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An Early Oscillator Model: Studies on the Biodynamics of the Piano Strike (Bernstein & Popova, 1930)

Bruce A. Kay, Michael T. Turvey, and Onno G. Meijer

In the following paper, published in 1930, Bernstein and Popova report an impressive study (even by modern standards) of a complex motor behavior: movements performed by concert pianists, specifically series of octave strikes made with one hand. As so pungently discussed in their introduction, Bernstein and Popova were trying to rectify the situation at the time, a situation in which "most studies in the area of piano methodology originate either from physiologists who are *dilettantes* in music or from musicians who know nothing about physiology" (present paper, p.5). According to many of the contemporary pedagogues, such octave strikes are produced by letting the hand fall passively onto the targeted keys, using the weight of the arm from the highest point attained in transit—the so-called "weight" technique. This "weight" idea had only intuitive and virtually no scientific support. Given the to-be-rectified situation, it must have seemed to Bernstein and Popova that they could actually submit this particular idea about piano methodology to experimental tests.

Certainly Bernstein felt a strong impulse throughout his career to "objectify" notions and conjectures about movement, and this paper represents a great effort to do so. As seen in the paper, Bernstein and Popova discredited the weight notion, showing that active muscle forces are almost always at play in a piano strike, regardless of variations in strike force and/or tempo. Along the way, they analyzed the movements further to see how the active forces are produced. From our present vantage point, we are sorely tempted to see hints at coupled oscillator models, particularly with respect to changes in one control parameter, the tempo.

Bernstein and Popova's study is impressive in many respects, beginning with the methodology. Of first note, in line with Bernstein's earlier work, is their choice of such a complex behavior, a bold move, to say the least. Even though they limited the motions under study to rhythmically produced octave strikes, their focus was still a complex movement. Second, they used an elegant experimental design, varying two key parameters: the tempo of the movement and the force

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with which the keys were struck. Third, they employed the sophisticated method of "kymocyclography" (Bernstein, 1927, 1928/1936) to register the movements, a method that was probably unsurpassed until the recent advent of optoelectronic techniques. Perhaps the most impressive methodological innovation, however, was the biomechanical analysis they performed on the obtained kinematic data. They analyzed free-body diagrams to infer how forces were produced over time at the wrist, elbow, and shoulder joints. Today such analyses are done routinely, but these authors' analyses proved to be unique for quite some time thereafter (e.g., Zajac & Gordon, 1989). Of particular note is the fact that they addressed issues of interaction torques—a topic that has assumed prominence in (again) modern research (e.g., Hollerbach & Flash, 1982; Hoy & Zernicke, 1986; Zajec & Gordon, 1989).

The second main way in which this paper is impressive is in the interpretation of the results, which would fit into current dialogues about motor control quite nicely. At the slowest tempo, movement segmentation seemed to occur (sounds familiar, doesn't it?), while at the faster tempi, movements became more continuous. At the fastest tempi, the movements of the hands appeared to be "forced" by those of the elbow. Thus, the coupling between limb segments changes as movement parameters are varied:

> During slow and medium tempi, both the hand and the forearm move under the action of their own active muscle impulses. At medium tempi, a sequence of such impulses merges into a single continuous chain, while during slow tempi, individual impulses leading to strikes are separated by more or less prolonged periods of inactivity. During tempi over about 6.5 strikes per second (390 per minute), hand motion transforms into forced elastic oscillations of a rather simple construction, with force amplitude close to the theoretical minimum. (p. 38)

Absent the exact equations of motion, one must be circumspect in trying to infer what Bernstein and Popova were thinking with respect to dynamical modeling. However, their discussion can be interpreted rather straightforwardly in the following coupled oscillator model terms.

For slow and moderate tempi, active muscle forces are produced at both the wrist and elbow joints. This could imply separate active oscillators for each segment, which have to be coupled in order to produce coordinated movement. For the slow tempi, this is a discontinuous "burst-like" process; first one joint and then the other joint becomes active. The latter feature is suggestive of either discrete impulse production (which would not be consonant with coupled-oscillator models) or coupled relaxation oscillators. In either case, Bernstein and Popova had in mind some kind of coupling mechanism between the joints. For moderate tempi, it is clear that "smoother" oscillators are implied, with attendant coupling.

For fast tempi, the wrist is "forced" by elbow motion. One possible interpretation is that there is an oscillator at the elbow that is driving the wrist. Apparently, the wrist is not a contributor of active muscle forces but rather a passive biomechanical element at the fast tempi, interacting only elastically with the elbow. The forcing of one element by another implies that only one of the two effectors is actively producing movement, with the other being passively driven, so this is a very different situation from the slow and moderate tempi. Furthermore, the finding that "force amplitude [was] close to the theoretical minimum" (p. 38) appears to imply that the system is operating at resonance.

At all tempi, the results imply that the wrist and elbow are tightly coupled. In the analysis on the effect of strike force the authors emphasize this:

> . . . it is significant to point out, however, that the relation between the amplitudes in the wrist and in the elbow remains very stable. This fact unequivocally suggests that the elbow and the wrist, during a piano strike, represent a tightly linked system whose biomechanical (and, probably, also innervational) unity is contrasted by the much higher independence of the shoulder. (p. 29)

Another key insight presented here is that there is a shift from one control regime to another as a movement parameter is varied. Specifically, as the tempo is varied there could be a qualitative shift in the structure of a putative coupled oscillator system from (a) two coupled active oscillators to (b) an active oscillator coupled with a passive element. One could model this shift in structure by modulating the oscillator term for the wrist to eliminate the source of energy required for oscillation, making it a passive element thereby. Additional changes in parameters, for example, coupling strength, might also be required to follow the data pattern. Qualitative shifts in regime with changes in special movement parameters—currently deemed control parameters—have been a focus of many investigations into inter-segmental rhythmic movements in recent years (e.g., Haken, Kelso, & Bunz, 1985; Kelso, 1995).

There are several other gems of inspiring thought about motor control in this paper. To give one example, this is the paper with the first hints at the non-univocality of the relationship between muscle forces and movements, such as in the wonderful sentence, “Live movement is a ball of entangled interactions” (p. 12). All in all, there is much for a present-day movement scientist to ponder. From methodology to theory, the past works of Bernstein and his colleagues continually reveal that there is oftentimes not much new under the sun, at least in terms of thinking creatively about the design features of biological movement systems.

Studies on the Biodynamics of the Piano Strike[1]

N.A. Bernstein and T.S. Popova

Paper 1. Studies of the Rhythmic Octave Strike Using the Kymocyclographic Method[2]

Chapter I. Aims of the Study

In the autumn of 1926,[3] the SIMS (State Institute of Musical Science) invited N.A. Bernstein to lead the Laboratory of Movement Studies and to organize in the Laboratory a systematic study of the biomechanics of piano playing movements. The Laboratory had originally included N.A. Bernstein and P.N. Zimin, and was later expanded with T.S. Popova, A.S. Sheves, and M.E. Feigin. It started with constructing a new device for movement recording, a kymocyclograph, based on the design by N.A. Bernstein (see the Chapter on Methods). The process of construction of this new and hard to produce apparatus, together with its tuning, calibration, etc., lasted until the spring of 1927, when first pilot experiments, and then regular studies began.

The Laboratory was forced to make a rather strict choice from the wide range of questions facing researchers of piano movements. In fact, the research area of piano playing movements had practically been undeveloped, despite the overwhelmingly urgent need of an understanding of the mechanism of the piano strike. In books and journals,[4] since rather early times, every now and again studies of a primarily popular or educational nature have surfaced pretending to suggest a universal interpretation of the mechanisms, not only of the simplest elements of piano movements, but also of the whole wide body of the technical richness of the art of piano playing in its most complex expressions. There exist two types of such papers.

First, quite frequently, studies of this kind are written by pianists, virtuosi, and instructors. After many years of practical work, accumulating not only a large amount of factual material but also a rather rich supply of practical advice of most diverse origins, an author-musician (who is always a practitioner, whether a concert performer or a teacher) commonly develops a need to unite the whole accumulated collection of empirical data under some kind of general theory. Whether one should blame the suboptimal state of scientific knowledge, the inability of the author to do scientific research, or the excessively pressing urge to base a theory, way ahead of its time and possibilities, on a body of rather raw material, works of this kind typically lead to an appealing scheme, which can easily fit one's imagination, but is always understocked with objective, factual proofs, and is commonly in contradiction not only with facts but also with certain aspects of its own theoretical position. An example of such works is the book by Breithaupt.

Second, books that attempt to provide a psychophysiological basis of piano technique and motor pedagogy are written by authors with a medical or general physiological education, in the hope that rather general anatomic and physiological facts, accompanied by their own not less general thoughts, would already be of highly valued help for musicians, and enjoy high demand among art professionals, who attempt to somehow generalize their own thinking, and provide a scientific basis for their own empirical impressions. Such books include those by Ivanovsky, Kalyand, Ritschl, etc.

Such books can do much harm in two respects. First, their marginally informed authors, who are extremely brave and convinced in their infallibility and the inalienable right to produce any number of hypotheses and arbitrary statements, sometimes generate a total mess in the discourse and conclusions. The biomechanical literature is poor, but this is another reason for the author, attempting to perform such a study, to read it, if he does not plan or is unable to resolve all the relevant special problems through own experimenting; however, the authors of these books commonly consider themselves free of such requirements apart from those necessary to pass a college exam in anatomy or physiology. On the other hand, these books are harmful because, being written by a physician or a physiologist, they, by the very fact of their origin, generate trust in musicians who are inclined to accept everything said in these books, using them while convinced that they deal with the last word of the most genuine science. When such books become sources of scientific support for an idiosyncratic theory of a pianist-theoretician, he will believe in the validity of the psychophysiological basis presented, and will be inclined to accept even the most arrogant tone.

To summarize, one can conclude that most studies in the area of piano methodology originate either from physiologists who are dilettantes in music or from musicians who know nothing about physiology. The never realized combination, in one person, of a physiologist and a highly qualified musician who strives to create an objective science of the piano strike, poses a seemingly insurmountable obstacle for effective progress in this direction. The lucky exception in this area is Dr. Steinhausen whose studies and observations deserve close and serious attention but who, unfortunately, did not organize a systematic experimental study of piano playing.

We think that the only way to avoid all the pitfalls, due to insufficient knowledge and poorly founded generalizations, resides in maximally narrowing the area of interest, limiting the study to a minimal number of problems, and trying to obtain answers by purely experimental methods. Experiments should be designed in such a way that their outcomes are clear and free of arbitrary interpretations. Such design of experiments can be viewed as a *testimonium paupertatis* of contemporary science, accepting the complete lack of reliable material in the area, forcing researchers to start from the very beginning and from the simplest elements. On the other hand, this is a way to provide an indisputable basis for future experiments, immediately discarding all ephemeral hypotheses that would contradict objective experimental findings.

There exists already a small number of purely experimental studies of piano movements. First of all, within this group, we should mention studies of the contractions of upper arm and forearm muscles during playing piano. In 1923, Dr. Bruzhes in Moscow suggested using for this purpose a myographic recording of muscle contractions with a pneumatic cuff, placed on the upper arm or on the forearm of the subject, which recorded bulging of muscles by using air flow. In 1927, Dr. Kurt Johnen and the engineer-psychotechnician[5] Andreas Schulhoff, independently of Bruzhes, used the same method to record muscle contraction in shoulder and thigh muscles during piano playing. Experiments by these authors are in their very early stage and do not provide a clear enough differentiation among the problems; however, they definitely promise to generate very interesting results in the future.

Another category of available experimental observations, also started in the post-war time, is related to recordings of piano playing movements with a time magnifier by Lemann-Ernemann, i.e., with a cinematographic device which allowed for making 15 to 20 times more shots than a regular cinematographic camera within the same time period; therefore, during projection it allows for a corresponding 15- to 20-fold slowing of the recorded movements, beautifully displaying numerous details that escape the naked eye.

This excellent device has not been used yet to perform a single serious study in the area of our interest. Fortunately, recordings of movements of great piano players have been, and are continuing to be made, preserving for us unique documents of their motor technique; however, until now, there has not been a single study with a systematic analysis of the mechanisms underlying this technique. Most recently, a number of such recordings have been made by pianist Louta Nouneberg; however,[6] the researcher has apparently not escaped the common fate of theorizing musicians: Based on a few photos, she has generated universal conclusions and fabricated a general theory of piano pedagogy—thus making an application to patent a new *elixir vitae*.

Somewhat aside are the recently performed studies of the energetics of piano playing, using the method of gas exchange. These studies have not addressed issues of the dynamics and construction of movements, and we are not going to review them here.

As mentioned already, we have decided to limit very strictly the object and field of our study. To us,[7] this appears to be the only way of guarding ourselves against the overwhelming flood of facts and considerations which would certainly create an avalanche blocking the only reliable path. In such conditions, the choice of an object for the very first study presents, by itself, many difficulties.

First of all, we limited ourselves to a group of questions related only to the form and muscle dynamics of arm movement. Thus, our field of study included neither the general seating, nor the leg and trunk movements. We also excluded questions related to breathing, heart activity, and energy exchange; thus, all questions related to the assessment of work were left aside. Further, because we considered it necessary to monitor major forces emerging and changing during piano playing movements, we paid first and foremost attention to movements of the major segments of the arm and of the arm as a whole, without considering movements of the individual fingers. Approximately from the end of the last century, one could consider as established the fact that the dominant force production role during piano playing movement is not played by the fingers but by the more proximal segments of the arm. Hence, first of all, we focused our attention on these movements, leaving the study of finger dynamics for the future.

To further dissect our object of study, we decided not to perform an experimental analysis of spatial-coordinative motor mechanisms (accuracy). We considered the examination of the force construction of any piano strike, independent of spatial refinements, a more general and urgent task. One should note that contemporary pedagogical schools (including the followers of Breithaupt and Steinhausen), for early stages of the development of the piano strike, also consider it appropriate to start from the development of a general construction of the movement, and to refine accuracy at later stages.

Since we did not plan to justify or disprove any proposition formulated earlier, and since, furthermore, we were *a priori* suspicious towards all the earlier formulations regarding certain techniques and rules of piano playing, we naturally did not want to focus on representatives of one of the performance or pedagogical schools. To be most impartial, we had to focus our attention on maximally different schools and techniques of playing, in an investigation of a whole variety of very different pianists. We were led by the following considerations.

