1,6-glucosidase. Acting together with phosphorylase,

amylo-1,6-glucosidase completely degraded glycogen to a mixture of about 90 percent hexose 1-phosphate and 10 percent free glucose. William Whelan subsequently showed that this enzyme has two activities: first, to reposition the several glucose residues on the product of the action of phosphorylase on glycogen, such that the singly 1,6-linked glucose residue was exposed (transferase activity); second, to catalyze the hydrolysis of the 1,6 linkage, releasing free glucose.

I still recall Gerty's excitement when I discovered free glucose as the only reaction product. She ran up the hall to Carl's office at the other end of the Department shouting "It's free glucose, it's free glucose!"

I continued to work with Gerty, using the new enzyme amylo-1,6-glucosidase and phosphorylase in a sequential manner to work out the arrangement of the branch points in glycogen and in amylopectin, the branched-starch fraction. Gerty also continued working with Pat Keller on the PR interconverting enzyme. They found that, in the conversion of phosphorylase *a* to *b*, the molecular weight was halved, and Carl immediately renamed the PR enzyme phosphorylase rupturing.[1]

I next worked out the mechanism of the branching enzyme, while Gerty carried out independent studies on hexokinases with Milton Slein and—with Severo Ochoa, Milton Slein, and Carl in 1951—on fructose phosphorylation and metabolism in liver.

By far her greatest interest in her remaining years was the nature of the enzymatic defects in the glycogen storage diseases, a return, in a sense, to her original interest in pediatrics. I first considered and then proposed to Gerty the possibility that the disease—then considered to be a single ailment termed Von Gierke's disease—might be due to a lack of the debranching enzyme, amylo-1,6-glucosidase.

But Gerty felt that the missing enzyme was glucose 6-phosphatase. Behind us in the laboratory stood a chemical cabinet that contained, among other things, a set of glycogen samples Gerty had isolated from tissues sent to her by numerous clinical investigators interested in this disease. I reasoned that, if I were correct, the glycogen itself would have an abnormal structure, with shortened outer chains, but with branch points intact. If Gerty were correct, the glycogen structure would be normal.

We made a wager on the outcome—a common event in the laboratory. With her permission I proceeded to take one of the glycogen samples from the cabinet, dissolve a small aliquot in water, and stain it with a few drops of iodine solution in a test tube. To both Gerty's and my amazement, the sample stained bluish-purple with iodine—more like a starch than a glycogen! This sample had been sent by Dorothy Anderson, my former pediatrics teacher from Columbia University College of Physicians and Surgeons. Serendipitously, it was the only sample in the collection with an abnormal iodine color.

I immediately rationalized an explanation, arguing that the branch points were intact but that the outer chains were elongated because the child had died in a state of good nutrition, so that the outer chains had been built up. Gerty became very excited, recognizing that with an abnormal glycogen structure, glycogen storage was a molecular disease. (The only example of a molecular disease known at that time was Pauling's sickle-cell hemoglobin.)

Gerty began to study this problem intensively. It soon became clear that it was in fact a disease with multiple enzyme defects. She was able to document four forms, one related to lack of glucose 6-phosphatase in liver, a second related to a lack of amylo-1,6-glucosidase with a generalized organ distribution, a third related to a lack of the

branching enzyme responsible for the bluish-purple-staining glycogen, and a fourth of unknown etiology leading to a generalized organ disease.

Gerty Cori summarized these findings in a Harvey Lecture in 1952. Both of our hypotheses had been correct. Her last published work, in 1957, was a review on the glycogen storage diseases.

Gerty was a tireless scientific worker and an avid reader. She was at all times a superb experimentalist and analyst with the most demandingly high standards. Even though I arrived in St. Louis with a paper already published in the *Journal of Biological Chemistry*, she personally taught me how to pipette, watched over my shoulder as I performed my first standard curve for the analysis of glucose, taught me how to crystallize muscle phosphorylase, and had Earl Sutherland teach me how to crystallize the potassium salt of glucose 1-phosphate.

She was constantly in the laboratory, where we two worked alone. We washed our own laboratory glassware and she would occasionally complain bitterly to Carl about not having any dishwashing help. When she tired, she would retire to her small office adjoining the laboratory, where she would rest on a small cot. She smoked incessantly and dropped cigarette ashes constantly on the tar-covered laboratory benches. I often wondered if this helped in the enzyme crystallizations.

Gerty had a vivacity and a love of science and discovery that were infectious. She wanted to make the exciting discovery first, then do the necessary controls later. She and Carl had an instinctive "feel" for the right path to follow to solve the problem. She needed only one exciting experimental finding to jump into a problem with unbounded energy.

