COMMAND: Certifiable Open Measurable Mandates

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Abstract

Security mandates today are often in the form of checklists and are generally inflexible and slow to adapt to changing threats. This paper introduces an alternate approach called open mandates, which mandate that vendors must dedicate some amount of resources (e.g. system speed, energy, design cost, etc.) towards security but unlike checklist security does not prescribe specific controls that must be implemented. The goal of open mandates is to provide flexibility to vendors in implementing security controls that they see fit while requiring all vendors to commit to a certain level of security. In this paper, we first demonstrate the usefulness of open security mandates: for instance, we show that mandating 10% of resources towards security reduces defenders losses by 8% and forestalls attackers by 10%. We then show how open mandates can be implemented in practice. Specifically, we solve the problem of identifying a system’s overhead due to security, a key problem towards making such an open mandate enforceable in practice. As examples we demonstrate our open mandate system—COMMAND—for two contemporary software hardening techniques and show that our methodology can predict security overheads to a very high degree of accuracy (< 1% mean and median error) with low resource requirements. We also present experiments that quantify, in terms of dollars, how much end users value the performance lost to security, which help determine the costs of such a program. Taken together—the usefulness of mandates, their enforceability, and their quantifiable costs—make the case for an alternate resource-based mandate.

1 Introduction

Attempts to mandate or regulate security have typically resulted in prescriptive lists of do’s and don’ts and are referred to, often disparagingly, as “checklist security” [5]. Such approaches can lead to meaningful security improvements but they are often mired by the unavoidable pitfalls: Checklist security is slow to adapt to dynamic and changing threats, it lulls vendors and users into a false sense of security, and adherence becomes more about compliance and bureaucratic due diligence rather than actually improving security [17]. However, without mandates, longstanding security issues may continue to go unresolved [14], since marketplace failures like risk externalization and information asymmetry discourage vendors from adequately investing in security [4]. This means that there is an opportunity for more effective mandates than checklist security.

We propose an alternate approach: Rather than telling product vendors what security features must be put in their products, we propose to leave that decision up to the vendors themselves, and instead enforce that all vendors allocate a percentage of resources towards security. After all, no one knows a product like its creator, and vendors are naturally the ones who would know where to best invest resources to improve their products’ security if nudged to do so. This is essentially the opposite of checklist security, and hence we name such a mandate an open mandate.

Specifically, in our open mandates proposal, product vendors would be required to design their products such that a mandated percentage of development, maintenance and runtime costs are dedicated towards security. Some types of resources—like time and money spent on secure design development or developer training—can be measured through standard accounting means, but measuring other types of systems resources—like CPU cycles or energy—requires novel technical solutions. In this paper, we introduce such a technique—COMMAND—which enables real-time, on-device, and highly accurate estimations of runtime overhead due to security. This makes an open mandate measurable and thus enforceable.

The benefits of such an open mandate extend beyond simply giving vendors the flexibility to apply security as they see fit. By requiring all vendors to play by same rules and incur the same costs, an open mandate removes the incentive for vendors to skimp on security in favor of performance. This eliminates the “first-mover” disadvantage, where solving a security issue at the expense of system performance makes a
Our approach also provides insight and a methodology for how regulators might determine mandated security investment levels.

We then demonstrate a practical implementation of a performance-based open mandate. Our solution accounts for workload-, input-, and system-dependent overheads. For instance, a user running a web browser with a certain memory safety solution may need to account for overheads differently than a user running a mail client with the same memory safety technique. Similarly, a user loading static webpage will incur very different overheads from a user loading webpages with lots of JavaScript, even though both users are technically running the same program. Furthermore, a user running a demanding program on a high-end gaming laptop may need to account for overheads differently that a user running lightweight programs on a low-provisioned laptop. Finally, the solutions need to be extremely low overhead to ensure that the cost of measuring security does not consume resources better spent on security itself.

To satisfy these requirements, we designed COMMAND, a system for Certifiable Open Measurable Mandates (Figure 1) COMMAND uses a Deep Neural Network (DNN)-based regression model to predict on-device, in situ, real-time performance overheads based on current system information obtained from Hardware Performance Counters (HPCs). By training this model offline, vendors can provide end users with pre-trained models that allow user devices to produce high-accuracy estimates of security overhead without ever require user device benchmarking, i.e. without ever asking users to compute overheads by enabling and disabling security features. Our models predict security overhead with high accuracy (< 1% mean and median error) for two contemporary defenses: binary hardening and a runtime memory safety technique.

The organization of this paper is as follows: Monte Carlo security games and takeaways are presented in Section 2 to motivate the work. Section 3 then outlines the requirements of a performance-based open mandate. We present the design of our COMMAND approach in Section 4 and evaluate its
performance in Section 5. We then quantify the value of performance in terms of dollars in Section 7 to help answer the question of how much users should be compensated for participation in such a program. In Section 6, we provide commentary on important considerations of such an open mandate. Related work is treated in Section 8, and conclusions are given in Section 9.

