Impact Study: Initial LaPHIE Implementation

Louisiana Office of Public Health, STD/HIV Program

Special Projects of National Significance Program Systems Linkages and Access to Care for Populations at High Risk of HIV Infection Initiative: Louisiana

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Executive Summary

This study – the first in a series of studies produced by PRG on the efficacy of LaPHIE – aims to assess if the LaPHIE system impacts whether an individual who is recently out of care will reengage in care.[1] Research on LaPHIE exists, including one study that looks at HIV-related outcomes associated with being identified by LaPHIE.[2] The current study advances these efforts by: (1) looking at reengagement in care itself as the outcome of interest; (2) aiming to draw causal inferences about the program's impact as opposed to associational outcomes; and (3) investigating the broader impact of the program on the community of individuals who are newly out of care as opposed to just those who are flagged by LaPHIE.

Reengagement in care is the most proximate desired outcome of the LaPHIE system. Previous research has examined whether a LaPHIE identification (i.e. being flagged) is associated with desirable – yet more distal – outcomes, including changes in CD4 and viral loads, but up to now there are no empirical investigations of the more direct and causal relationship between the LaPHIE system and reengaging in care. We are able to make causal inferences because we compare out-of-care individuals who are potentially exposed to LaPHIE with an equivalent group of out-of-care individuals who have no chance of being identified by the system.

Our approach is known as a natural experiment and it uses the initiation of the LaPHIE system in 2009 as an assignment mechanism by which out-of-care individuals are assigned to treatment and comparison conditions in an "arguably random" manner. Individuals are assigned to the comparison group if they fall out of care in the two years before LaPHIE is activated and to the treatment group if they fall out of care in the two years after LaPHIE is operational. The validity of the study rests on the argument that the assignment procedure is random and produces an analytic sample that is balanced. While this claim is not entirely verifiable, baseline balance diagnostics do indicate that our treatment and comparison samples are remarkably equivalent across the limited number of variables that are available. We expect this study to contribute to the existing research and provide necessary context and baseline estimates for upcoming research that will examine the impact of changes made to LaPHIE under the HRSA Special Topics of National Significance (SPNS) grant. A complete overview of our design can be found in the April 2013 *PRG Local Evaluation Plan* and details of our analytical methods can be found in the appendices section of this report.

We present a summary of findings here and elaborate on them further in subsequent sections of this report:

- In our benchmark (causal) analysis, we find that LaPHIE does not produce a statistically significant impact (at the $p < .05$ level) on the propensity of out-of-care individuals to reengage in care.

- The estimated average treatment effect is, however, marginally significant (at the $p < .10$ level). In conjunction with this, follow-up analyses and sensitivity studies do provide evidence of a real and meaningful but small impact of LaPHIE on reengaging in care. In part, we base this conclusion on the following: (1) a power analysis indicates that our analysis is not sufficiently powered; (2) a

[1] An individual is considered reengaged in care if, after falling out of care (i.e., going one year, or 13.5 months without a CD4 or viral load test), he or she receives at least one CD4 or viral load lab test during the study window.

[2] Herwehe, J., Wilbright, W., Abrams, A., Bergson, S., Foxhood, J., Kaiser, M., Smith, L., Xiao, K., Zapata, A. & Magnus, M. (2012). Implementation of an innovative, integrated electronic medical record (EMR) and public health information exchange for HIV/AIDS. Journal of the American Medical Informatics Association, 19(3), 448-452; Magnus, M., Herwehe, J., Andrews, L., Gibson, L., Daigrepont, N., De Leon, J. M., Hyslop, N. E., Styron, S., Wilcox, R., Kaiser, M. & Butler, M. K. (2009). Evaluating health information technology: provider satisfaction with an HIV-specific, electronic clinical management and reporting system. AIDS patient care and STDs, 23(2), 85-91; Magnus, M., Herwehe, J., Proescholdbell, R. J., Lombard, F., Cajina, A., Dastur, Z., Millery, M. & Sabundayo, B. P. (2007). Guidelines for Effective Integration of Information Technology in the Care of HIV-infected Populations. Journal of Public Health Management and Practice, 13(1), 39-48.

sensitivity study indicates that covariates and regional controls do not substantively change the magnitude and significance of the impact estimate; (3) another sensitivity study with a reduced sample estimates impacts of a greater magnitude that rise to the level of statistical significance. In short, we find that persistent results across different analytical conditions are compelling evidence of a real –and not chance – impact, that being eligible for a LaPHIE flag increases the probability that one will reengage in care.