First, we consider it necessary to find those common features, universal for everybody, that are present in piano playing movements in very different pianists (if, indeed, such common features exist—we have restrained ourselves from making assumptions with respect to this issue as well). These common properties could coexist with or be concealed by a whole spectrum of individual features, as well as features typical of individual schools, but they must invariably reveal those specific motor principles that should be as inherent to humans as their anatomo-physiological structure. We view an identification of these basic features as the most urgent and basic objective that can be achieved by research, since the authors of theories of piano playing will be less inclined to protest against findings that can be seen in each and every pianist, from any piano dynasty. At this stage, the first, undoubtable anatomico-physiological facts need to be established.

Second, the selection of various subjects, irrespective of their schools and views, is also necessary because subjective opinions about movements performed by individuals can rather dramatically contradict what they actually do. This is supported by our whole past biomechanical experience. We expected to meet,[8] and actually met, most convinced champions of weighted piano playing who, for the first time, found out (and, certainly, not always believed) from our data that they had not been playing according to the weight principle. Independently of the inability, frequently encountered among pianists, to describe verbally particular movements that they have just performed, even a direct, slow demonstration has frequently been completely different from the objective results of the experimental recordings.

That is why we have completely abandoned attempts at subject selection based on any type of classification, and limited ourselves by only one criterion, reflecting our desire to obtain as clear results as possible: For the first series of studies, we recruited as subjects only established performing pianists. This was done to make sure that the hypothetical common features are not smudged by randomly occurring mistakes reflecting poor skill of the subject. We have decided to defer the question of differences between movements of a skilled pianist and an unskilled one to a later time, as a development of the main theme of the study.

Our desire to eliminate various differences, aside from the inevitable ones between individuals, have led us to ask our subjects to play the same piano excerpts; these excerpts were selected so that differences in their performance depended only upon purely motor features of the individuals, not upon more general psychophysiological differences, and, in particular, not upon the artistic style of the performer. Thus, it was not appropriate for the study to record and investigate excerpts of purely artistic performance. We have decided to use excerpts of *études* (we will address them as "tests" although this term does not fit well), and arranged them in the most simple way which did not allow for different rhythmic or dynamical interpretations.

The process of performing a piano playing movement is certainly very complex. In particular, one of the essential difficulties, facing studies of most movements in general, is the indisputable fact that preceding movements influence following ones.[9] A movement of striking the same keys with the same force will be different depending on which particular movement preceded it. Thus, a new complication emerges which we tried to avoid by arranging our tests in the form of simple rhythmic sequences. By doing so, we definitely moved away from what is emotionally interesting for a pianist-pedagogue, i.e., from musical art; however, we more precisely focused our attention on the basic movement mechanics of the pianist.

All the aforementioned considerations have led to the final selection of initial objects of study. These were one-handed sequences of monotonous rhythmic structures, devoid of any artistic value. To avoid pure finger technique, all the objects were based on purely octave movements, and in order to avoid specific mechanisms related to accuracy, the objects were focused on only a few notes. Such a primitive object of study is quite sufficient for the identification of basic features, both general and individual, of the construction of the piano strike movement.

Similarly difficult was the problem of research design using the aforementioned, original objects. The first thing expected by a pianist-pedagogue from research is recommendations of what is correct and what is incorrect, what is better and what is worse. Meanwhile, even if such a qualitative criterion can be revealed, obviously, this would take rather time-consuming and advanced studies. We think that the mistake of most of the contemporary piano manuals is exactly that they formulate such hasty criteria without any support from a well controlled sequence of experiments.

The ultimate goal[10] of a piano playing movement or of a sequence of such movements is to produce musically beautiful sounds. Attempts at interpretation of what fits this term and what does not, not even to mention its differences from one performance to another, or from expert to expert, would draw us into the area of esthetic studies, presently absolutely beyond accurate experimentation. This is known to piano methodologists who, therefore, try to create foundations of their theoretical constructs by using *a priori* assertions that would allow them to philosophize, while avoiding to deal explicitly with the slippery area of pure esthetics. Such assertions include, in particular, the frequently made statement that the best performance is simultaneously the most energetically efficient. In some aspects, and in its simplest interpretation, this assertion is based on the unchallengeable observation that additional movements prevent a student from getting to a desired and achievable level of quickness, while spasm-like muscle fixation leads to early fatigue. However, in all its generality, this assertion cannot be accepted as the basis for an experimental study since it is still biased and, as such, can lead to a deviation from the correct path.

Another assumption, which is accepted *a priori* to an equal degree, appears to be much more reliable and acceptable; it is championed in Germany by Dr. Steinhausen, and in the USSR by Prof. G.P. Prokofiev. This is the assertion that actual piano playing movements are maximally natural movements, most closely resembling the natural psycho-motor reactions of the organism. This statement is more reliable if only because it can be supported by facts that have been well established by contemporary psycho-neurology, and in the area of piano pedagogy, and it can be justified by the practical effect of its application to teaching. However, using this assertion for the assessment of our experimental data would contain an unavoidable *petitio principii,* since the notion of natural piano movement itself needs to be precisely defined, based on experiments, thus becoming the object of our study. To accept this assertion would ultimately mean assessment of recorded movements based on a comparison with themselves.

Hence, we have resolutely rejected any subjective evaluation. Our goal is the most accurate description of phenomena that are reproducibly and to a similar degree seen in all educated, professional pianists. Only the future will show if such a description provides a foundation for the assessment of movement appropriateness, and if it points at routes towards the practical elaboration of criteria for such an assessment.

Thus, we differ in two aspects from our few predecessors and companions. On the one hand, we are less subject oriented and less scrupulous, since we are searching neither for a theory nor for an assessment scale. On the other hand, we are more precise in our selection of the object of study and in defining what we want to obtain from it.

First of all, we would like to obtain quantitative data, as precise as possible, on the kinematics of the piano playing movement in its simplest forms, then data on its dynamics, and ultimately, data on the mutual dependence between the two. We strive to find quantitative answers to questions on the degree of involvement of different segments, joints, and muscles in the execution of such a movement. This we need primarily to be able to built mechanical models of the studied movements that would reveal to us the nature of, and causal relations between forces that participate in the movements.

The most basic variables, upon which we need to ascertain dependence, are the variable of tempo and that of strike force. The significance of each of these variables can be seen from the following. With respect to tempo, there still exists a very basic problem: Do changes in the tempo of a movement influence its construction and dynamics? Solution of this problem will be crucial for a whole range of methods in pedagogy and instruction.

If movement mechanics undergo obligatory changes with changes in tempo, isn't it so that exercises with slower tempo, an inherent part of contemporary piano teaching, lose their meaning; doesn't the possibility, relevance, and appropriateness of slow demonstration disappear; doesn't the meaning of self-observation and of understanding elements of one's own motor reactions, as commonly recommended in our times, vanish? Practice at a slow tempo tries, first, to develop accuracy and, then, quickness within a given piece; if changes in the tempo lead to changes in the construction, it may be more appropriate first to practice quickness and only then accuracy. Listening to either one is equally intolerable.

The effects of strike force on the construction of movement touch a different, equally important group of practical issues. Experiments on piano mechanics show that the same objective sound level can be generated with different combinations of at least two factors, velocity and mass of the body hitting the key. Note that sound level is not proportional to, and shows an ambiguous dependence on, kinetic energy. Hence, sound of a certain level (although, probably, of different quality) can be generated with various methods.[11] It has not been determined which methods are used by pianists to vary sound level, particularly during those quick tempi at which conscious control and the possibility of self-observation are lost.

The existing views on the effects on movement forms, hand configuration,[12] etc., on the dynamics of sound are not supported by scientific experiment and may happen to be disproved. All these factors render the relationship between the level of sound and the construction of a strike a problem of utmost importance.

These two variables were selected by us at the initial stages of the study. Accordingly, we used, as a test, a sequence of octave strikes on one and the same pair of keys: a). With the tempo changing from *adagio* to *presto* and back with an average level of sound (*mf*[13] to *f*); and b). The same octave sequence in a moderately fast tempo, shifting from *pp* to *ff* and back. Tempi in the first tests (*accelerando*) climbed up to 500 strikes per minute, while in the second test (*crescendo*) there were about 200 to 250 strikes per minute.

One question, of secondary theoretical importance, emerged at the very beginning of our study because of insistant requests by practitioners. This is the question of the so-called "weighted playing". Further in the text, we will provide a detailed discussion of an indisputable answer to the question: To what extent does the weight of the extremity contribute to the studied exercises? A very clear answer was received from the first series of experimental materials. Because of the very large popularity recently gained by the theory of weighted playing, and the considerable confusion reigning in the literature, presentations, and pedagogical practice, clarification of this essentially secondary problem is worthwhile.

So, this first paper is dedicated to the elaboration of a problem which can be formulated in the following way: "Study of the general foundations of the kinematics and dynamics of rhythmic piano strikes in their dependence on the tempo and force of the strike".

To identify[14] approaches to the aforementioned complicating effects of preceding movements on following movements, we introduced into the program of tests three more with a somewhat complicated rhythm. The tests represent monotonous passages in octave on a few adjacent notes, as illustrated in Figure 1.The choice of certain rhythmic sequences was determined by the following considerations. Most generally, effects of preceding strikes can be different when the rhythm is strictly monotonous, when there is a strike immediately preceding the next one ("thicker impulses"), and when, on the opposite, a strike within a monotonous series is missed ("thinner impulses"). The second case is realized in punctuated rhythms (test #3), the third one in syncopated rhythms (test #4). Analysis of the results in these tests will be presented in the second paper of this series.

We want to note in passing that in order to get samples of purely artistic performance of our pianists-subjects for future analysis, we recorded from all of them, with the maximal possible resolution (stereoscopically), one and the same

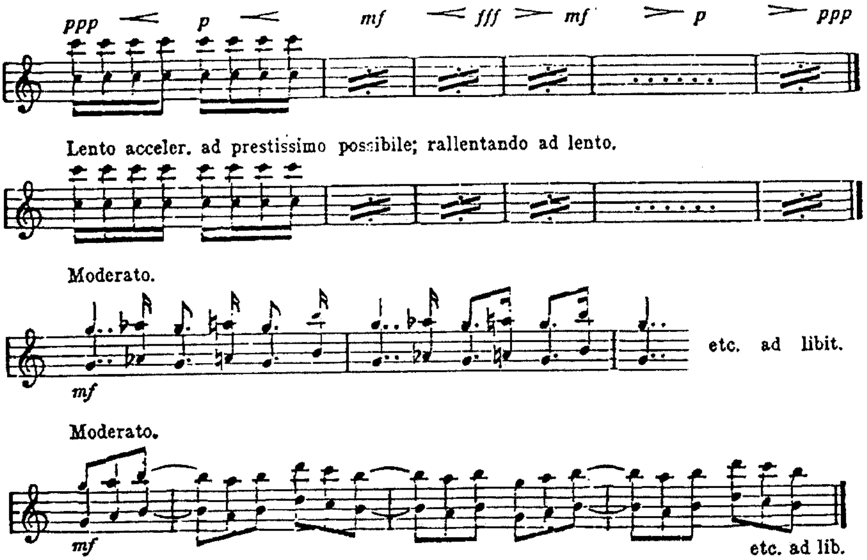


Figure 1 — Motor "tests" offered to the subjects: 1. test crescendo; 2. test accelerando; 3. punctuated test; 4. syncopated test.

musical excerpt: The first six tacts from the *Es-dur* concert by F. Liszt (the right hand). We have not even tried to decipher these records, and store them for future analysis.

As said before, experiments with recording of piano playing movements had started in the spring 1927 and were continued during the next academic year. We recorded a total of 14 subjects, exclusively prominent concert pianists, soviet as well as foreign. The accumulated material consists of up to 100 records. Due to the exceptional complexity of data processing, the present paper is based on only some of the available experimental material.

By using this material, we have been trying to obtain objective answers to the questions that are the focus of analysis in the present paper. These questions can be characterized in the following way.

Any movement, in general, results from the interaction of different forces that act on the elements of the moving system. When the system is a moving part of the human body, these forces can be classified into external ones, independent of the system, and those that emerge from within the system. External forces can be very different and, correspondingly, their analysis can present various difficulties. However, when a moving part of a body swings through the air, these forces can be reduced (ignoring the very small force of air resistance) to the unchanging force of gravity. This is the force with which forces generated within the moving segment interact; these latter forces always have the same origin, that is, muscle contractions. Any swinging motion, independent of its structure, represents the result of the interaction of these varying forces with the force of gravity. In cases when the swinging motion ends with a strike against an external obstacle (e.g., a keyboard), one more external force joins the interaction of forces, that is, the reaction of the struck object.

If the mass and inertia of a moving system are small in comparison with the acting forces (such as in the case of the fingers), the movements of the system will very closely and quickly follow the muscle contractions generating the forces on the moving parts. In an overwhelming majority of cases, however, the inertia of the segments connected to the muscles is so high and the kinetic energy acquired during the movement is so large that overcoming them, by itself, requires substantial and rather long-lasting work of the muscles before they are able to move the massive organ in the required direction. As illustrated by our earlier studies,[15] it frequently happens that, during fast movements, a group of muscles starts and finishes its contraction before a limb ultimately moves in a corresponding direction. Obviously, movements at a higher tempo are characterized by higher kinetic energy of the moving parts and, on the other hand, by less time available for the muscles to counteract this kinetic energy. It is easy to show that the forces needed to exert specific movement effects must grow as the square of tempo. That is why, during fast movements, the actions of the muscles are not as simply and clearly related to the motor effect, as they are during very slow movements. In this respect it is interesting to note that, in movements that we have studied until now, muscle forces did not grow proportional to the square of the tempo but, alternatively, the muscles seem to try to interfere less and less with the accelerating movement as if leaving it to its own. Because of that, as a rule, the dynamic structure of a live movement typically becomes more and more simple when the movement speeds up (see later on p. 31).

Forces that prevent the muscles from exerting their effects immediately during movements are apparently inertial forces, dependent upon the mass and moment of inertia of the moving parts. This group of forces, interacting with the earlier described forces, creates the movement pattern in all its complexity.[16] Most confusions and mistakes in superficial analyses of movement dynamics originate exactly from ignoring or underestimating the inertial forces whose actual role during fast movements dominates by far over the role played by the force of gravity.

The method that we used during the analysis of the piano playing movement, as will be described in Chapter II, allows one to establish, with undoubted correctness, the degree and pattern of participation of all the described force factors in the course of a movement, and to reveal the underlying changing patterns of muscle tensions. Knowing such a muscle scheme of a movement does not allow one to reconstruct it, to build its model, and therefore, does not give us full knowledge of the movement. In fact, the same scheme of muscle tensions can lead to movements of very different patterns depending on the initial velocities and positions of the body parts participating in the movement, exactly because these variables will affect the changes and magnitudes of inertial forces which determine the movement pattern.[17] Live movement is a ball of entangled interactions. By moving a limb into a certain position and accelerating it to a certain velocity, muscles create different inertial forces, which, in their turn, affect the future action of these same muscles. Mathematically speaking, any movement of the organism represents one of the possible solutions of a differential equation of the order of not less than two.[18]

Thus, if we know the curves of muscle contractions during the course of a movement, we still know next to nothing about the movement itself. Movement should be studied on the basis of such curves as a mechanical problem. Only when we are able to define the magnitude and type of the interactions between the available muscle scheme and the inertial forces that emerge depending on the kinematics of the movement itself, we will obtain an answer to the problem of the mechanical construction of the movement, and be able to create mentally its mechanical model.[19] This is how the main goal of any biodynamical study is defined, this is how we understand it and try to accomplish it.