2 Are Mandates Useful?

In this section we establish the motivation for conducting this work. We first wish to answer the question: Are mandates useful? To answer this question, we used Monte Carlo simulations to determine the economic conditions under which a mandate would be beneficial. Here we define useful as 1) decreasing the amount of assets that good actors lose to malicious actors and 2) lengthening the amount of time necessary for malicious actors to steal the assets of good actors. Our simulations model the following scenario:

Setup and Initialization

The following information is used to establish the initial conditions of each scenario for our model:

• Players are split into a group of attackers and a group of defenders such that there are more defenders than attackers.
• Defenders are initialized with an approximately lognormal distribution of assets drawn from global data generated by Credit Suisse [7] along with a cost to attack, which can be interpreted as a threshold (in terms of assets) that an attacker must pay to take a portion of a defender’s assets.
• Attackers are assumed to be on average less wealthy than defenders: Assets of attackers are determined by scaling the defenders’ lognormal distribution of assets by a percentage, which we label inequality.
• Attackers have perfect information about defender assets, but defenders do not have perfect information about attackers. This models an information asymmetry.
• Our model contains 6 parameters that are shown in Table 1. Each parameter can be thought of as a percentage, with real values [0, 1].

Next, we describe the rules in order to carry out a simulation.

Rules

For each iteration of the simulation, the following steps are performed:

1. Each member $d$ of the defenders $D$ is assigned its assets $d.\text{assets}$, and its cost to attack $d.\text{cost}$.
2. Each member $a$ of the attackers $A$ is assigned a number representing its assets $a.\text{assets}$ and its probability of attacking successfully $p_{\text{success}}$.
3. Each attacker $a$ is paired with a defender $d$.
4. Each $a$ “fights” the $d$ it has been paired with in the following way: The attacker computes its expected earnings given the $d$ it has been paired with and $p_{\text{success}}$.
5. Each $a$ then checks whether $a.\text{expected\_earnings} < d.\text{cost}$ and if $d.\text{cost} < a.\text{assets}$. If these conditions are met, $a$ performs an attack on $d$. $a$ wins with probability $p_{\text{success}}$.
6. Attackers are punished via a loss of assets if their attack fails, and if the attack fails $d.\text{cost}$ is subtracted from $a.\text{assets}$.
7. Any defender $d$ s.t. $d.\text{assets} \leq 0$ is then removed from the game following the fight, before the next iteration of the game takes place, and the next iteration carries out starting at step 1 above.

We obtained our value for $p_{\text{success}}$ from a report analyzing ransomware incidents across a variety of different economic sectors. We see that on average, 39% of ransomware attacks are successful with a standard deviation of 6.2% across the included sectors [1]. We use these values to create a normal distribution and select a value from this distribution when an attack is carried out during each iteration. An attackers’ expected earnings are computed using the following equation:

$$a.\text{expected\_earnings} = d.\text{assets} \times \text{PAYOFF} \times p_{\text{success}}$$

Termination

Simulations terminate under three possible conditions:

1. $\forall d \in D, d.\text{assets} \leq 0$; All defenders lose all assets.
2. The simulation converges; where given two successive iterations of the simulation $i$ and $i + 1$, “convergence” is defined as meeting the following condition for $\bar{\delta}$ iterations:

$$\sum_{i+1} \delta.\text{assets} - \sum_{i} \delta.\text{assets} \leq \varepsilon$$

3. A set maximum number of iterations is reached; although we ran our simulations with a maximum of 10,000 iterations, we never encountered this termination condition in our simulations.

In our simulations, $\varepsilon = \$100$, which is very small relative to the amounts for assets in our simulations, and $\bar{\delta} = 50$ iterations, which we observed to be a significant portion of a typical simulation’s length.
Figure 2: A CDF showing losses vs. percent of iterations for 10 different security mandate percentages. Losses are defined as the amount spent on the security mandate plus the amount lost due to attackers. As the mandate increases, so does the initial losses for Defenders (i.e. at the 10% mandate, all participants lost at least 10% of their initial assets). However, an increase in mandate also raises the percentage of Defenders who lose no additional assets beyond the mandatory security investment. For example, we can see that without a mandate, roughly 65% of simulations have no loss, whereas with the 10% mandate, roughly 73% of simulations have no loss, implying that 8% of parameter sets are covered by the 10% mandate.

| Parameter | Definition |
|-----------|------------|
| ATTACKERS | Number of attackers compared to defenders, as a percentage |
| PAYOFF    | Max percentage of defender assets that can be taken in an attack |
| INVESTMENT| Percentage of defender assets that are spent on security measures |
| EFFECTIVENESS | Percentage of INVESTMENT by which the cost to attack a defender increases |
| INEQUALITY | Fraction by which defender wealth distribution is scaled to create attacker wealth distribution |
| SUCCESS   | Initial percentage of defender assets that count toward the cost to attack a defender |

Table 1: Parameters of our Monte-Carlo model

2.1 Mandates Can Be Useful

We are interested in understanding the usefulness of mandates and the impact of ATTACKERS, PAYOFF, INVESTMENT, EFFECTIVENESS, INEQUALITY, and SUCCESS (shown in 1). To study this impact, we performed simulations of all possible configurations while varying each of these parameters through their full range. We present the data as a cumulative distribution function of loss encountered by defenders for different mandated spending levels from 0 to 100%, as shown in Figure 2. In the baseline case of having no mandate, we observed that roughly 65% of the possible scenarios in our model incurred no losses. This in turn implies that it would be useful to implement a mandate in 35% of the simulations that our model can run with the parameters that we define in Table 1, given that no open mandate exists today.