During these years her health was failing, and Carl was personally involved in monitoring her blood hemoglobins

and administering her transfusions. Yet the illness never carried over emotionally to affect her attitude during her work in the laboratory.

Gerty was extremely broadly read. The Mercantile Library was an organization in St. Louis from which one ordered books by phone. Gerty would routinely order five to seven books per week, which were delivered to the laboratory or to the department office. By week's end she would have read them all, put in her next week's order, and have the old ones picked up. This occurred week after week.

She could speak authoritatively on a variety of topics, from political theory, to sociology, to art and the humanities, to grocery shopping. Her intellectual breadth never ceased to amaze me.

I feel most fortunate to have been trained by Gerty and Carl as a student and to have had the opportunity to soak up some of their approach to dealing with scientific problems.

THIS I BELIEVE[2]

I have been very fortunate to have been allowed to work freely in the field of . . . biochemistry, in which I have been intensely interested ever since I got a first glimpse of it as a medical student in 1914. I came to this country in 1922 and owe it the greatest debt of gratitude for having treated me and my husband with fine generosity, giving us wonderful opportunities for research work, security, and a happy life.

I believe that the benefits of two civilizations, a European education followed by the freedom and opportunities of this country, have been essential to whatever contributions I have been able to make to science.

I believe that in art and science are the glories of the human mind. I see no conflict between them. In the past they have flourished together during the great and happy periods of history and those men seem to me short-sighted who think that by suppressing science they will release other creative qualities. It may be, however, that the present period is more favorable to the development of science than of art.

Contemplation of the great human achievements through the ages is helpful to me in moments of despair and doubt. Human meanness and

folly then seem less important. Humanity has but a short history of civilized life and the hope for greater wisdom must resign itself to a fairly distant future. Gone are the somewhat Utopian hopes of my youth, the belief in continuous progress. I still believe, however, that Western civilization is at least on the right track.

Cruelty and malice have decreased as the well-being of people in the Atlantic community has increased. An immense advance in this direction has taken place in the last hundred years, even if one considers the setbacks caused by the two terrible wars of this century. Modern medicine, aided by chemistry, has decreased human suffering. Throughout, science has thus conferred an immense boon on mankind.

The greatest achievements in art and science, I believe, have been made by men who had faith, or compassion, for their fellow men, and I like to think in this connection of the moving outcry in Beethoven's opera, *Fidelio*: "Es gibt eine Gerechtigkeit!" (There is justice!).

I believe that cynicism and despair and the straight jacket into which totalitarian systems try to force the human mind are inimical to first-rate achievements in art and science.

I believe that the excessive will to power of some men has been, and is still, the cause of great suffering of humanity. Science has given these ruthless men tools of great effectiveness and has vastly increased the domination they can exercise over their fellow men. This has created in some men's minds the misconception that science itself is evil.

My beliefs have undergone little change during my life, though I like to think they have developed into a somewhat higher plane.

Honesty—which stands mostly for intellectual integrity, courage, and kindness—are still the virtues I admire, though with advancing years, the emphasis has been slightly shifted, and kindness now seems more important to me than in my youth.

The love for and dedication to one's work seems to me to be the basis for happiness. For a research worker the unforgotten moments of his life are those rare ones, which come after years of plodding work, when the veil over nature's secret seems suddenly to lift, and when what was dark and chaotic appears in a clear and beautiful light and pattern.

NOTES

1. As mentioned above, the final solution of the interconversion mechanism was provided by Krebs and Fischer, and by Sutherland and Rall and their collaborators.

2. Gerty Cori submitted this short statement of her personal philosophy to the National Academy of Sciences in 1954.

HONORS AND DISTINCTIONS

DEGREES

1914–20

M.D., Carl Ferdinand University, Prague, Czechoslovakia

PROFESSIONAL APPOINTMENTS

1920–21

Clinical training in pediatrics, Karolinen Kinderspital, Vienna, Austria

1921–22

Department of Pathology, New York State Institute for the study of Malignant Disease (Roswell Park), Buffalo, New York

1931–47

Professor of pharmacology, Washington University School of Medicine, St. Louis, Missouri

1947–57

Professor of biochemistry, Washington University School of Medicine, St. Louis, Missouri

HONORS

1946

Midwest Award of the American Chemical Society

1947

Squibb Award of the Association for the Study of Internal Secretion

1947

With C. F. Cori, Nobel Prize in Medicine or Physiology

1948

Frances P. Garven Medal, American Chemical Society

1948

St. Louis Award

1950

Sugar Prize of the National Academy of Sciences

1951

Borden Award of the Association of American Medical Colleges

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