Such an understanding of the problem allows us to introduce the necessary methodological simplifications into our study. We strive to understand muscle contractions not so much by themselves, but particularly in their meeting with the reactive forces within the system. Therefore, separate analysis of the actions of those muscles that interact only among themselves, without having to deal with interference from reactive forces, is of much less interest for a biodynamical study. Such cases are encountered when muscles under investigation act on the same joint (irrespective of whether they act in the same direction or in opposite directions). Determining the resultant force of such muscles can always be done with the simple parallelogram rule for force summation which does not require solving differential equations. In such a case, it is absolutely unimportant for a movement what the individual contributing muscles forces are, as long as the resultant force remains the same. Hence, to simplify the study (and, correspondingly, to simplify the experimental equipment), we limited ourselves to an analysis of resultant muscle forces acting at each separate joint, independent of the actual number of muscles acting at the joint and, correspondingly, of how many components bring about the resultant force that is of interest to us.

Thus,[20] with respect to the piano strike movement, our problem is formulated in the following way. We need to find out what the actual dynamical construction of the movement is in the process of the rhythmic piano strike, which specific features of this construction are necessarily inherent to any piano performer, and further, which individual differences in the construction can be seen in individual pianists, and how this construction emerges and develops. The second and third part of the problem are the subject of presently conducted investigations[21] and will be discussed in further papers within the present series. The present paper deals mostly with the first part of the suggested formulation.

To determine the typical construction of a striking movement, we have studied the resultants of muscle forces acting at all the joints of the arm (with the exception of finger joints, because of considerations given earlier), while simultaneously recording arm movements in their spatial, kinematic development. Further, we accept the existence of certain assumptions regarding different possible mechanical structures of the movement, and test the correctness or wrongness of such assumptions by comparing the dynamic and kinematic (force and spatial-motor) movement patterns. As a result of this analysis, we should determine whether it is possible to reveal a general law of the process of the rhythmic piano strike movement, and the principles of changes in forces that underlie this process.

One needs to add that rhythmic piano movements are very fast (typically, a few hundred movements per minute). Obviously, dynamic changes of muscle forces occur even faster. Time delays between kinematic and dynamic phases relatively to each other, which commonly play a crucial role in interpreting the movement structure, are, generally speaking, on the order of one thousandth of a second. These factors force us to focus our attention on the shortest time intervals.

Our studies during the last year have been based on recordings performed at a frequency of 60 per second; in further studies, the frequency has been increased up to 100–150, and sometimes even up to 500 expositions per second.

Chapter II. Methods

Our experiments were performed with the kymocyclographic method, a modification and improvement of the method of cyclography. The latter, as is well known, consists of using a motionless photo plate and making a photographic picture on it of the trajectories of the motions of lighted bulbs that are fixed to a moving organ of a subject. The kymocyclographic method replaces the motionless plate with a steadily moving photosensitive film, which makes it possible to record the smallest, repeated movements without compromising the clarity of the record.

The cyclographic equipment used in our studies had been constructed in 1924 by a member of the laboratory, P.N. Zimin. The equipment consists of a distributor box which houses the elements that control the whole system (control of bulb brightness, control of the illuminators and the motor of the revolving shutter, circuit breakers, etc.), bulbs with cuffs to place them on the hand of a subject-pianist, and a motor with a revolving shutter.

Bulbs with a diameter of 10 mm[22] are fixed on cork holders of very small weight attached to cloth cuffs. In our experiments, the bulbs were placed on the following points on the body:

bulb c^{23} = on the top of the skull;

bulb b = over the center of the shoulder joint, on the lateral side (the tip of the large prominence of the humerus);

bulb a = over the center of the elbow joint (external tip of the humerus);

bulb m = over the center of the wrist joint; and

bulb gm = close to the center of mass of the hand, near the head of the fourth carpal bone.

According to the reports of our subjects, bulbs with cuffs did not load the arm and after the few first minutes the subjects did not feel them any more. They were more confused and distracted by the light-sparkles on the hand, but they also got used to this factor very quickly. To help them get used to the illumination, the bulbs were turned on during the whole experiment, not only during the recordings.

The photographic camera used in our studies had one lens (or, during stereoscopic recordings, two), produced by Tessar Zeiss, 1:4:5 with a focal distance of 13.5 cm. In front of the lens, there was a rotating cardboard disk with narrow holes, the shutter, that was equipped with a siren to measure the speed of rotation. Figure 3 shows the shutter that was used during the recordings.

The kymocyclographic camera[24] constructed by us for the State Institute of Musical Science is shown in Figs. 4 and 5; Fig. 6 illustrates the schematics and gives cross-section drawings of the apparatus. The apparatus consists of a cassette *K* for a roll film *F*, and can be adjusted to any photographic camera of appropriate size. The film can be moved from the right reel to the left one with key *Sch*[25]; during this motion, the film passes the optical image plane of the lens. The reels can be adjusted to allow use of film of any width, from 3.4 cm (cinematographic film) to 12 cm.

The film is moved by the rotation of key *Sch* which can be turned manually or by an electrical drive (Fig. 4) with a speed that can range from 0.06 cm/s to 5 cm/s.

Rotation of the electric motor is transmitted to a flexible shaft whose free

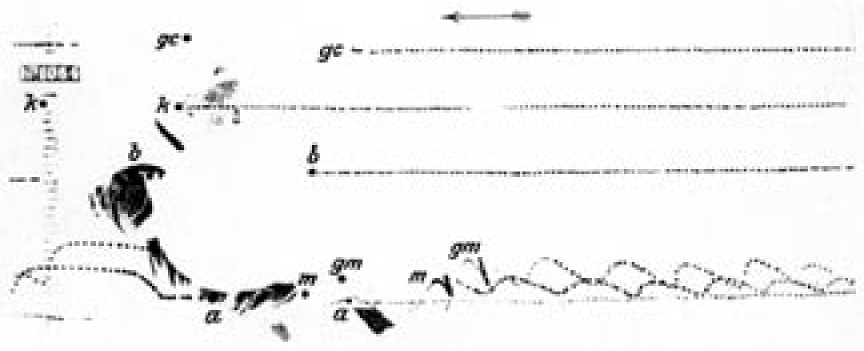


Figure 2 — Excerpt from a crescendo negative (#1064). One can see the scale plank with cm marks on the left; the right part of the Figure shows the last fragment of the crescendo kymocyclogram. The frequency of the recording is 60 strikes per second. For abbreviations, see the text.

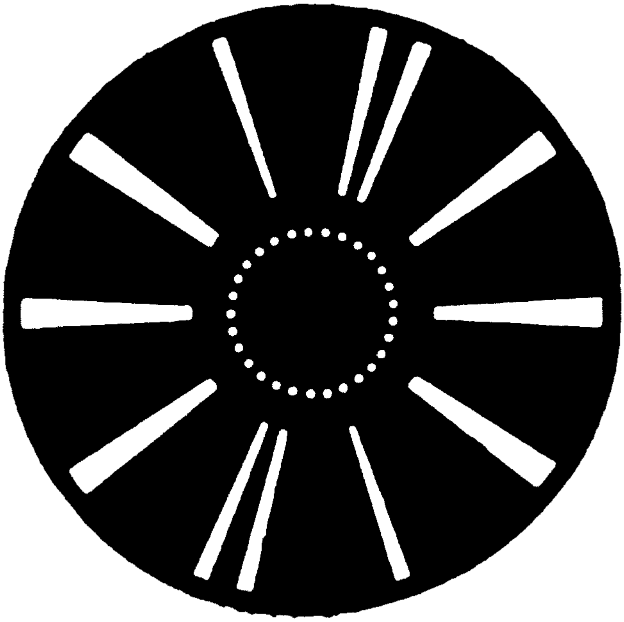
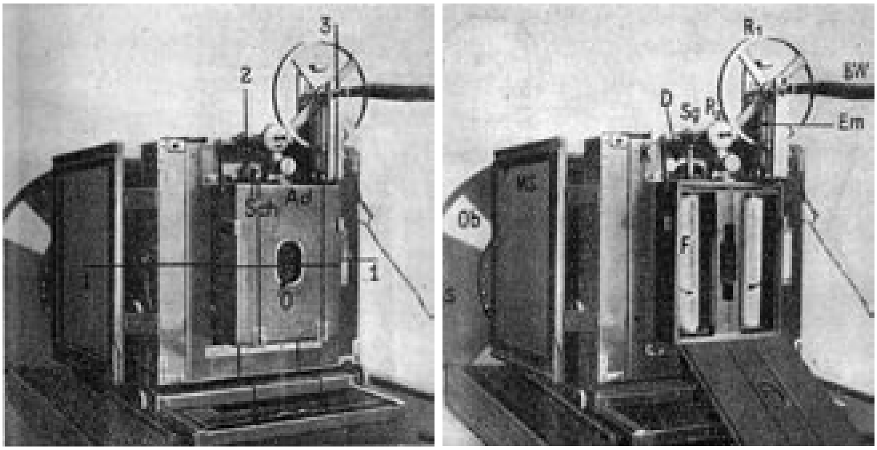


Figure 3 — The shutter used in the series of described experiments. On the circle, there are holes blocking and opening the lens; closer to the center, there is a siren for the measurement of the speed of rotation.



Figures 4 and 5 — The first model of the SIMS kymocyclographic camera (this model was used to perform the experiments described in the paper). Fig. 4 (left): Back view of the camera with the cassette closed. Fig. 5 (right): View of the camera with the cassette open. For abbreviations, see text.

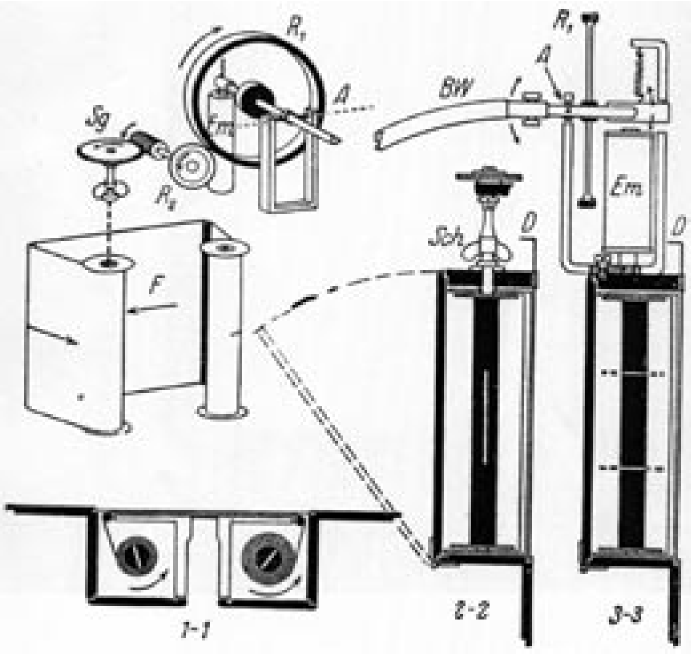


Figure 6 — General scheme and sections of the first model of the kymocyclographic camera. For abbreviations, see text.

end is fixed by two bolts on the axis of wheel R_1.

This axis has a limited freedom of swinging up and down about a transversal axis A, and is held in its uppermost position by a spring. In this position of the axis, wheel R_1 can rotate without touching the small wheel R_2.

The anchor of electromagnet *Em* is placed on the free end of the axis; the magnet itself, just above the axis, powered by a 6 V direct current power supply. When the current is turned on, the electromagnet attracts the anchor with the axis of the wheel R_1, thus pressing it against the small wheel R_2. This small wheel is equipped with a rubber roller, and is connected to key *Sch* through a helix transmission *Sg*.

Thus, the motor always moves together with the small flywheel R_1, and when current flows through the electromagnet, the motor immediately starts to move the film which stops instantaneously when the current circuit is broken.

The cassette can be oriented in the apparatus vertically or horizontally; in other words, the film can move right-to-left, top-to-bottom, or in the opposite directions, depending on the type of the recorded movement. Focusing of the system can be done either prior to recording, by using mat glass *MS*, or in the process of recording, through opening *O* covered with red glass. The cassette has a gate valve *D* which allows one to place it or remove it in full daylight.

During the time from writing this paper to its publication, its authors, together with photographer V.I. Lavrentiev, constructed two new, much more advanced types of kymocyclographic cassettes.[26] The latest model, which is presently built by the shop of W. Engelke in Berlin, has a built-in winding mechanism to assure steady motion of the film allowing one to vary the speed of film motion from 1 to 150 mm/s, as well as a number of gadgets which allows one to fix an image on the film with very high precision.

Zebek's siren, used earlier to determine the speed of the shutter, has been replaced by a newly constructed mechanism, which we have termed "nonius-siren", which allows to reach accuracy of the measurement of shutter speed up to a millionth of a second.

Our simple apparatus requires a few additions to simplify the process of measurement and of interpreting the recordings.

Photos recorded by a kymocyclographic camera apparently represent geometrical sums of: 1). The projections of actual movements of the recorded object

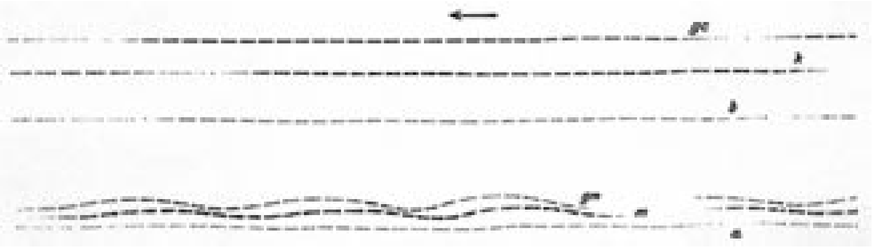


Figure 7 — The negative of a crescendo test, photographed with the second camera model at a frequency of 520 per second (negative #1109). For abbreviations, see the text.