This 35% of states consisted of two possible outcomes: first, scenarios in which attackers were able to plunder large portions, but less than 100%, of defenders’ assets (Figure 3a) and second, scenarios in which the attackers won outright, taking all assets from defenders (Figure 3b). The first outcome occurred with approximately 99% frequency, while the second outcome covered the remaining 1% of scenarios. In the case that defenders lost some but not all of their assets, defenders lost less of their assets over a longer period of time with the introduction of a mandate, and in the case of total asset loss, defenders lost all of their assets over a longer time-frame. Both of these results suggest that a mandate would provide more time to stem the flow of assets to attackers and actively intervene in the situation over time. An increasing mandate eventually reduced the number of instances of total loss outcomes to zero as the mandate reached around 40% of assets. We argue that given the state of our world today, we exist in one of these 35% of scenarios where a mandate would be useful to implement, and in 100% of these scenarios, a mandate provided meaningful benefits to defenders.
Figure 3: Examining the two outcomes of our simulations: **Outcome 1)** With no mandate, this outcome occurs 99% of the time. The simulation reaches our definition of an equilibrium ($\epsilon = 50$ iterations with $< \delta = $100 in total assets being exchanged from attackers to defenders), in which case we examine the rates at which defenders lose assets over time. In this scenario we observe that an increased mandate investment decreases the rate at which defenders lose assets and **Outcome 2)** Defenders lose all assets, which accounts for around 1% of simulation outcomes with no mandated security investment, but decreases to zero with an increase in the mandate. We see in this outcome that not only do the number of simulations in which attackers win drastically decrease with an increased mandate, but the mean of the distributions shifts to right, indicating a longer amount of time until the attackers win.

Further, our results show (Figure 2) that upon imposing a 10% mandate, 8% more of the scenarios in our model experience only the losses of the mandate and do not have their assets plundered by attackers. This trend then continues with diminishing returns as we increase the percentage of assets required to be invested in security, with a 20% mandate adding another 7% of scenarios, a 30% mandate adding another 6%, and so forth.

Beyond the most desirable outcome for an individual defender, losing no assets, we would additionally like to establish whether or not mandates can stem cumulative losses over time. In this respect we classified the negative outcomes of our model into two categories: 1) simulations under which the parameters led to the complete loss of all defender assets, and 2) simulations under which the parameters led to an “equilibrium” in which after the protracted loss of assets by many defenders, the losses eventually halted asymptotically over time.

In our simulations, the first outcome occurred in 1% of suboptimal scenarios (Figure 3b), while the vast majority were associated with the second. Even in the worst case of complete loss of defender assets, we see that a mandate increased the amount of time before this happened, and in the slightly better case that only some defenders lost assets, we can see that the rate at which defenders lost assets reliably decreased with an increased mandate for investment, which we can see displayed in Figure 3a.

### 2.2 When Are Mandates Useful?

In the previous section, we showed that mandates are useful in 35% of simulations. In this section, we examine the parameters for this subset of simulations to understand the influence these parameters have on the success of mandates.

The histogram depicted in Figure 4 displays the distribution of parameter values for simulations where mandates have a positive effect. We synonymously refer to these simulations as *useful*. From them we can make several observations regarding the model parameters:

- Attackers and their relative inequality have little (if any) effect on the benefit from a mandate. The simulations are distributed approximately equally throughout the possible values for these model parameters—in fact, so evenly that in Figure 4 the two lines for the ATTACKERS and INEQUALITY parameters are indistinguishable at the scale that we present the figure.
- Attackers are favored heavily by lower values of SUCCESS as well as lower INVESTMENTS among parameter sets that are receptive to the introduction of a mandate. This makes sense, as this implies that the attackers have their
Figure 4: Histograms of Monte-Carlo simulation parameters in useful parameter sets

Table 2: Expected values of parameters in useful Monte Carlo simulations. These values represent a default configuration from which we investigated the boundaries of the subset of the parameter space that a mandate would be useful to apply to. The distance that the parameter is from 0.5 gives a relative measure of how strongly biased the parameter is toward favoring attackers or defenders.

| Parameter     | Expected Value |
|---------------|----------------|
| ATTACKERS     | 0.5            |
| PAYOFF        | 0.8            |
| INVESTMENT    | 0.2            |
| EFFECTIVENESS | 0.5            |
| INEQUALITY    | 0.5            |
| SUCCESS       | 0.3            |

2.3 Summary

Given our Monte-Carlo style model for an attack-defend scenario based on the real-world statistics for ransomware, we have demonstrated the tangible effects of a mandatory security investment. In addition to the clear net benefits for defenders in terms of relative losses over time, the model emphasizes that it is the defender’s job to allocate the security investment as wisely as possible (EFFECTIVENESS) as well as consider...
its security policies related to their assets as a whole (SUCCESS). We have shown how the model indicates that just over one third of simulations can benefit from a mandate and that it is very likely that we currently reside in this subset of the simulation space, as evidenced by losses due to cyberattacks. We have observed how the different parameters of the model were constructed and how they interact with each other in terms of establishing improvement via a mandated security investment.