- While the causal effect that we estimate is very small,[3] it is also meaningful because it represents the predicted impact on the full population of individuals who are newly-out-of care and eligible to be flagged by LaPHIE – not just those who have been directly identified by the system. And though it may seem imprecise to estimate an effect beyond that which applies to those exposed directly to the intervention itself (i.e. the flag), it is a necessary attribute of the causal analysis. It is also, we contend, at least as policy relevant as the alternative narrower estimate because it applies to the full (and known) population that could potentially benefit from the intervention. After the fact, we may know how many have been flagged, but it seems important to offer an impact estimate that is applicable to the full set of individuals who may benefit and one that is known beforehand.

- In an exploratory (i.e. non-causal) analysis we find evidence of a statistically significant impact when we model the treatment effect as a non-linear interaction with time. Findings indicate that the statistically detectable effect of the LaPHIE system tends to manifest itself by the 6th month, peak in the 7th month and be gone by the 14th month.

Explanation and Logic of Analysis

Our study aims to determine if potential exposure to the LaPHIE system increases the likelihood that newly-out-of care individuals will reengage in care. Our approach uses the initiation of the LaPHIE system in (April through August) 2009 as an assignment mechanism by which out-of-care individuals are assigned to treatment and comparison conditions in an "arguably random" manner. Individuals are assigned to the comparison group if they transition to an out-of-care status in the two years before LaPHIE is activated and to the treatment group if they transition to an out-of-care status in the two years after LaPHIE is operational.

While we are interested in a full empirical exploration of the relationship between LaPHIE and reengagement, our core intention here is to make causal and not just associational inferences about the effects of the program. We are able to make such inferences with credibility only when we compare outcomes for groups that are equivalent except for exposure to the treatment of interest (i.e. being listed as out-of-care in the LaPHIE system). Such inferences also require that inclusion into these groups is not explainable by any factors that could plausibly or theoretically influence our outcome of interest. For example, since individuals directly identified by LaPHIE are systematically different from those who are not (one set of individuals comes into contact with the healthcare system implementing LaPHIE and the other does not) contrasting outcomes for these two groups would likely bias results because the background and motivational factors that result in different behaviors (appearing at a hospital) are confounded with the "treatment" (i.e. receiving a flag).

In the absence of a randomized controlled trial, PRG investigated a number of quasi-experimental techniques that might have the capacity to generate equivalent treatment/comparison groups. One conventional approach – propensity score matching – was considered, but ultimately dismissed because

[3] While we believe that the specific magnitude of the estimate is contingent upon some assumptions used in constructing the scope of (and intensity of direct exposure within) our analytic sample (the definition of the hospital catchment areas, for example), the effect remains generally of the same magnitude (and significance) across a number of different models and samples.

we determined that sufficient covariate data were not available or complete enough to make a compelling match. We felt that since an array of data on important background characteristics were not available, any balance that could be established would be specious at best.

Without random assignment or a robust set of covariates by which we could convincingly establish equivalence, our solution was to rely upon the external selection imposed by the commencement of LaPHIE as a natural experiment that assigns study participants into treatment and control groups. Our contention is that these groups should be equal in expectation in a way that approximates random selection. That is, individuals who are part of our analytic sample – persons who become out-of-care in specific catchment regions or Hospital Service Areas (HSAs) in Louisiana – will be collectively similar before and after the implementation of LaPHIE. This approach is not without its limitations, but we believe that it is the best available.

Diagnostic statistics from the benchmark and sensitivity samples produced in Table A.2 provide partial validation of this claim. We say partial, because it is impossible to assess whether unmeasured factors are balanced at baseline – as they are expected to be in a well-executed randomized study – and because the variables that exist in the data are very limited. Nevertheless, at baseline they do exhibit remarkable equivalence.