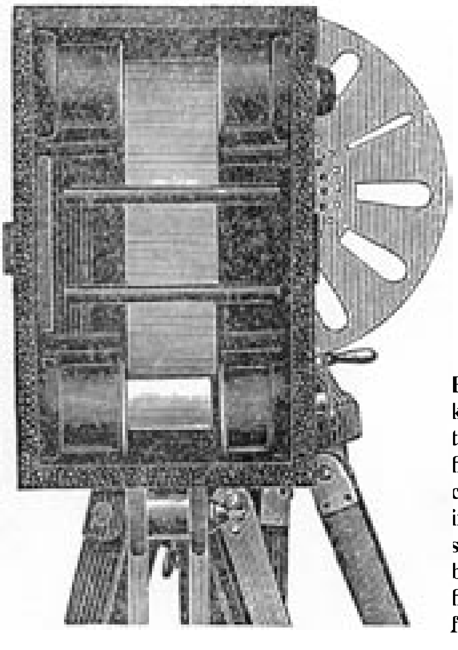


Figure 8 — The second model of the kymocyclographic camera (Laboratory of the State Institute of Labor Safety), which films up to 600 images per second. This camera was used for the recording shown in Figure 7 (520 per second). The Figure shows the back view of the camera with the back panel open, which exposes the roll of film. One can also see the shutter, designed for super-fast recordings.

on the focal plane of the lens; and 2). The movements of the film itself. Therefore, we need to subtract the motion of the film from the overall result to reconstruct the actual pattern of the studied movement.

This is done most conveniently by placing into the visual field of the apparatus, in addition to the bulbs that are fixed on the subject and participate in the movement, a motionless control bulb *K*, commonly fixed on a scale (see below). This bulb is photographed together with all the other bulbs through the rotating shutter during the motion of the film, and draws on the film a dashed line which exactly corresponds to the motion of the film itself. Further calculations of the positions of moving points with respect to the corresponding time points of the dotted control line allow one to eliminate the motion of the film itself. Let us add that, in cases when film motion can be considered steady with certainty, velocity components of each moving point in the direction of the film motion differ from the actual velocity of the same point by a constant. Velocities in a direction perpendicular to the motion of the film, as well as accelerations in any direction under the same condition, are identical to actual velocities and accelerations.

We also need to establish the time correspondence between different trajectories on a kymocyclogram, i.e., to define which points on each of these simultaneously recorded trajectories correspond to one and the same time moment. If recording continues for a relatively long time, and includes simultaneous movements of a number of independent parts of the body (for example, of the head and of an arm), identification of such corresponding points can sometimes present significant difficulties.

First of all, the solution of this problem is helped by the fact, as seen in Fig. 3, that the openings on the shutter are not all of the same size. The presence of a single narrow opening, translating into a narrow dash in the dashed line, allows one to establish time correspondence among dashed lines within short time intervals, of not more than a few hundredths of a second. To assure this possibility for longer time periods, up to a few seconds, we allow the current to the bulbs to flow through a small, serial rheostat which can also be bypassed by the current via a shorter loop. In the latter loop, there is a metronome driven circuit breaker which periodically breaks the short loop for a few hundredths of a second. If such a “second clock arm” is used, turning down the light in all the bulbs simultaneously, the identification of corresponding points does not present further difficulties.

The recording was performed in the following way. A subject sat in front of the piano on a stool; a vertical scaling plank with centimeter marks was attached to the back of the stool and was photographed prior to each recording in order to define the spatial scaling of the movement. The following was measured during each experiment: the distance from the lens to the scale plank, the height of the control bulb with respect to the lens, arm segment lengths defined as the distances between corresponding pairs of bulbs attached to the arm, and the weight of the subject. The right arm of the subject was photographed, to which the bulbs were attached. Correspondingly, the camera was positioned to the right of the subject, at a distance of 1.5 to 2.5 m. For test records, the non-stereoscopic setup was used with horizontal motion of the film; for records of the concert excerpt, the stereoscopic setup was used with vertical motion of the film. The shutter was set at a speed of 6 revolutions per second which corresponded to 60 openings of the lens per second.

To get used to the setup, the subject was asked to play anything at all for a few minutes, with the bulbs attached. The subject was given one of the tests, as written notes, and asked to perform it. To obtain movements from each subject that were as natural as possible, no additional instructions were given except those that were related to the most exact performance of the task (tempo during the *crescendo* test, average required force in the *accelerando* test, etc.). Thus, the subject was absolutely free to choose sitting posture, arm posture, and the whole style of performance.

After a certain test had been properly understood (some subjects asked to be informed about the tests in advance), the subject was asked to perform the test movement a few times in a row, and one of the repetitions, unknown to the subject, was recorded. After the experiment was over, we asked the subject to write a self-observation report relevant to the test performance; however, as further processing has shown, these reports were commonly incoherent and contained little in common with what was revealed by the record.

The recordings were typically done by N.A. Bernstein, with participation of P.N. Zimin and A.S. Sheves.

The film negatives were projected, at a high amplification, from above upon the horizontal surface of a table, and then measured. A piece of white paper was placed on the table, and the positions of all the bulbs, including the control one, were drawn by a pencil. Amplification was adjusted in such a way that the projection was about half the actual size. This was achieved by comparing the image of the scale plank, photographed in each negative, with a centimeter ruler.

Each negative included 10 to 15 s of movement, corresponding to 500 to 1000 points on each of the light trajectories. Because of the extreme difficulty in measuring all these points, we usually selected from a negative one or a few episodes of 1 to 1.5 s duration, which were measured and analyzed. Each such episode included 2 to 3 strikes (during slow tempi) up to 10 strikes (during fast tempi).

Processing of the measured data was done with methods described by us in detail in other works[27]; therefore, we will not describe this in detail here. Graphodynamic methods were used to define motions of the centers of mass of individual arm segments, their acceleration, and the inertial forces at these points. Further, the force moments in joints were measured, representing, as mentioned earlier, the resultant forces of muscular contractions. Besides that, we measured changes in the wrist and elbow joint angles, vertical displacements of the centers of gravity of individual segments, etc. The measured values were expressed in degrees for angles, in kg of weight for linear forces, and in kg*cm for force moments.

Chapter III: Experimental Results

Typical curves that we used as material for our analysis are represented in Figure 9. The trajectories of bulbs recorded during the experiment are shown in the Figure by the following letters:

gm = center of gravity;
m = center of the wrist joint;
a = center of the elbow joint;

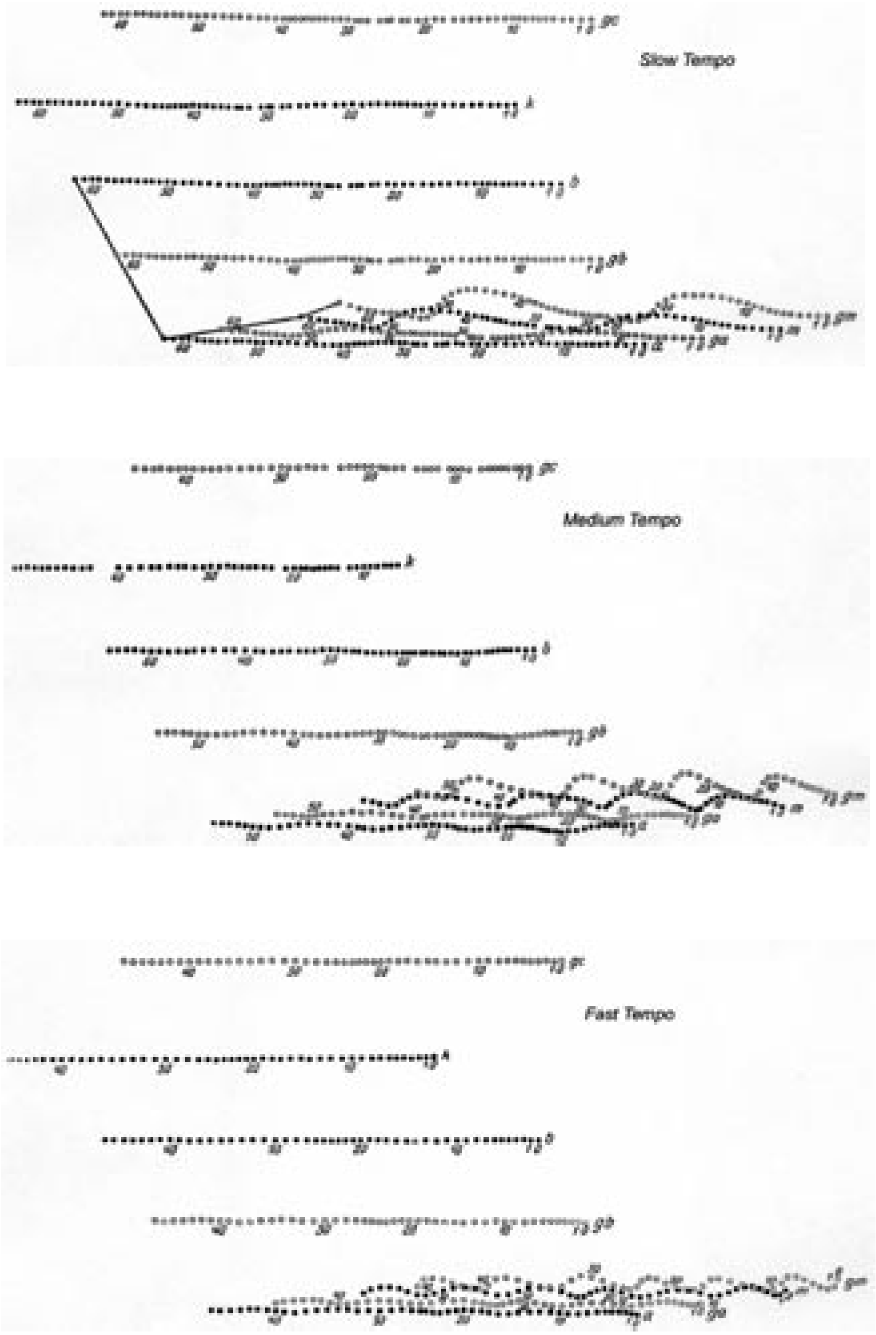


Figure 9 — Three fragments from an accelerando negative, redrawn for measurement at a higher resolution (negative #1055, cf. Figure 17). The subject is pianist K. (#12). For abbreviations, see the text.

b = center of the shoulder joint;

gc = top of the skull;

k = motionless control bulb.

These curves represent the summation of the movements of the points under investigation and those of the film itself which moved in our experiments from left to right; hence, the curves should be read from right to left. Any rhythmic movement presents itself in such curves in the form of repetitive waves while obviously vertical shifts being shown as ascending and descending of the curves, and horizontal shifts (to the right and to the left, in correspondence with the drawing) are reflected as an increase or a decrease in the density of the points of each curve. As said before, all the photographs are made from the right side, making a side view of the subject, in such a way that increases in point density correspond to arm movement forwards, while decreases in the density correspond to arm movements backwards.

Because of the imperfection of the first model of the camera, as used for this series of experiments, the dotted trace of the control bulb also shows more and less dense segments, due to the unsteady motion of the film. These artefacts, however, were eliminated with rather simple methods described in the aforementioned paper[28] (p. 661).

Corresponding to our normal speed of filming (60 images per second), each trajectory contains 60 points per second while, obviously, the number of points is the same for all trajectories. If one selects one point on each trajectory so that all these points correspond to each other in time (or, which is the same, they all have the same number starting from the beginning of the trajectory), a shapshot is obtained of a particular phase of the movement. Connecting these selected points by straight lines, one gets the positions at this particular moment in time of the axes of the individual segments of the arm such as the upper arm, the lower arm, and the hand, i.e., the instantaneous posture of the arm (see Figure 9, top). This method allows us therefore to monitor arm position continuously at a frequency which is several times higher than that of conventional recording. We used this method to determine joint angles, to be discussed later.

Figure 9 also shows the curves *ga* and *gb* which are already results of preliminary processing of the record. These curves are sequential positions of the centers of gravity of arm segments, of the forearm (*ga*) and of the upper arm (*gb*). We are not going to discuss the methods of calculation of these curves.

Curve *gm* represents the trajectory of the hand center of mass; it was obtained directly by filming the bulb attached at the fourth metacarpo-phalangeal joint. During finger movements, the center of gravity of the hand shows substantial migrations within the hand; nevertheless, in our case, where finger movements were all but eliminated by the selection of the tests, the location of the center of gravity within the hand could be considered constant. Hence, its motion is reflected by the motion of bulb *gm* with sufficient accuracy.

Knowing the motions of the centers of gravity allowed us to determine inertial forces defined as the product of the mass concentrated in the center of gravity and its acceleration. Direct measurement of the masses of human arm segments encounters insurmountable difficulties; therefore, we used the method of Otto Fischer based on the body weight of our subjects. Calculation of accelerations was performed using the grapho-dynamic method described in the aforementioned paper (l.c., pp. 662-663).

In the first Chapter, it has been said that during pendular movement, arm muscles need to deal with forces of two categories: forces of inertia and forces of gravity. The force of gravity acting on each of the studied centers of gravity is defined as the product of its mass and the acceleration of gravity, $g = 981$ cm/s^2. Thus, if one denotes acceleration of the center of gravity as w, the total forces interacting with the muscles can for each center of gravity be expressed as:

$$f = mg - mw = -m(w - g),$$

where $(w - g)$ should be considered as the geometrical sum. All the elements necessary for calculation of f are available.

However, the use of muscle forces is determined not by these forces f themselves, but by their moments with respect to the centers of the corresponding joints. Muscle moment in a joint should be equal in magnitude to and directed against the moment of force f with respect to the same joint. Since moment is calculated as the product of force × the length of the lever arm and × the sine of the angle between the two, it is clear that the same magnitudes of f can correspond to very different magnitudes of its moment depending on the length of the lever arm and on the angle between the force and the lever arm. If, for example, the arm hangs loosely down, the angle (and, therefore, its sine as well) between the force of gravity and the lever arm with respect to any of the arm joints is equal to zero, and, therefore, the arm can hang down while its muscles are completely relaxed despite the fact that the force of gravity acting on it preserves its constant value. Hence, all problems related to the muscle structure of a movement can only be solved on the basis of calculations of muscle moments which we are going to describe now.

Figure 10 summarizes curves of muscle moments and changes in joint angles in a typical case of performing the test *crescendo-diminuendo*. We chose this case for our initial explanation because it corresponds to a medium tempo (6 strikes per second) in which all the details can be observed with much clarity.

Solid curves in Figure 10 depict muscle moments, and dashed curves show changes in joint angles. Moment curves, top to bottom, correspond to moments in the wrist, the elbow, and the shoulder, respectively, while angle curves show the angle between the longitudinal axes of the hand and the forearm (the top curve), as well as between the axes of the forearm and of the upper arm (the lower curve). Angular scales in degrees are shown to the left of the ordinate axis. Time in tenths of a second is shown on the bottom.