3 Measuring In Situ Performance Overheads

A mandate is only useful if it is enforceable. In the case of a security-performance mandate, this means the mandate must be measurable: In other words, there must be a way to verify how much resources a system is dedicating towards security. Although this may seem like a simple task, this is a much harder problem than it may appear to be.

To illustrate the difficulties of measuring per-system security overheads, consider the obvious approach: Enumerate all the security features installed, find the reported average overheads, and count these overheads towards the desired security mandate level. For example, one might install three security defenses on a system, each with an overhead of say 4%, and then compute the system’s security effort to be $1.04 \times 1.04 \times 1.04 = 1.1249$, or a 12.5% security overhead. While simple and attractive, this “sum-of-its-parts” approach is too naïve and inaccurate to be the basis of a security mandate, for three reasons:

Workload Dependent—Runtime security overheads are highly sensitive to the types of applications being run. Even security defenses benchmarked on the same system can exhibit wildly different overheads depending on the exact benchmark or workload: For example, a recent runtime memory safety defense, which reports a geometric mean runtime overhead of 8%, demonstrates a 62% overhead on the gcc SPEC 2017 benchmark and a 0% overhead for others [28]. For some users and some systems, the use of such a defense may cause only negligible slowdowns whereas other users (perhaps programmers who rely heavily on gcc) will be heavily burdened. Therefore, a performance-based mandate cannot be evenly and fairly applied if it relies on reported average overheads. Thus an open mandate on performance requires continuous measurement to account for workload-dependent performance overheads.

System Dependent—Precomputed, static overheads fail to account for the wide variations between systems’ hardware builds, configurations, and operating systems. As an example, consider a security defense like Merkle Trees, which can provide full memory integrity but require a large memory footprint, and come with reported runtime overheads of around 20% when optimized without using newer integrity protection structures that rely on counter-mode encryption like Bonsai Merkle Trees [13], [23]. On a system with limited physical memory, this might produce serious runtime overheads if the system runs out of memory and starts thrashing. However, if the system owner installs more physical memory, this problem may resolve itself and the runtime overheads may once again become tolerable. In either case, is the reported 20% runtime overhead an appropriate estimate of the actual overhead endured by the system? Likely not. Performance overheads are largely a function of the underlying system, and do not easily translate from one system to the next. Furthermore, testing all security features on all possible systems is not feasible with
Thus an open mandate on performance requires on device overhead measurement to account for the infinite number of variations between systems.

Non-Linear Combinations of Overheads—It is highly likely that the overheads of various security features combine in non-linear ways, e.g. three separate defenses with a 4% overhead might have a combined overhead of 6% or even 20% instead of the expected $1.04 \times 1.04 \times 1.04 = 12.5\%$. This is due to the complex dynamics of systems, where an overhead in one part of a CPU pipeline or memory hierarchy might hide or even exacerbate overheads in other parts of the system. For example, consider the security feature of full off-chip memory encryption, which has reported overheads of X%. Encrypting and decrypting a full page of DRAM on every LLC miss places significant strain on the memory hierarchy between chip and DRAM. Now consider an additional security feature, perhaps such as, which adds delays to the processor pipeline and has a reported runtime overhead of Y%. If the memory encryption places a bottleneck at main memory, the additional pipeline delays may have no effect on overall runtimes. In such a case, it is inappropriate and inaccurate to report and overall security overhead of $(1 + X) \times (1 + Y)\%$. Furthermore, given the many different security features available for system designers to choose from and the many more yet to be proposed, it is unrealistic to precompute the actual expected overheads for all combinations of security features. Thus an open mandate on performance requires in situ measurements to account for the non-linear and unexpected ways that security overheads can add up.

4 Design of COMMAND

As demonstrated above, pre-computed static overheads are an insufficient approach towards measuring per-device security overheads, and motivate the need for on device, in situ, and continuous measurement. We also add that any measurement for a security mandate should itself be very low overhead, so that the cost of measuring security overheads does not impinge on performance that would be better spent on actually improving security. Finally, a security mandate must be measurable without without ever needing to disable security features. (This last point rules out the naïve “run-both-and-compare” strategy, or running user programs with and without security features enabled, since this would actually increase insecurity, not to mention the 2x overhead!)

This leaves us with five constraints for a security overhead measurement mechanism. We find that we can satisfy all constraints by using Hardware Performance Counters (user-accessible registers that track hardware events like CPU pipeline stalls and cache misses) to predict a program’s real-time overhead due to security. We achieve this by creating a model of security overhead that inputs HPC data and outputs an estimate of current security overhead. We find that Deep Neural Networks (DNNs) are a suitable modeling technique that can estimate performance overheads with a high degree of precision and accuracy. Our model is relatively small (12KB) and fast (only four layers), meaning it can provide real-time, low-cost, per-device, per-usage, in situ estimations of security overheads. By solving these challenges, our DNN-based security overhead model makes a performance-based security mandate measurable and actionable. The remainder of this section demonstrates how to train such a model and provides results on our models’ precision and accuracy.

4.1 Creating the Training Set

Training DNNs typically requires a large amount of training data. Before describing DNN training and results, we outline the process of creating our dataset(s) used for DNN training.