Analysis

Overview

When we estimate whether or not newly-out-of-care people with potential to be exposed to LaPHIE have a higher likelihood of reengaging in care than those who do not have potential to be exposed as an average treatment effect, we find no statistically significant impact.[4] However, when we consider these results within the context of additional and exploratory analyses we find reason to conclude that there is some evidence of a real but small impact of LaPHIE on reengaging in care. In brief, our justification for this analytical conclusion is as follows: (1) in our benchmark analysis there is a slight difference in reengagement between our treatment and comparison groups that borders on statistical significance; (2) power analysis indicates that our analysis does not include enough observations to detect with significance the magnitude of the impact that is estimated by our model;[5] (3) a sensitivity study indicates that the effect that we are modeling as a treatment effect is entirely explainable by a LaPHIE flag; (4) a sensitivity study that models treatment alongside a wide range of covariates and regional controls does not substantively change the magnitude and significance of the impact estimate; (5) a sensitivity study that models the treatment effect identically but with a different sample of individuals produces similar, albeit more significant, results.[6]

An exploratory analysis intended to further investigate the nature of the treatment effect offers further support for this inference. Findings indicate that the effect of being eligible for a LaPHIE flag is contingent on the amount of time that an individual has spent out of care. For roughly eight months after being eligible for a flag, out-of-care individuals who have potential LaPHIE exposure are more likely to reengage in care than those who do not. Results in this case are statistically significant.[7]

Moreover, while the magnitude of the effect is small, it is not necessarily substantively unimportant. It is worth emphasizing that the "treatment" impact we estimate is one that is relevant to the entire community of recently out-of-care HIV-positive individuals and not just those who were directly flagged by the

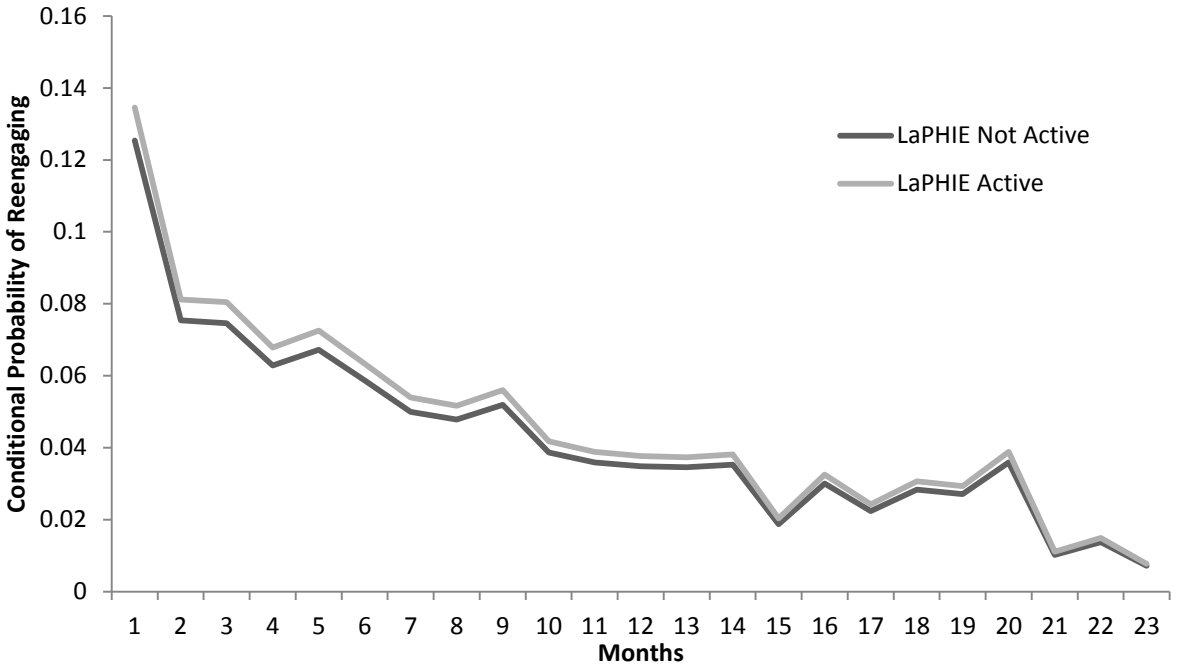
[4] Results are not significant at the p <.05 level. They are "borderline" significant at the p <.10 level.
[5] This is not a weakness in the design or planning of the study but rather an artifact of the limitations of the data. To be adequately powered, we estimate that the sample would need to be twice the size – or approximately 55,000 person-period observations.
[6] In this analytic sample we do find statistically significant results at the p <.05 level. See table E.4 in Appendix E.
[7] In the exploratory analysis results indicate a higher likelihood of linking to care between the 6th and 13th month. Marginal effects are statistically significant at the P <.05 level.