Force moments were calculated in kg*cm. In the case shown in Figure 10, wrist moments vary from +30 to –20 kg*cm, elbow moments from +160 to –70 kg*cm, and shoulder moments from +300 to –80 kg*cm. The relation among the amplitudes of the three curves was approximately preserved in all our negatives, i.e., the range of effort in the elbow is about five times bigger than that in the wrist, while the range of efforts in the shoulder is eight times bigger than in the wrist.

The wrist curve and, to a lesser degree, the elbow curve preserve during successive strikes a certain repetitiveness, a certain constancy of form. This cannot be said about the curve of the shoulder moments which continuously shows very irregular zigzags. It is very hard to give an interpretation for such an improper behavior of the shoulder curve; later, we will try to suggest ways towards generating such an interpretation.

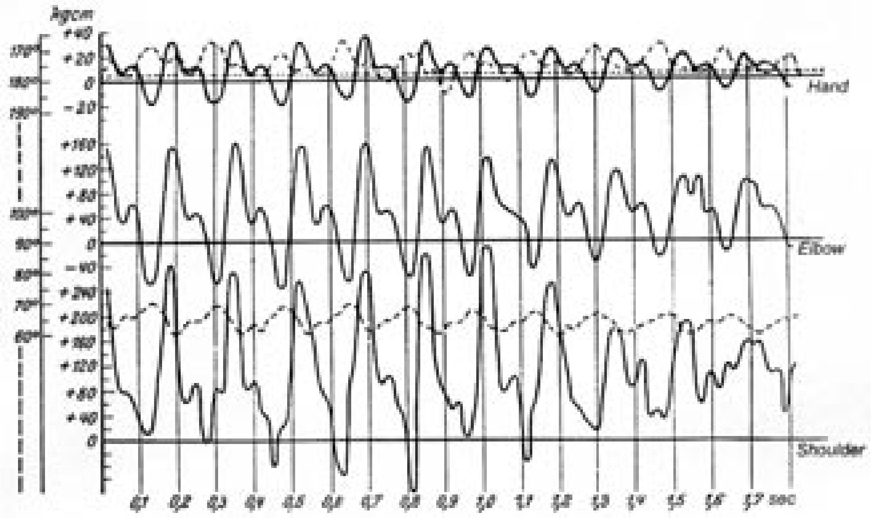


Figure 10 — Muscle moments (solid curves) and joint angles (dashed curves) from a diminuendo negative (#1054, subject K.I., #12). Successive ordinates designate tenths of a second (similar as in Figures 11, 17, and 18).

Let us first focus on the interpretation of the moment curves. Positive moments, shown above the abscissa axis, correspond to lifting muscle forces (more precisely, to forces rotating the limb in the plane of Figure 9 counter-clockwise); negative moments correspond to forces that draw the limb down. In cases where the moment is zero, i.e., at times when the moment curve crosses the abscissa axis, the resultant of muscle forces acting on the given joint is zero. In other words, at these times, the joint is allowed to behave the way it wants and the attached segment of the arm drops freely under the action of gravity.

The moment of the force of gravity with respect to the joint equals the product of the weight of the arm segment attached to the joint, the lever arm (i.e., the distance from the center of gravity to the center of joint rotation), and the sine of the angle between the lever arm and the vertical line. This is exactly the moment that induces limb motion at times when the moment curve crosses the abscissa. In cases where the muscle moment represented by the curve is equal to the moment of the force of gravity, and directed against it, the limb segment attached to the joint is in a perfect balance, i.e., it is either motionless or moves at a constant angular velocity (more precisely, with a constant rotational moment). Since both the lever arm and the angle between the lever arm and the vertical change very little during hand movements in piano playing, we can draw a straight line on the plot (dotted horizontal line in the upper part of Figure 10) of a magnitude that is equal to and directed against the moment created by the weight of the hand. At times when the moment curve for the wrist crosses this dotted line, the hand is not under action of forces, i.e., it is moved by its inertia only.

Thus, we can identify three zones in the curve of muscle moments acting at the wrist which are characterized by different movement outcomes. If the curve is above the dotted line (let us call this straight line the equilibrium line), the segment is driven by forces acting upwards, i.e., it is being accelerated upwards.

(It is very important here to realize clearly that we are talking about upwards acceleration, not movement. An acceleration directed upwards can result in a decrease in the velocity of the limb if it moves downwards or in an increase in the velocity if the limb moves upwards. If motion of the limb is plotted as a curve, upwards acceleration will be seen as a counter-clockwise curvature of the curve, while downwards acceleration will be seen as a clockwise curvature. Episodes without acceleration will be seen as straight segments.)

So, parts of the moment curve that are above the equilibrium line correspond to upwards acceleration of the hand. Parts of the curve below the equilibrium line correspond to downwards acceleration. From what we said earlier, it follows that parts of the moment curve that are below the abscissa axis show active acceleration downwards, i.e., *an acceleration that is due to active contraction of muscles pulling the segment down*. Curve fragments that are below the equilibrium line but above the abscissa axis correspond to a downward acceleration which takes place without *a contribution from muscles pulling downwards*, only due to the force of gravity. When the moment curve crosses the abscissa, the force of gravity acts without any resistance; at the level of the equilibrium line, the force of gravity is completely balanced by the action of elevating muscles. Therefore, in the interval between the abscissa and equilibrium lines, the action of the force of gravity is balanced to a larger or smaller degree by the braking influence of elevating muscles while there is absolutely no action by muscles acting downwards.

It follows clearly from the assumptions of those theoreticians of the weighted play, who think that striking the keys results from a free fall of the arm due to its weight, that the moment curve should not drop below the abscissa axis. This lower area becomes forbidden for a weighted strike. In our experiments, we analyzed a large number of pianists, including representatives of the weighted playing school; however, in all the cases, as in Figure 10, moment curves do not obey this restriction. Exceptions are seen in certain cases of very slow tempi which we will consider later.

During typical playing configurations, the moment of the force of gravity is about 6 kg*cm for the wrist, about 40 kg*cm for the elbow, and about 100 kg*cm for the shoulder. At the same time, the active component of downwards efforts was up to 25 kg*cm for the wrist, up to 70 kg*cm for the elbow, and up to 80 kg*cm for the shoulder. In other words, active resources exceed passive, weight-related resources for the hand by a factor of three or four, for the forearm by the factor of 2, while for the upper arm they are nearly equal to each other. Analysis of these relations in time is even more impressive: The time when the moment curve is within the "weight margin" is only one-fifth to one-tenth of the whole time when a downward acceleration acts on the hand.

To understand the meaning of the different phases of the moment curve, we need to consider joint angle curves as shown in the same Figure 9.[29] These curves show wrist extension and elbow flexion (i.e., motions lifting the hand) upwards, while the opposite, lowering movements are shown downwards.

The moment of strike is seen on all the moment curves as a peak (for example, in Figure 10, at times 0.19, 0.36, and 0.52 s). In cases where a cycle shows a double-peaked curve, the strike always corresponds to the higher peak. The origin of this apex is easy to explain. At the time of a strike, besides all the forces acting during the course of the pendular movement,[30] the reaction force from the key starts to act upwards. Since this force is unknown to us, and we cannot exclude it from the summed force curve, it reveals itself as an upwards directed notch. The apex of the peak, therefore, does not correspond to instantaneous muscle force but rather to its sum with the reaction of the keyboard. Since the time of contact between the fingers and the keyboard is small (about 0.01 s), losing these brief segments of the curve would not devalue its remaining segments.

Prior to a strike, there is a period of accelerating downward movement of the hand characterized by the moment curve being in the intermediate or the lower zone. According to our calculations, between this period of accelerated downwards movement and the moment of contact with the keys, there is a very brief braking period, i.e., an active muscle effort directed upwards. However, more refined experiments are required to investigate this point.

After the instant of strike, the upwards acceleration of the hand and the forearm is initially very high because of the rebound, but then it quickly drops to zero or even lower and, in some pianists, is substituted for a short interval by a downward acceleration (below the equilibrium level, sometimes even below the abscissa, see Figure 11). Then, a new increase in the upward acceleration occurs, after which the curve drops abruptly creating an ultimate striking acceleration directed downwards. Such double-peaked curves for the wrist and elbow moments are very typical for medium tempi (5 to 7 strikes per second) in all the studied pianists; presently, it would be very hard for us to suggest a satisfactory interpretation for the curve.

To gain a better understanding of the meaning of different parts of the moment curve, an understanding that will provide support for the rest of the analysis, let us consider Figure 12 which shows a schematic picture of a typical moment curve (solid line) and joint angle curve (dotted line) for the wrist joint during playing at

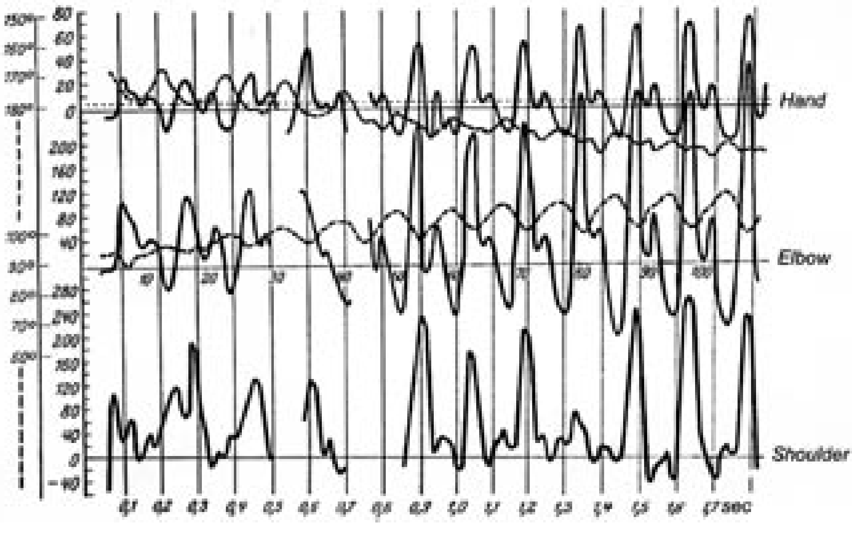


Figure 11 — Muscle moments (solid curves) and joint angles (dashed curves) from a crescendo negative (#1030, subject pianist E.P., #10).

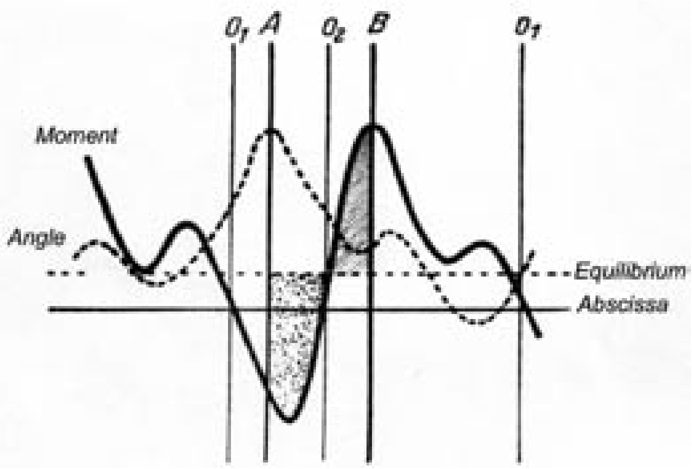


Figure 12 — A schematic drawing of a muscle moment curve (solid line) and a joint angle curve (dashed line) for the wrist joint. Medium tempo. For details, see the text.

medium tempi. The instant of strike is shown by ordinate *B*.

Let us denote as time *A*, the time that corresponds to maximal wrist extension. This time does not correspond to the maximal elevation of the hand center of gravity, differing from the latter by a few thousandths of a second; we will suggest an explanation for this phenomenon during further analysis of the test *accelerando*.

Time *A* always corresponds to large (not necessarily maximal) downward directed forces. If at time *A*, angular velocity of the wrist can be considered zero, the downwards directed forces will increase it in the direction of flexion (let us assign these changes negative values) up to a point when the moment curve crosses the equilibrium line (point O_2). The area between the equilibrium line, ordinate *A*, and the moment curve from this ordinate to point O_2 (filled) corresponds to the rotational impulse accumulated by the hand during its lowering, and at O_2 this impulse will be maximal. Further increase of the moment curve above the equilibrium line, which corresponds to lifting acceleration, leads to a decrease in the impulse so that the hand approaches the keyboard with a decreased momentum. At the instant of a strike (*B*), the hand stops and its impulse becomes zero. Therefore, the area limited by the moment curve from point O_2 to ordinate *B*, ordinate *B*, and the equilibrium line, should be equal to the area under the curve from ordinate *A* to point O_2 mentioned earlier.

Time O_2 during a striking movement and time O_1 during a lifting movement, both correspond to the maximal value of the momentum. This value increases from zero to a maximum between points *B* and O_1, and again drops to zero between point O_1 and *A*.

Note that intervals AO_2 and O_2B are approximately equal to each other, while interval BO_1, corresponding to the accumulation of upwards momentum is several times longer than interval O_1A during which the momentum is lost and the hand ultimately stops in the highest position. Correspondingly, lifting forces during interval BO_1 are very low. As mentioned earlier, they sometimes become zero or, for a short period, are replaced with counter-directional, flexing forces (Figure 11). During slow tempi, this sluggishness and frugality of the lifting effort is seen even more clearly, only during the fastest tempi, when the double-peaked shape at the BO_1 interval disappears altogether, and phenomena of a different nature can be seen.

At medium tempi, the angle curve does not follow exactly the force curve. This is easy to understand if one remembers that joint centers are themselves moving and, therefore, changes in the momentum of an arm segment are not unambiguously related to changes in angular velocity in the corresponding joint. For example, the moment of impact does not correspond to maximal wrist flexion. In Figure 12, it can be seen that, during the *AB* interval, the wrist performs a fast and energetic flexion, followed by a low extension wave at the moment of impact (due to the reaction force). Then, simultaneously with the beginning of the next lifting of the hand from the keyboard, a new, again rather large wrist flexion starts and continues up to its limit during interval BO_1. This flexion, accompanied by hand lifting rather than lowering, is dependent upon an energetic lifting of the forearm with a more pronounced effect than the inevitable hand lowering induced by the wrist flexion. New wrist extension starts only just prior to the time when the hand, being drawn by the forearm, reaches its maximal upwards motion (point O_1). During the O_1A interval, the hand quickly loses the accumulated upwards motion while extension continues more vigorously, until its maximum. This is possible only because the forearm stops and begins to move down earlier than the hand, whose motion is, therefore, related not only to the upwards movement of its center of gravity but also with the simultaneous lowering of the wrist.