Since our desired model receives HPC data over some time interval and outputs and estimated security overhead, our training data must be in the same format: It must consist of samples of real-world program execution that contain HPC event data alongside with the security overhead during the interval of time which the HPC data was collected. Determining how long this time interval should be in consequential—too long of a time interval sacrifices your ability to make fine-grained and real-time estimations of overhead, but too short of a time interval burdens the system with frequent HPC data collection and model estimation. We find the boundary of system calls to be a suitable interval for collecting HPC data.

We build our dataset in five steps, outlined below:

**Step 1: Create Baseline and Secured Program Variants**—The first step in creating our training dataset is to make baseline and secured variants of a set of exemplar programs. We chose to use SPEC 2017 benchmark programs as our exemplar programs. Two security defenses in particular were considered: 1) Binary hardening flags (specifically `-fPIE -pie -D_FORTIFY_SOURCE=2 -fstack-protector-all -fsanitize=stack -Wl,-z,relro,-z,now`)—which incur a small performance overheads), and 2) a recent runtime memory safety technique—which can incur a negligible to large performance overhead [28]. Both happen to be compiler techniques, but our approach is not limited to compiler-based defenses.

**Step 2: Raw Data Collection**—With baseline and secured variants created, we can start the process of collecting HPC data and, in particular, determining the runtime overhead caused by an applied security defense. Note that this is not as simple as measuring full program runtime with and without a defense, since this would provide only a very coarse-grained

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3 We add that Ordinary Least Square regression—while not discussed in this paper—was attempted, and provided poor results.
picture of security overhead, and we desire to find security overheads at the granularity of system calls.

We use two open source tools to acquire HPC data (from which overheads are derived) during program execution. The first tool, easyperf, allows us to programatically query a system’s HPCs, while the second tool, DynamoRIO, allows us to intercept program execution at system call boundaries and write HPC data obtained from easyperf to a log file. For reasons discussed in Step 3, each log entry also contains the number of the system call that caused DynamoRIO to trap and execute our custom logging callback routine. Programs were run three times and log files averaged together to minimize the effects of system variation between data collection runs. Additionally, since x86-64 systems like the one we used for data collection only allow for five HPC events to be collected at a time, we repeated this whole process $20 \times 4$ times to collect all 20 HPC events of interest. This process is also repeated for both the baseline and secured variants of each exemplar program.

After running each variant of each exemplar program $4 \times 3 = 12$ times, we combine all 12 log files together in a single log file for both variants of each program. The resulting log file contains a series of ordered tuples of HPC event counts since the last system call along with the number of the system call itself. For convenience purposes, we define this period of time between system calls to be an epoch (not to be confused with a training epoch used in machine learning). In the remainder of this section, the term epoch may also refers to the data collected during the time period between system calls as well.

**Step 3: Syscall Alignment**—If the count and numbers of system calls were perfectly aligned between baseline and secured variants, computing per-syscall security overheads would be trivial. However, many compiler-based security defenses—including the ones we used for this work—will change the count, order, and numbers of system calls a program uses. This presents a challenge if we want to compare syscall-granularity epochs between baseline and secure variants of a given program: How do we know which epochs in the baseline program correlate with the epochs in the secured variant if the syscall numbers do not match?

Fortunately, this problem resolves to the well-studied problem of sequence alignment. Given two program traces (one baseline and one secured), which may have different (but still similar) sequences of system calls, we can use the classic Needleman-Wunsch algorithm to find the “best fit” between the two sequences, subject to some scoring function. We modify the scoring function so that only sequence insertions and deletions are considered and mutations (or mismatches) are ignored.

The Needleman-Wunsch algorithm runs in $O(nm)$ time and space complexity, allowing us to align sequences of thousands of system calls in about ten minutes on a moderately provisioned desktop computer².

**Step 4: Data Pruning**—Sequence alignment gives us a chance to line up two program executions—baseline and secured—and make a best effort attempt at identifying which sequences of execution appear in both variants. By identifying the common sequences in both variants, we give ourselves the ability to determine the security overhead a program is experiencing during each epoch. As for the “uncommon” (i.e. unique) segments of baseline or secured variant execution, there is no corresponding execution to compare to and thus no way of computing the security overhead in the secured variant, and so these epochs are pruned from the training set.

We also prune our training set of “singly-aligned” system calls. These are system calls that have been aligned in the previous step but are not included in a longer cluster of aligned system calls, raising doubts as to whether or not the paired system calls in the baseline and secured variants actually correspond to one another. For example, consider a segment of system calls with numbers 1, 6, 74, 4, 3 in the baseline and 15, 14, 14, 74, 16 in the secured variant. The two executions are highly dissimilar and likely have no code execution in common yet both contain the syscall 74. Naïve sequence alignment might align the sequences at syscall 74, but it is unlikely that this alignment corresponds to equivalent execution in both variants. Since there is no additional alignment in the neighborhood of syscall 74, we consider these to be “singly-aligned” and prune them from the training set.

In practice we find that only a moderate number of system calls are pruned from baseline or secured variants, indicating that system call sequences are fairly robust to the changes induced by applying security features like binary hardening or the runtime memory safety technique. On average, about 90% of system call sequences are aligned between baseline and secured variants, and only about 10% of epochs are pruned at this step. One outlier is the *xz_r* SPEC 2017 benchmark, in which the runtime memory safety defense lost over 40% of its epochs after alignment and pruning (this was caused because applying the defense happened to introduce a large number of *mmap* system calls which were not found in the baseline variant).

**Step 5: Data Transformation**—After pruning, the number of epochs in the baseline and secured variants should be equivalent and the remaining sequences of system call numbers should be identical. Each epoch in a baseline execution has a corresponding epoch in the secured execution, and vice versa. We can now begin transforming our data into a training set usable by a DNN.

First, we transform each HPC event collected into a proxy of performance. We do this by taking the count of each performance-related system event collected during an epoch.

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²One program trace containing 60,000 system calls exceeded our system’s available 16GB of physical memory and could not be aligned and was removed from the training set. This had no consequence beyond diminishing our training set by one benchmark.
and dividing by the number of instructions executed in the same epoch. Normalizing HPC data by the number of instructions in an epoch allows each HPC event count to be a standalone measure of performance (e.g. “pipeline stalls per instruction” instead of the less informative “pipeline stalls”). This set of performance-related HPC events normalized by an epoch’s instruction count becomes the input data for our DNN.

The output data (or target) for DNN training is the overhead experienced during the secured variant’s execution at the granularity of system calls. To measure an epoch’s security overhead, we compute the percent increase (or sometimes decrease) in the cycles between baseline and secured variants.

The last transformation we apply to our training set is the removal of outlier data. We find that some epochs report gigantic overheads (likely erroneously) and find that removing such epochs from the training set only improve our models’ performance. Our training data is now complete. The above steps yield roughly 20,000 epochs for DNN training.

4.1.1 DNN Architecture

We use a standard feedforward-style network architecture for regression. For both binary hardening and the runtime memory safety defense we use the same network configuration. Our network accepts a 20-dimensional input (since we use 20 HPC events as inputs) and uses four layers for an overall configuration of $20 \rightarrow 32 \rightarrow 64 \rightarrow 32 \rightarrow 1$, where the output is a prediction of current security overhead. Each layer uses Leaky ReLUs for activation functions. We chose to use half-precision (16-bit) floats for model weights to reduce the model size (we aim to minimize model size to highlight the feasibility of building such performance-monitoring models in hardware). Total model size is 12KB. Our models were built using the PyTorch framework.

4.1.2 DNN Training

We used an 90-5-5 split for training, testing, and validation sets, respectively. We used the Adam optimizer with a learning rate of $10^{-3}$ and an epsilon of $10^{-4}$. Without the epsilon parameter, our half-precision weights would quickly converge to 0 and leave model weights as NaN. We used Mean Squared Error (MSE) as our loss function. Training occurred over 100 training epochs (not to be confused with the syscall-granularity epochs defined earlier) with a batch size of 32. Training for both the binary hardening and the runtime memory safety defense’s model finished in about 10 minutes on an Nvidia GTX-1050 Ti GPU\textsuperscript{3}. To avoid overfitting, we choose the model that minimizes the validation set’s loss.

\textsuperscript{3}We observed that using half-precision training weights actually increased training time significantly.

5 Evaluation

Given a input of HPC data, our models can predict the actual observed overhead with a high degree of precision and accuracy. We now present characterizations of model performance.

5.1 Binary Hardening

![Histogram of absolute errors](image1.png)

![Histogram of relative errors](image2.png)

(a) Absolute errors

(b) Relative errors

The most obvious metric for evaluating model performance is the absolute difference between predicted overhead and actual observed overhead (i.e. $\text{actual} - \text{predicted}$) which we call absolute error (i.e. a predicted overhead of 25% vs. an actual overhead of 22% would be an absolute error of $-3\%$). A histogram of the model’s absolute error when predicting the overhead of binary hardening compiler flags can be found in
Figure 6a. The mean of the distribution is $-0.941\%$, the median is $0.085\%$, and the standard deviation is $6.658. 95\%$ of predictions are have an absolute error between $-13.971\%$ and $6.944\%$. The smallest absolute error was $-92.584\%$ while the largest was $28.519\%$.

Absolute errors are only one way to evaluate the model’s performance, and can sometimes be misleading. For example, consider two predictions: 1) An estimated overhead of $20\%$ vs. an actual overhead of $10\%$, and 2) an estimated overhead of $310\%$ vs. an actual overhead of $300\%$. Both predictions have an absolute error of $10\%$, yet it is clear that the first prediction is worse than the second since the prediction is twice the actual value (compared to 310 being only a 3.33% increase over 300). For this reason, we also consider a model’s relative error, or a prediction’s error relative to the actual overhead (defined as $(\text{actual}/\text{predictions}) - 1)$ The relative error of our binary hardening performance model can be found in Figure 6b. The mean of the distribution is $4.152\%$, the median is $0.012\%$, and the standard deviation is $99.922. 95\%$ of predictions are have a relative error between $-11.071\%$ and $16.517\%$. We observe some very rare but very large relative errors (up to 3069.000%) when the predicted value is very close to 0 but remark that in such situations, absolute error should be used instead.

Like absolute error, these results suggests that our modeling is highly effective at estimating the overhead a program is experiencing due to a security feature.