Force moments in the elbow follow closely the moments in the wrist and differ from them only in magnitude. The elbow shows maxima and minima of positive and negative (lifting and lowering) forces in perfect synchrony with the wrist, and the second, smaller peak of upwards forces in the elbow moment curve also arises in absolute synchrony with the corresponding peak of the wrist curve.

However, changes in the elbow angle differ from changes in the wrist. The striking flexion of the wrist frequently takes place somewhat earlier than the maximal elbow extension (Figures 10 and 13, *a*); in other words, the elbow continues to extend, and the forearm continues to move down during 2 to 3 hundredths of a second after the strike.

Since the fingers cannot move further down after hitting the keyboard, naturally, the period of elbow extension is accompanied by a small wrist extension (a small bump on the curve of wrist joint angle in Figure 13, *b*). This link between the two phenomena is also confirmed by the fact that, in cases where the small bump of the wrist angle is barely seen, there is visually ideal synchronization between the lowest points on the elbow and wrist joint curves (Figure 11). We may add that, according to our observations, cases as illustrated in Figures 10 and 13 (separation of the minima and a double-peaked wrist angle curve) correspond to a more “free” arm, while the case illustrated in Figure 11 probably suggests a more “fixed” arm.

At the beginning of arm lifting immediately after impact, for a short time counter-directional motions arise in the elbow and wrist joints, observed in all studied cases. The beginning of elbow flexion always coincides in time with the beginning of wrist flexion, and the wrist moves up to its maximal flexion, commonly

6° to 8° more than its flexion at impact. Therefore, during this period, which also lasts about 2 to 3 hundredths of a second during medium tempi, the forearm and the hand rotate in opposite directions. The following hand rotation upwards acts on the forearm (hand reaction) and delays its flexion or even sometimes leads to a short-lasting extension blip (Figures 10, 11, and 13, *c*).

Figure 14 illustrates an attempt at a schematic summary of all the major force and kinematic phenomena which always repeat themselves in this particular sequence in all the cases of playing at medium tempi.

Now we can start to analyze differences in the piano strike induced by changes in the *force and tempo* of the movement. Here, we encounter very consistent behaviors in pianists of very different schools and directions.

The effect of an increase and decrease of the *strike force* can be seen in Figure 11 (*crescendo*) and in Figure 10 (*diminuendo*), showing the records of two very different, renowned *virtuosi*. The first obvious change is in the *amplitude of the wrist and elbow moment curves*. They increase during *crescendo* and decrease during *diminuendo* with high regularity, absolutely parallel to each other. There is no such a regularity in the shoulder forces. However, as a rule, an increase in the force of the strike (beginning of Figure 9 and end of Figure 10) leads to a more regular shape of the shoulder force wave which is virtually destroyed during relatively weak strikes. This observation suggests that, during strong strikes, there is an active contribution of the shoulder joint which generates regular active impulses directed downwards (the shoulder moment curve is below the abscissa line). During weak strikes, the shoulder joint may not be involved actively but only compensates, *in a reflex fashion*, rather complex reactive phenomena emerging because of the active forces in the elbow and wrist joints. Note, that we failed to see a similar destruction of the elbow moment curve, even during the weakest strikes.

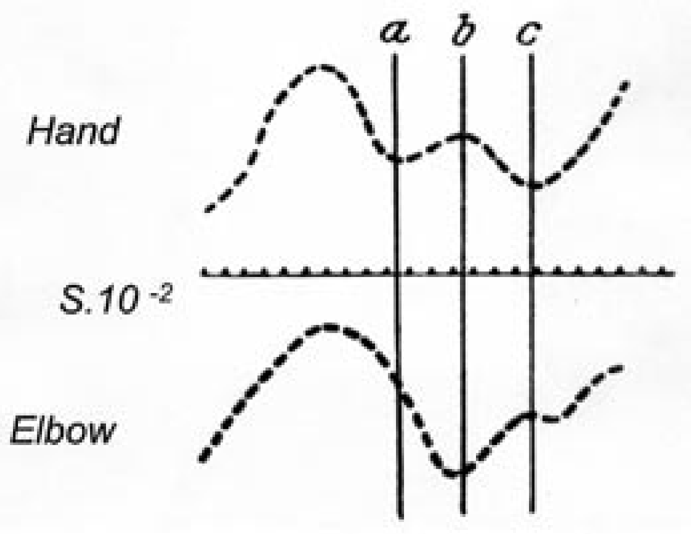


Figure 13 — A scheme of joint changes in the wrist and elbow joints during one strike cycle at medium tempo.

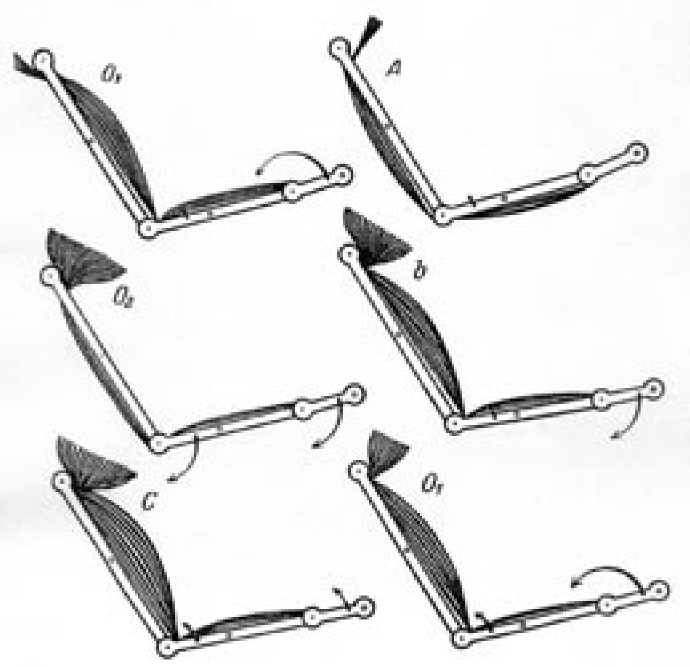


Figure 14 — A summarizing scheme of the kinematics and forces for a typical piano strike (after negative #1055). Angular velocities in the joints are shown by curved arrows, muscle moments are shown by the location and relative thickness of the muscles. We have to note that "muscles" in this Figure are symbolic representations of resultant muscle moments, in no way representing actual anatomical units. For abbreviations, see the text, and also Figures 12 and 13.

Changes in the force amplitudes during *crescendo-diminuendo* are very significant, and Figures 10 and 11, which show only small episodes of the movement, do not reflect them adequately. Nevertheless, it is significant to point out that the relationship between the amplitudes of the wrist and the elbow remains very stable. This fact unequivocally suggests that the elbow and the wrist, during a piano strike, represent a tightly linked system whose biomechanical (and, probably, also innervational) unity is contrasted by the much higher independence of the shoulder.

Modifications of strike strength are also accompanied by a similarly unchanged presence of "active-striking" forces, i.e., a lowering of the moment curves for both elbow and wrist below the abscissa axis. The depth of this dip changes more or less proportionally to the overall amplitude of the force curve, but the phenomenon never disappears; on the contrary, its relative magnitude is higher during *p* than during *f* (Figure 11). Hence, neither the strongest nor the weakest strikes, at medium tempi, arise due to the weight only. *The degree of participation of the active striking effort in the overall striking effect does not depend on the strike strength.*

Similarly, there are no changes in the relationship between contributions of the elbow and wrist muscles and changes in the strike strength. *Our experiments do not confirm transfer of force from the wrist to the elbow during* crescendo; however, transfer from the forearm-wrist system to the shoulder joint probably takes place.

Since the construction of the movement, as reflected in the form of force and angle curves, does not change during *crescendo* and *diminuendo*, changes in the strength of a strike must inevitably be tied to changes in the amplitude of the movement itself (Figure 15). It is mainly the amplitude of oscillations in the *elbow joint* (Figures 10 and 11) and, therefore, the wrist trajectory, that changes parallel to changes in strike strength. The amplitude of wrist motion may remain unchanged (Figure 10) or even change in the opposite direction (dropping during *crescendo*) which should probably be attributed to an increase in the fixation of the wrist. There are peculiar kinematic changes during *crescendo* in a *virtuoso*[31] whose records are shown in Figure 11 (this pianist, however, was not satisfied with the octave technique of his right hand): An increase in the strength of the sound is accompanied by a more and more flexed and fixed wrist, while the elbow steadily flexes, increasing its movement amplitude (Figure 16).

While changes in the dynamics during *crescendo-diminuendo* are of exclusively quantitative nature, differences induced by *changes in tempo* are incomparably more deep and qualitative. One may say that, during slow (2 to 3 strikes per second), moderate (5 to 6 strikes per second), and fastest (8 to 9 strikes per

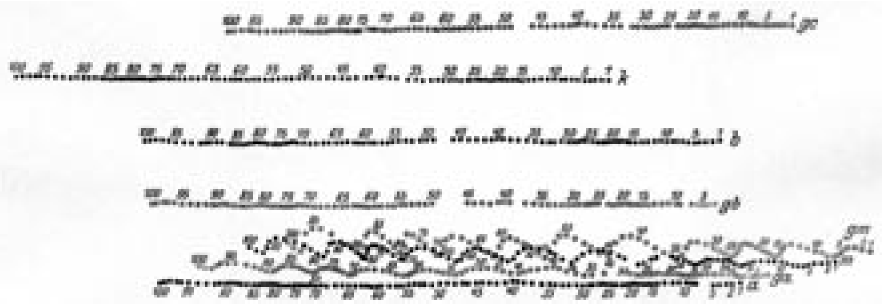


Figure 15 — Crescendo negative (#1039), redrawn for measurement.

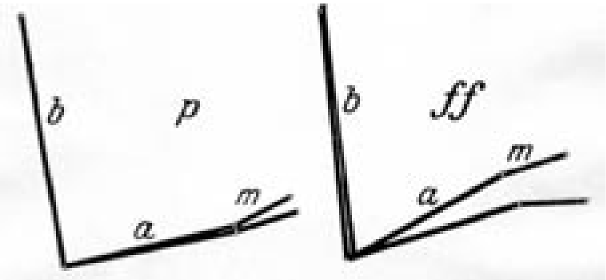


Figure 16 — A scheme of a subject's arm (E.P., see Figures 11 and 15) in its limit positions during a single strike. Left: piano. Right: fortissimo.

second) tempi, we witness three absolutely different force constructions with three movement mechanisms that are very much unlike each other. Obviously, a comparative study of these data should provide rich material for an understanding of each of the three mechanisms.

Let us look at Figure 17. This Figure unites three episodes from the test *accelerando* performed by the same *virtuoso* whose curves are shown in Figure 10. The left part of the Figure was taken from the beginning of his kymocyclogram (slowest tempo), the middle part from the medium tempo, and the right part from the fastest tempo. The test was performed with approximately the same strike strength *mf*. Labels on the Figure are the same as in Figures 10 and 11.

The middle part of Figure 17 shows the familiar pattern of a medium tempo and medium strike strength, analyzed in the earlier text. One can see the double-peaked force curves for the wrist and for the elbow, the irregular pattern of the shoulder force curve, and the relations between the wrist and elbow angle curves which basically repeat Figure 13. However, both the left and the right part of the Figure present very unfamiliar patterns.

The slow, left part contains familiar elements. If one draws ordinates *C* and *F* corresponding to the beginning and the end of a striking movement, parts of the force curves for the wrist and for the elbow between the ordinates are nearly identical to those for the medium tempo. Beyond these ordinates, there is an undefined, "lingering", nearly static state of the muscles which shows some oscillations[32] only in the shoulder; the role of these oscillations is unknown to us. Everything suggests that, at slow tempo, there are separate impulses, isolated from each other, interrupted by episodes of relative tranquillity. At medium tempo, these impulses retain the same structure and the same duration, but they merge into a single, continuous chain. We have not yet been able to define at which tempo such merging takes place for the first time.

The most significant difference between the *CF* impulse during the slow tempo and the force patterns during the medium tempo is that, during the slow tempo, the dropping of the force curve below the abscissa *occurs rarely or even not at all*. In this situation (the only one), the ideal case of a weighted strike is

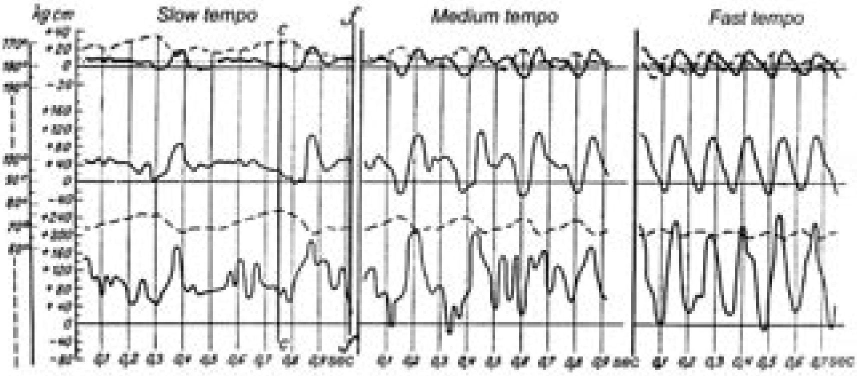


Figure 17 — Muscle moments (solid curves) and joint angles (dashed curves) corresponding to the three episodes of the accelerando negative in Figure 9. Abbreviations are the same as for previous Figures.

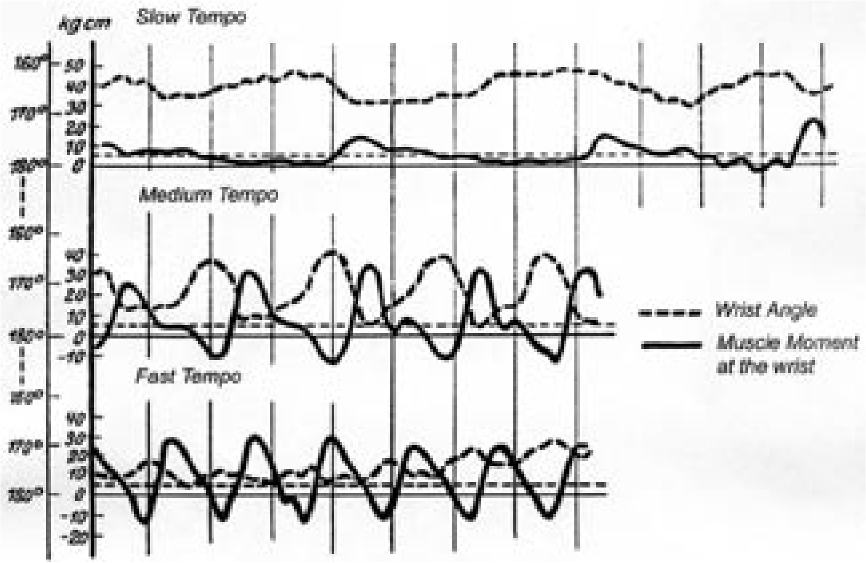


Figure 18 — Muscle moment and wrist joint angle in another subject (pianist A.B., #13) during accelerando (negative #1062).

close to being realized. Figure 17[33] represents a case of total compliance of the elbow and wrist curves to the "weighted play rule". We have never observed such phenomena, except during the slowest tempi, and in the pianist illustrated in Figure 17, as in all others, the phenomenon disappears when the tempo accelerates to 4 or 5 strikes per second.