5.2 Runtime Memory-Safety Technique

We now repeat the above process for our model of the runtime memory safety defense performance overhead. A histogram of the runtime memory safety defense model’s absolute error can be found in Figure 7a. The mean of the distribution is $0.955\%$, the median is $0.172\%$, and the standard deviation is $24.881. 95\%$ of predictions are have an absolute error between $-29.088\%$ and $31.522\%$. The smallest absolute error was $-224.609\%$ while the largest was $394.727\%$.

Likewise, a histogram of the runtime memory safety defense model’s relative error can be found in Figure 7b. The mean of the distribution is $0.350\%$, the median is $0.004\%$, and the standard deviation is $30.593. 95\%$ of predictions are have a relative error between $-8.391\%$ and $10.623\%$. As with the binary hardening, we observe rare but large relative errors when predicted overhead is close to 0, but again suggest that absolute (not relative) error should be used in such situations instead.

Like with the binary hardening model, our trained model provides high-quality estimates of program overhead due to security. Both models illustrate the feasibility of continuous, in situ, and on-device measuring of performance overheads due to security, and furthermore illustrate the feasibility of a performance-based security mandate.

6 Deployment Considerations

We now discuss a few considerations that must be made when implementing such a performance-based open mandate.

Opt-in vs. Opt-out — A reasonable objection to any online security-performance mandate is that it is an overreach of government to dictate how users should dedicate the CPU cycles of their devices. Indeed, in any free and open society, the role of government must necessarily be limited in its ability to mandate or coerce its citizens. For this reason, it is desirable that participation in a performance-for-security program be “opt-in” for citizens instead of “opt-out”. In an opt-in program, citizens can be incentivized to participate through mechanisms like tax credits or deductions4. By mak-

4This is somewhat akin to percent for arts programs, where construction projects are offered tax incentives in exchange for paying for public art.
ing participation voluntary, citizens are still free to use their devices how they choose and are not subject to mandatory device inspections or audits. This is in contrast to an opt-out program, which forces all citizens to participate and punishes them via fines or fees if they do not.

Privacy— In any such performance-for-security program, it is unacceptable to simply let system auditors look into users’ devices at will to determine whether or not a mandate is being met. In line with the opt-in philosophy of the program, users must be able to voluntarily report their devices’ security effort. Additionally, security audit reports must be at a sufficiently coarse granularity so as not to reveal which programs a user runs, how often they use their device(s), or other information which might infringe on participants’ rights to privacy.

Audit Integrity— The mandate system needs to be reason-ably tamper-proof so that malicious participants cannot easily alter the audit logs in an attempt to falsely claim adherence to the mandate. This might be achieved by signing generated audit reports with an on-chip private key stored in a physically tamper-proof location and known only to the hardware vendor and auditor.

Vendors Act in Good Faith—Our approach currently assumes that vendors are generally honest and law-abiding and will make a best-effort attempt to adhere imposed mandates. Of course, this is a strong assumption which may not be the case in the real world. We save the issues of cheating or malicious vendors for future work.

7 Determining Payouts

The COMMAND system allows for a regulator to enact an open mandate, while our DNN-based model enables on-device resource consumption, as shown in Steps 1 and 2 in Figure 1, respectively. Deployment considerations (Step 3 of Figure 1) are discussed in Section 6. We now examine the last part of COMMAND—Step 4 in Figure 1—in which a regulator mediates compensation to users in exchange for participation in the COMMAND mechanism. We identify two reasons why a user should be compensated:

Incentivize Security— First, the COMMAND system can be used to incentivize security adoption among users. Users—who are generally unaware of which security measures they should employ—are disincen-tivized from hurting performance in the name of security if they are unsure or unaware of how much more secure they become in exchange. As previously discussed, tax breaks like credits or deductions can be used to incentivize security adoption among users who might not otherwise be willing to trade off performance for security.

Mediate Hardware Patching— Unlike software patching, hardware patches (such as in response to Spectre [16] and Meltdown [18]) often come with high performance over-heads, which can be as high as 30% [27]. Payouts and rebates for after-purchase performance losses may become commonplace as more and more hardware vulnerabilities are found. For example, in a recent lawsuit, Apple had to pay out $500 million to customers for throttling older iPhones. In a world where hardware updates and patches become a regular feature of device ownership, a COMMAND-like system of measuring the performance cost due to patches may become necessary. Under this model, users are compensated for accrued time losses due to after-purchase patches using the same methods as described in the open mandate scenario.

Naturally, the question that must be asked is, how much do users value the performance lost due to security? To answer this questions, we designed and executed an experiment that asked participants to make incentive compatible choices between money and computer performance.

7.1 Experimental Protocol

We now briefly describe our experimental protocol, which was approved by our IRB. Participants were recruited from the Mechanical Turk platform. They were asked to download a computer program. Upon running the program, participants were given the option to slow down their computers by some percentage for 24 hours in exchange for some amount of money. Participants could either decline the offer, removing them from the study, or accept the offer, and endure the consequences of a slowed down computer for 24 hours. After the 24 hours had elapsed, remaining participants would again be offered the same amount of money to keep their devices slowed down for an additional 24 hours. This process would then repeat for at least 7 days.