The pattern of the fastest tempo (the right part of Figure 17) is significantly different from that seen during the medium tempo. First, the double-peaked shape of both wrist and elbow force curves disappears and is replaced by a slight asymmetry. Second, the curve of the shoulder moments, for the first time, starts to display clear regularities which are, however not as impeccable as the beautiful regularity of the wrist and elbow curves. Third, the double-peaked shape of the wrist angle curve becomes considerably less pronounced. Finally, there is a whole spectrum of more subtle differences from the medium tempo which we will be able to analyze only after a brief divergence; these will ultimately prove to be the basic features that determine all other manifestations of the difference between the medium and fastest tempi.

First of all, let us try to find out what the minimal possible forces are, necessary to generate movement of a body that is mechanically identical to the human hand, movements with the same spatial amplitudes and tempi that we have observed in real life.

This question requires an explanation. If a body or a system performs a rhythmic oscillation with a certain spatial amplitude A and at a certain tempo (n times per second), there exists a certain minimal magnitude, F_0, of forces that are necessary to effect such a movement; forces cannot drop below this magnitude without violating either amplitude A, or tempo n. The amplitude of force changes, F, can be higher than the minimum F_0, but never lower.

The lowest force amplitude, F_0, will occur when system oscillations are simple, harmonic, at a single period, and at a single phase. All other, more complex patterns of oscillations require forces of higher amplitude. Hence, by analyzing the difference between forces during piano playing and the lowest possible ones, we address the issue of the difference between a piano strike movement in our tests and a simple harmonic oscillation.

Let us start from the simplest formulation of the problem. First, let us calculate the lowest possible accelerations that have to act on the hand center of gravity so that it moves at frequencies and with amplitudes observed in our kymocyclograms.

A harmonic oscillation with frequency n and amplitude A obeys the following equation:

$$x = A \sin(2\,\pi\, nt),$$

if one ignores the phase. Acceleration during such an oscillatory movement will be expressed by the equation:

$$\frac{d^2x}{dt^2} = -4\pi^2 n^2 \sin(2\pi nt).$$

Therefore, the acceleration will always be between $+4A\pi^2n^2$ and $-4A\pi^2n^2$, i.e., the amplitude of its change will be $8A\pi^2n^2$. Let us denote this value W_0. It corresponds to the minimal possible amplitude of acceleration for oscillations with an amplitude A and at a frequency n.

The following Table (Table 1) presents the data of three virtuosi whom we studied in this respect.

First of all, let us note that at tempi over 3 per second the lowest possible amplitude of acceleration is always over 2000 cm/s^2.[34] If one imagines a body dropping due to the force of gravity with acceleration g = 981 cm/s^2, and moving upwards at a similar acceleration, the overall amplitude of acceleration changes will be under 1960 cm/s^2. Hence, in all the studied cases (except tempi at less than 3 strikes per second), tempi and characteristic movement amplitudes are such that, even in the best possible scenario, the movements could not be due to the field of gravity.

The last column in Table 1 shows the ratio between the minimal necessary amplitudes of acceleration (W_0) and resources supplied by the weight. These minima exceed the weight factor, 2g, 1.5 to 3-fold during medium tempi at *mf* and 4 to 6-fold during fast tempi and *fortissimo*. In other words, during these tempi and with these strike forces, weight manner of play is not only absent (as mentioned earlier), but even theoretically impossible. One needs accelerations 2 to 4 to 6 times larger.

Now, the time has come to see not the theoretical minima but the actual accelerations demonstrated by the pianists, which, as mentioned above, can be above the theoretical minimum. The results of these computations are presented in Table 2.

The data in Table 2 are particularly interesting. First, the Table shows that actual hand accelerations are always considerably higher than the theoretical minimal accelerations (W_0); only for the fastest tempo, these accelerations become equal to each other. The second column[35] from the right in Table 2 shows the ratios of actual amplitudes to the theoretical minima, denoted by letter U. When the strike force is more or less constant, U changes inversely with tempo; it is the highest

Table 1

Pianist	Tempo (strikes/second)	W_0 (cm/s^2)	$W_0/2g$
#13 (Figure 17)	2.6	1,470	0.75
	5.3	5,700	2.90
	7.5	7,540	3.85
#12 (Figures 9 & 16)	2.2	1,020	0.52
	4.6	3,000	1.53
	6.0	4,490	2.29
	7.8	4,980	2.54
#10 (Figure 10)	(*p*) 6.3	5,150	2.63
	(*f*) 6.7	11,600	5.92

Table 2

Pianist	Tempo	Amplitude of actual acceleration, *W*	Theoretically minimal amplitude	$W/W_0 = U$	$W/2g$
#13	2.6	3,600	1,470	2.38	1.78
	5.3	8,950	5,700	1.57	4.56
	7.5	7,500	7,540	0.99	3.83
#12	2.2	4,920	1,020	4.80	2.51
	4.6	6,250	3,000	2.08	3.19
	6.0	6,400	4,490	1.43	3.26
	7.8	5,120	4,980	1.03	2.62
#10	(*p*) 6.3	11,300	5,150	2.20	5.76
	(*f*) 6.7	19,900	11,600	1.72	10.15

at slow tempi and approaches 1 at high tempi (the magnitude of $U = 0.99$ in the third line of the Table is certainly related to errors in measurement). Second, the last column of Table 2 shows that, even during the slowest tempi, during which weighted play is theoretically possible according to Table 1, it actually does not occur, and the amplitude of accelerations *W* is 1.5 to 2.5-fold higher than the ideal, weighted amplitude.

From the point of view of the theory of piano strike, we are particularly interested in value *U*. We have already noted that, during fast tempi, force curves lose their double peaks and approach in their shape pure sinusoids. Now, we can see that, during an increase in the tempo, the amplitude of accelerations approaches its theoretical minimum, and therefore, the movement itself approaches the mechanically simplest form of harmonic oscillation. Let us note that we have always underestimated the magnitude of *U* because of problems with taking into consideration the moment of inertia of the hand. Figure 19 shows graphically how changes in W_0 and W depend on the tempo.

Obviously, getting force moments from accelerations is straightforward; therefore, we will not discuss force amplitudes *F* which change perfectly in parallel to the aforementioned changes in amplitudes of accelerations. We should now analyze what the reasons are for the described changes with tempo and which motor mechanisms they can implicate.

Oscillatory motion of the hand does not represent a free oscillation: These oscillations are induced by forces whose origin is outside the hand, in muscles of the forearm and the upper arm. If motor impulses moving the hand originate from contractions of forearm muscles, hand movement can be very different depending on the patterns of the impulses. However, if the hand is moved by muscles that are located above the forearm, i.e., it gets an oscillatory motion through an interaction between forearm motion and the passive elastic state of forearm muscles, the hand can only demonstrate a type of motion known as forced elastic oscillations.

During free oscillation, maximal accelerations (and, therefore, maximal forces) coincide in time with maximal deviations from the equilibrium state. During forced oscillations, however, there is a phase shift between forces and displacements. Since force requires some time to be transmitted through an elastic link from the source of forces to a moving body, maximal forces in the elastic link, during forced oscillations, should occur earlier with respect to maximal body deviations which

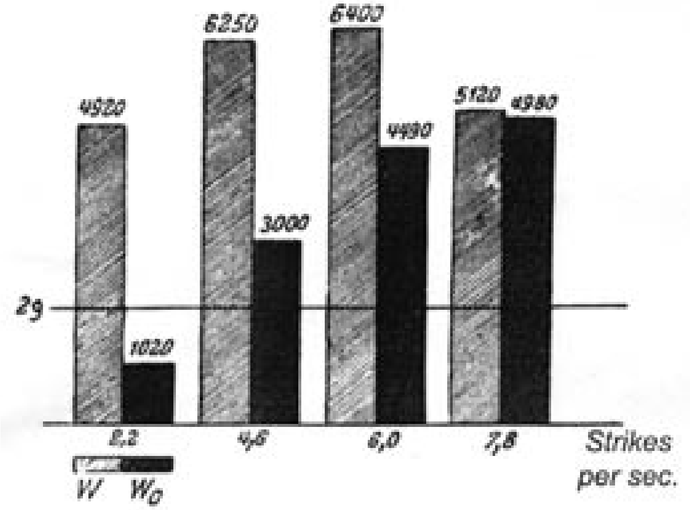


Figure 19 — Average values of the theoretically minimal amplitudes (W_0) and the actual amplitudes of the acceleration of the hand center of gravity at different tempi (corresponding to Table 2).

the forces try to dampen. Therefore, with respect to the studied case, if the hand is in a state of forced oscillations, maximal flexion forces in the wrist should precede maximal wrist extension (ordinate *A* in Figure 12). Alternatively, if hand motion is active, such a time shift should not be seen. Let us see what kind of information is provided by our data.

Table 3 shows that, at tempi lower than 7 strikes per second, there are relations corresponding to active hand motion. At tempi over 7 strikes per second, there is considerable evidence for the forced oscillation regime.

We can do an indirect test of this observation, using other, barely noticeable movements of the hand. First of all, let us consider the kinematics of the hand motion. If the hand is performing forced oscillations determined by movements of the forearm, the phase of the hand movement should be behind the phase of the forearm movement. Therefore, maximal wrist extension should happen after maximal elbow flexion and after the associated maximal elevation of the hand

Table 3

Pianist	Tempo	Delay (+) anticipation (–) with respect to maximal extension (in thousandths of a second)
#13	4.9	+ 8.3
	5.7	+ 10.8
	7.3	– 3.3
#12	2.2	+ 21.0
	4.6	+ 6.7
	7.8	– 1.7
#10	6.5	+ 11.6

Table 4

Pianist	Tempo	Delay (+) anticipation (–) of maximal hand elevation with respect to maximal wrist flexion (in thousandths of a second)
#13	4.9	+ 1.7
	5.7	+ 0.3
	7.3	– 5.0
#12	2.2	– 16.6
	4.6	+ 1.7
	7.8	– 3.3
#10	6.5	+ 1.7

Table 5

Pianist	Tempo	Elbow after wrist (+) or prior to wrist (–) (in thousandths of a second)	
#12	2.0	–	30.0
	4.3–5.0	+	6.7
	5.5–6.3	+	5.0
	7.5–8.0	–	15.0
#10	6.0–6.3	+	15.0
	6.7–7.0	–	6.7

center of gravity. Hence, one needs to compare the time of the highest position of the hand center of gravity with the time of maximal wrist extension.

The assumption of changes in the mechanism of the oscillations during fast tempi is also corroborated by Table 4. In this Table, during medium tempi, wrist extension precedes maximal elevation of the hand, while at the fastest tempi, it lags maximal elevation. The overall clear picture is smudged by the very slow tempi performed by pianist #12. This may be explained by the overall unclear pattern of movements during slow tempi as seen clearly in Figures 17 and 18.

Finally, we can perform a more direct test of our assumption. We can directly check in which cases maximal elbow flexion precedes maximal wrist extension. Time relations between these two events are illustrated in Table 5.

Table 5, once again, confirms the correctness of our assumption and, with even higher precision, reveals the crucial moment when one mechanism is replaced by the other. Up to approximately 6.5 strikes per second, we deal with active hand motion through contractions of forearm muscles. At higher tempi, the hand starts to demonstrate forced oscillations under the action of forearm movement. Now we can understand both the difference in the shapes of the movement curves during the medium and fast tempi, as well as the difference in the dynamics in the two cases.

When the hand performs forced oscillations under the action of elastic forces of the statically active muscles of the forearm, its motion, naturally, proceeds in conditions that are close to elastic oscillations in general. Hence, both the pattern of the movement and the shape of the force moment curves are close to a perfect sine wave; naturally, in such conditions, the amplitude of forces is close to the theoretically possible minimum. On the other hand, during active movements of the hand, impulses at the wrist must interact with impulses from elbow muscles producing a very complicated pattern of movements of a complex pendulum. This particular case of oscillations of a complex pendulum seems to us the most probable explanation for the double-peaked shape of force curves at the wrist and elbow during movements at medium tempi. If one adds motion of the shoulder joint to these already very complicated movements, the absolutely irregular pattern of shoulder moments will emerge, typical of these tempi.

Thus, changes in the strike construction and dynamics, in their dependence upon tempo, can be summarized as follows. During slow and medium tempi, both the hand and the forearm move under the action of their own active muscle impulses. At medium tempi, a sequence of such impulses merges into a single continuous chain, while during slow tempi, individual impulses leading to strikes are separated by more or less prolonged periods of inactivity. During tempi over about 6.5 strikes per second (390 per minute), hand motion transforms into forced elastic oscillations of a rather simple construction, with force amplitude close to the theoretical minimum.

Analysis of different tempi also allows us to draw one more conclusion which has practical importance and partially answers one of the questions posed in the first Chapter. Since the mechanism of fast piano playing movements differs so dramatically and deeply from the mechanism of slow movements, it becomes very clear that studying complex passages at slow tempi, or drilling difficult parts, is unjustified. Certainly,[36] it is not possible to design, for pedagogical purposes, a slow movement of such a construction that it would be innervationally close to a fast movement and, therefore, would facilitate learning of the fast movement. The presently available material does not allow us to identify such a construction. Similarly, slow demonstration does not make sense as well: The construction of a movement, independent of the strike force (as described earlier), changes completely when the tempo changes. Besides that, slow demonstration can be rejected for the following reasons. Wrist rhythms of 400 strikes per minute absolutely exclude any possibility of self-observation; therefore, a pedagogue who uses such a demonstration is not only unable to perform the movement that needs to be demonstrated but does not even know which movement he has to try to show. Such demonstrations can be realized only with the help of slowed film play, but even in this case they will have only a secondary didactic importance.

Summary

The paper describes an experimental analysis of the rhythmic octave strike of outstanding virtuosi pianists, performed by the authors in 1927–1928 with the help of the kymocyclographic method. Monotonic octave sequences in *crescendo-diminuendo* and in *accelerando-rallentando* were studied.

The following characteristics were studied in the photographic records: 1). Motion of the segments of the right arm (upper arm, forearm, and hand); 2). Joint angle changes; and 3). Moments of resultant muscle forces at the shoulder, elbow, and wrist joints.

The study allowed us to define a typical time pattern of a rhythmic octave strike, and to generate its precise description.

The available experimental material on the strike force shows that movement dynamics changes only quantitatively with the strike force, while its construction remains unchanged.

Experiments with tempo changes have shown that, depending on the tempo, the construction of movement changes significantly. During slow tempi, the movement consists of isolated impulses; at medium tempi, the movement corresponds to those of a complex pendulum; at the fastest tempi, it transforms into forced elastic oscillations, similar to those of a simple pendulum, while the wrist is kept passively elastic.

The experiments have shown that, during any of the studied movements at tempi over 3 strikes per second, falling of the arm under its own weight ("weighted" strike) definitely does not occur and cannot occur because of purely mechanical factors. During very slow tempi, such falling occurs sometimes, but much less frequently as could be expected theoretically.

The paper represents the first study of a series performed by the piano section of the SIMS and targeted at investigation of the piano strike: 1). During more complex rhythms; 2). In beginners and students; and 3). During comparison of pianists of different schools and individual styles.

Serendipity in Science: Gusto, a Woman, and the Terror

Historically, this is one of Bernstein's most fascinating papers coming to the attention of English readers. The paper is original and innovative, it is witty, sometimes hilarious, it is assertive and even belligerent, written with incredible precision but here and there not without the conceptual vagueness that renders the history of science so fascinating. Moreover, as if to counterbalance the rigor of its analysis, the paper is sloppy in its technical details, referring to the wrong Figure number, or forgetting to write s^2 instead of s. Maybe this is so because the galley proofs were not, or could not be corrected properly, but maybe it is also a sign of the authors' excitement at the time of writing their masterpiece (cf. Meijer & Feigenberg, 2000).

Readers of the "Classical Heritage" have sometimes cautioned against too much attention being paid to Bernstein lest his work be turned into some kind of Bible; at other times, the complaint is heard that the historical treatment of Bernstein is too frivolous. Naturally, these two points are often made by the same persons. So, Bernstein continues to titillate us, not because he was always right (he often was not), but he spent a lifetime in trying to understand the organization of movement, was well aware of the deep problems of old approaches, was never dogmatic in attempting to build a new framework virtually from scratch, and is simply infectious with his enthusiasm. The present paper offers a case in point.

Bernstein and Popova started their experiments in 1927, the year when Bechterev died, possibly killed by the Kremlin because he had known too much (Kozulin, 1984). In 1928, the experiments were finished, and, in the fall, Bernstein and Popova submitted their German paper (to Abderhalden), probably at the same time as the Russian version (to the State Institute of Musical Science). Simultaneously, attacks on the students of Bechterev and their mechanistic *reactology* were being planned in the Soviet Union. In 1929, Deborin, of the Communist Academy, celebrated the victory over mechanicism, accusing its adherents of "leftist perversions" (Kozulin, 1984, p. 20). In the 1930 Congress on Human Behavior, organized by Zalkind, an active party member, "mechanistic deviations" (o.c., p. 21) were denounced, it was declared that all science had to be based upon dialectical materialism, and leading scientists, such as Luria, ran publicly into trouble and had to retreat from their earlier views.

Were Bernstein and Popova, in 1928, aware of these developments? They may have been (Meijer, 2002), in that they were switching to a "dynamical" analysis, much more in agreement with then prevailing views on dialectical materialism than Bernstein's earlier work. Nevertheless, the paper itself is an attack (missing in the German edition) on virtually all piano education, a topic of little political relevance, but consistent with Bernstein's curriculum vitae, since after the Civil War he had attended lectures on mathematics and on musicology (Bongaardt, 1996).

The inducement to perform the study was in agreement with Braune and Fischer's by then classical mechanicism. Braune and Fischer (1895-1904) had criticized the view of the Weber brothers (cf. Flescher, 1997) that during walking the swinging leg exploited gravity and, thus, could be left more or less to itself. No, Braune and Fischer had retorted, cinematographic analysis revealed that the legs are under continuous control of the will. Bernstein and Popova engaged in a comparable enterprise, attacking the notion that the fingers arrive at the keyboard by weight alone, a topic alluded to in the introduction to their paper, then mentioned as something of "secondary . . . importance" (p. 9), and finally refuted for all but the slowest tempi. Still, the paper does much more than falsifying simplistic models of self-organization (if we may be forgiven to use this term outside its historical context).

In fact, during fast movements the hand muscles "seem to try to interfere less and less with the accelerating movement as if leaving it to its own" (p. 11), not because gravitation can organize the movement, but because the hands continue to oscillate. Compared to Bernstein's earlier, still mechanistic, papers, this latter hypothesis is an almost complete turnaround. For Bernstein, 1928-1930 were the years of great leaps into the unknown, exemplified by statements such as, in 1929, "there are no situations in which muscle shortening is the cause of a movement" (cf. Fel'dman & Meijer, 1999, p. 119), and, in 1930, "no movement can be entirely planned from its very beginning" (cf. Beek & Meijer, 1999, p. 5). Of Bernstein's translated papers, the present one, of which the German version appeared in 1928, is the first that contains revolutionary ideas—such as the emphasis on interactions between forces, or functional non-univocality—ideas that eventually would culminate in his now famous paper on coordination (Bernstein 1935/1967). Where did the impetus come from to leave the mainstream and head into uncharted territory?

We don't know.

In respect to Bernstein's efforts to counter Pavlov, 1928 appears to be somewhat too early, and the topic of the paper has little bearing on Pavlov's theories anyhow. Just correcting a prevalent mistake among piano educators, on the other hand, appears to be somewhat too local a motive for so grandiose a paper.

There is no doubt that Bernstein was an enthusiastic student of movement, wanting to understand it in order to know more of the brain. He had a habit of working with inspiring people—the previous year he had published on experimental psychology with Luria and Vygotsky (cf. Feigenberg, 1988). And he never hesitated to focus on the exceptional—such as the amazing intelligence of Pflüger's decapitated frogs (cf. Beek & Meijer, 1999). Bernstein's gusto, then, may have been a necessary requirement for the formulation of the first oscillator model of human movement in this paper on the piano strike. Still, gusto alone can hardly have been sufficient.

Most innovations in the paper derive from mathematics, with an emphasis on dynamics and the theory of oscillators. Much had happened and was happening in this field (e.g., Rayleigh, 1883; Poincaré, 1893; the works of Krylov and Lyapounov—cf. Grigorian, 1973a & b; Van der Pol, 1926; Liénard, 1928; Van der Pol & Van der Mark, 1928; Adrian & Buytendijk, 1931). Oscillators were the talk of the day among educated mathematicians. Bernstein was always inspired by mathematics, but not much of a mathematician himself, rather striving to cooperate with others (such as Gel'fand and Tsetlin in his later years; cf. Bongaardt & Meijer, 2000). Popova, however, was an educated mathematician. We know very little of her, apart from her lifelong admiration for Bernstein. After Bernstein's death, Popova turned her condominium into some kind of Bernstein museum (Feigenberg, personal communications). Bernstein, in his turn, referred to her work in many of his papers after 1928. Taken together, we think that the evidence is sufficient to conclude that Popova played a decisive role in the turn Bernstein was taking.

Much of the enthusiasm, so evident in the present paper, appears to derive from the sparkle, at least intellectual, between Bernstein and Popova. Together they come up with that puzzling notion of "forced elastic oscillations", "forced" clearly suggesting that the elbow tells the hand what to do, while "elastic" is sufficiently vague to leave space for interactions between the hand and the elbow, as if the hand tells the elbow how to tell the hand what to do. Maybe that is indeed what they meant, and maybe that is indeed how it works. And then, the dice were cast, and Bernstein found himself on a road which was bound to give him recognition. To us, it appears that that is how Popova had wanted it.

After 1928, events in Bernstein's biography mostly took care of themselves. He found himself promoting a dynamical approach to movement which happened to be in agreement with government prescriptions. Our present analysis suggests that he did not opt for that approach because of such prescriptions, however much of an enthusiastic dialectical materialist he may have been. Still, the prevailing official view may have added fuel to his 1935 (cf. 1935/1967) attack on Pavlov. The attack aborted because Pavlov was too famous to be touched, but in the years to follow, the years of terror, Bernstein's fame continued to build up, and it was not before 1950 that he himself ran into trouble with officialdom. Then, he did not give in. Once more, this is a fact suggesting that he was very much his own man, continuing on the road he had chosen in 1928.

If one looks carefully at scientific revolutions, serendipity is hard to miss. There always is a person, with colleagues, preceding scientific work, and a societal background. In Bernstein's scientific biography, all these factors played a role, in one way or another. The revolution he was to create rooted in his own enthusiasm, was pushed into the right direction because of a woman, was both a continuation and a falsification of the work of Braune and Fischer, and allowed him to ride the crest in the years when so many others would suffer from the terror of Stalinism. In 1928, Bernstein cannot have foreseen the consequences of his choice, but what came out of it was fun to him, at least initially. And we, the readers of this 1928 paper? We are left with that hilarious image of the piano student who studies tempo first before inserting the actual notes.

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Editorial Notes

[1] The paper appeared in 1930 in the: *Proceedings of the piano-methodological section of the State Institute of Music Science, Volume 1* (pp. 5-47). Moscow: Muzgiz. It was translated by Mark L. Latash, and edited for clarity. A paper largely similar to the present one appeared in German: Bernstein, N. & Popova, T. (1929). Untersuchung über die Biodynamik des Klavieranschlags [Study of the biodynamics of piano playing]. *Arbeitsphysiologie,* **1**, 396-432 (submitted 24 November, 1928). Footnotes will indicate where the two versions are different in relevant ways, and add some historical background. Footnotes about specific aspects of the translation were written by, or in close agreement with, the translator (M.L.L.). The original footnotes from Bernstein and Popova are also given.

[2] Notwithstanding the clear enthusiasm with which Bernstein and Popova announce further publications in this series, no such further publication is mentioned in Feigenberg's (1988) bibliography of Bernstein's works.

[3] The German text of this introductory paragraph (through "facing researchers of piano movements") is different. In the German introduction, the authors emphasize that piano playing is a special case of rhythmic movements, in particular pendular movements or rhythmically striking something. Given the general importance of such movements, the authors want to elaborate on their basic principles.

[4] In this and the following paragraphs (through "There exist already a small number of purely experimental studies"), Bernstein and Popova are strikingly belligerent, attacking more or less everybody who had published on the subject before. Interestingly, this whole part is missing in the German version.

[5] This, for contemporary readers, rather unusual profession of "engineer-psychotechnician" reveals how obvious it was in the early Soviet Union to want to understand the brain (the mind) through the study of movement.

[6] The rather colorful end of this paragraph is missing in the German version.

[7] This and the next sentence are missing in the German version.

[8] Sentence missing in the German version.

[9] Interestingly, this statement, which will contribute to Bernstein's revolution in movement science (cf. 1935/1967), is given here as an "undisputable fact". At least in his translated papers, there is no earlier mention of this fact which, apparently, everyone knows.

[10] This whole rejection of the use of qualitative evaluation (through "forces participating in the movements") is missing in the German version.

[11] Statement alluding, but not being quite identical, to non-univocality (cf. note 17).

[12] [M.L.L.:] Bernstein uses a term that in Russian is used specifically in music and means the shape of the hand prior to striking keys.

[13] The following terms are of relevance here:
ppp: "pianississimo", maximally soft
pp: "piano pianissimo", very soft
p: "piano", soft
mf: "mezzo forte", medium loud

f: "forte", loud
ff: "forte fortissimo", very loud
fff: "fortississimo", maximally loud.

[14] From here until "Any movement, in general" is missing in the German version.

[15] Bernstein and Popova are referring to the initial stages of the present study here and not to earlier work.

[16] Note that this is an overstatement, since it is the whole constellation of forces (moments) that creates the movement pattern, rather than only the inertial forces. Clearly, it is an important point for Bernstein and Popova to emphasize the role of inertia.

[17] This is the first non-univocality statement we are aware of. It took another 5 years (or 7, if one counts from the submission of the German paper) before this led to Bernstein's now famous statement on the nature of coordination (Bernstein, 1935/1967).

[18] Cf. Bernstein, 1935/1967, where this argument is elaborated.

[19] This appears to be the first time in Bernstein's work that the mechanics is subsumed under the dynamics.

[20] Paragraph missing in the German version.

[21] [B & P:] By T.S. Popova, A.S. Sheves, M.E. Feigin, V.V. Perovsky, and Dr. N.K. Vereschagin, under the direction of N.A. Bernstein.

[22] [B & P:] Since 1928–1929, we have worked with bulbs produced in Germany whose diameter is 2 mm.

[23] The German has *gc*, which is in agreement with the chapter on Results and the Figures of the present text.

[24] This whole part about the camera (until the paragraph that starts with "The recording was performed") is missing in the German version. Bernstein had just published two papers on kymocyclography in German (Bernstein, 1927 & 1928/1936). The present text (missing in Bernstein & Popova, 1930) is very close to that in Bernstein, 1928/1936, from which we took the Figures 4–6, because their quality is better than in the Russian edition of the present paper.

[25] Note that this is the abbreviation for the German "Schüssel" (key), while *Sg, later* in the text, is from the German "Schneckengang" (helix). Apparently, Bernstein used the illustrations he had prepared for his German publications.

[26] Figures 7 and 8 can be inspected here. They are given by Bernstein and Popova without a reference within the text.

[27] [B & P:] *Handbuch der biologischen Arbeitsmethoden,* herausgegeben von E. Abderhalden. [Editors: We have used a 1936 edition.]

[28] *Handbuch der biologischen Arbeitsmethoden* (Bernstein, 1928/1936).

[29] This should be Figure 10.

[30] Note that here and in the following Bernstein and Popova often refer to "pendular movements" or "oscillations". It is impossible to decide at which places the authors have in mind, or not, the whole literature (new at their time) on oscillations, such as Van der Pol's (1926) work on relaxation oscillations.

[31] Amusingly, the German has "a German *virtuoso*".

[32] Whereas the German version usually has "Schwingungen" for oscillations, the shoulder here is supposed to reveal "Schwankungen" (irregular movements). The Russian text, however, has no such distinction.

[33] According to the German version, this should be Figure 18. This discrepancy can be found again later in the present paragraph.

[34] In the Russian text (but not in the German), the squares after 's' are missing here, and in the following. We have corrected this.

[35] The Russian text (but not the German) refers to "lines" instead of columns. We have corrected this.

[36] This and the following sentence are missing in the German version.

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