7.2 Results

We ran the experiment for 7 days at slowdowns of 10% (N=26), 20% (N=27), and for 14 days at a slowdown of 30% (N=30). For the 10%, 20%, and 30% studies, 9, 15, and 14 participants declined their given offer, respectively, while 17, 12, and 16 participants accepted their offer for the full duration of the experiment, respectively. Figure ?? demonstrates that participants who accepted to slow down their devices were actually subjected to degraded device performance.

The 10% and 20% experiments were ran with offer prices ranging from $0 to $7 at increments of $0.25, while the 30% experiment was ran with offer prices ranging from $0 to $9 at $1.00 increments. Results of participants’ decision are shown in Figure ??.

For each variant of this experiment, participants are split into one of two groups: 1) Those who accept the daily monetary offer for the duration of their participation (7 or 14 days), and 2) those who do not. The first group values their device’s performance less than the offer price given to them, indicating that their willingness to accept a 10%, 20%, or 30% slowdown is less than or equal to the daily amount offered. The lowest
We use logistic regression to find the threshold point at which it is more likely than unlikely that a participant will accept their given offer rather than reject it. This is the offer price at which it is more likely than not that a participant will accept the offer for the full duration of the experiment.

- For a 10% slowdown, we find \( x_{\text{crit}} \) to have a mean of 2.270 with a standard deviation of 0.393. At the 95% confidence level, the value of \( x_{\text{crit}} \) is between 1.536 and 3.016.
- For a 20% slowdown, we find \( x_{\text{crit}} \) to have a mean of 4.069 with a standard deviation of 0.355. At the 95% confidence level, the value of \( x_{\text{crit}} \) is between 3.394 and 4.738.
- For a 30% slowdown, we find \( x_{\text{crit}} \) to have a mean of 4.443 with a standard deviation of 0.556. At the 95% confidence level, the value of \( x_{\text{crit}} \) is between 3.396 and 5.504.

Therefore, it takes an offer of at least $2.27, $4.07, and $4.43 per day for the average user to be willing to accept a 10%, 20%, and 30% drop in performance, respectively.

8 Related Work

8.1 Security Mandates and Regulations

Attempts at mandating, regulating, or standardizing security have been made many times before. In 1983, the US Department of Defense published the Trusted Computer System Evaluation Criteria (TCSEC, commonly known as the “Orange Book”), which provided rules and guidelines for evaluating the security of commercial products [26], but its effectiveness at improving security has been questioned by many and was never widely adopted by product vendors [17]. The Orange Book and others have since been replaced by international Common Criteria (CC) standard [2] but issues of checklist security remain—adherence becomes more about compliance and paperwork and likely divert resources from actually improving security. Security standards have also been created for cryptography modules (e.g. FIPS 140 [3]) but this too is a prescriptive list of design constraints and mandatory documentation. Security standards like CC and FIPS 140 have never been mandated for consumer products and are not widely used outside of government. In addition to standards, security has been mandated through presidential directives and executive orders [11,12,25]. but these too are generally pre-
(a) 10% slowdown. The crossover point is $2.27 ± 0.75 at the 95% confidence level.

(b) 20% slowdown. The crossover point is $4.07 ± 0.67 at the 95% confidence level.

(c) 30% slowdown. The crossover point is $4.43 ± 1.05 at the 95% confidence level.

Figure 9: Logistic regression results from the 10%, 20%, and 30% slowdown study. Each dot represents a unique participant. Some offer prices were given more than once, particularly in the 30% slowdown experiment.

scriptivist approaches and are aimed primarily at government. One exception is the NIST Cybersecurity Framework [19], which was created specifically with non-government organizations and infrastructure in mind, although its optional and compliance is not mandatory.

Some work has focused on non-mandated approaches towards overcoming consumer information asymmetry in an attempt to nudge vendors into providing more secure products. Kelley et al. propose a “nutrition label” for products [15]; Later work reports on expert advice for what should be on such a label [9] and find that such labels could influence consumer purchasing behavior [10].

8.2 Performance Modeling and Prediction

Some early work aimed to predict computer performance from provisioned hardware resources and configurations in an effort to design better and more efficient systems [6,8]. Saavedra et al. estimates benchmark program runtimes from program semantics and machine characterizations combined [24]. Other work predicts energy overheads in addition to performance [20–22]. To our knowledge, our approach is the only attempt to estimate runtime predictions of performance and the only to make real-time predictions of security overheads.

9 Conclusion

In this work we set out to demonstrate that a government mandated security allotment would be 1) beneficial and 2) feasible. We establish the benefits of such a requirement by performing detailed analysis of a Monte-Carlo model built from real-world ransomware data. We present the feasibility of such a scheme by constructing a Deep Neural Network to predict performance overheads for two specific security overhead features: 1) the runtime memory safety defense and 2) binary hardening via various compiler flags – gathering overheads via the standard SPEC CPU 2017 benchmark suite. Then, we show that generalization to more security features would be a low overhead task to undertake given the framework we construct throughout this work. Given that in the past, various governments have begun to take action to ensure security for their constituents, we hope that this work would inform such parties of how to implement such a security-performance mandate and demonstrate that it is not an outlandish suggestion with respect to the security-deprived and malicious-actor-populated state of the world today.

Acknowledgments

We would like to thank Lydia Chilton for her contributions to the Mechanical Turk surveys.
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