LaPHIE system. In other words, the estimated impact – an increased probability of reengaging in care that peaks at approximately .01 – applies to all individuals who have recently fallen out of care. So, all recently out-of-care individuals are predicted to be 1% more likely to reengage in care when LaPHIE is active as compared to when it is not.

Causal Analysis

Results of the causal analysis presented in Appendix D, Table D.1 demonstrate that the average treatment effect of being eligible to be flagged by LaPHIE does not result in a statistically significant increase in the likelihood of reengaging in care (at the p. <.05 level). However, the treatment effect of potential exposure to a LaPHIE flag is approaching significance at the p <.10 level. We produce a graphical representation of the predicted probabilities that result from the estimates produced in Figure 1 below.[8]

Figure 1: Estimated Conditional Probability of Reengaging in Care



The lines in Figure 1 plot the predicted probabilities that members of the treatment and comparison groups will reengage in care over the course of a 24-month period.[9] The dotted line charts the predicted "risk" that members of the treatment group will reengage in care. The solid line charts the same for the comparison group. Results demonstrate that for our sample: (1) the probability of reengaging in care in any one time period is slight – with an estimated probability of reengaging in care below .15 for all time periods; (2) the probability of reengaging in care declines as time progresses; and (3) individuals who are

[8] When we speak of the probability of reengaging in care, we are referring to the conditional probability that an individual will reengage in care during a specified time period, given that the individual was not censored or did not reengage in care in a previous time period.

[9] In our benchmark analysis we plot the "risk" of linking to care without any functional constraints on the shape of the line. See "Benchmark Model" in Appendix C for the specification of our benchmark analytical model. We also specify this model as a cubic polynomial that smooths the line. Estimates for this specification are substantively identical to the general model, as can be seen Table E.2.

in the treatment group (LaPHIE is active) have a slightly higher predicted probability of reengaging in care than do those in the comparison sample.[10]

The predicted difference in reengaging in care is small – very small.[11] However, when we recall that the impact we are estimating is not the effect of being flagged by LaPHIE but rather the effect of LaPHIE on the full population of individuals who are eligible to be flagged by LaPHIE (i.e. become out of care), an effect of this size can be considered substantively important. The magnitude of the estimated effect also provides a possible explanation for why the results are not statistically significant. A post hoc power analysis confirms this; the benchmark study is not sufficiently powered to detect an effect of this size.[12]

We therefore conduct a number of additional analyses to examine whether the statistically borderline findings are truly insignificant or are indicative of a material impact that is rendered insignificant because the study is insufficiently powered. We proceed to test the results with a number of sensitivity studies that examine: (1) whether the impact we are estimating is likely attributable to LaPHIE and not some other change in the effort to link HIV-positive individuals to care; (2) whether the findings persist with the inclusion of a range of individual-level covariates and regional controls; and (3) whether the findings persist with a different analytic sample. Since the benchmark results are borderline (i.e. fail to reach the conventional level of statistical significance), these sensitivity tests can help us interpret the findings as evidence of a real effect or a chance event.

We present a detailed discussion of our results in Appendix E. In summary, findings of LaPHIE impacts generally persist across different modeling specifications and samples. The effect that is estimated in our benchmark analysis is reproduced across a number of different scenarios – even when we include other explanatory variables and when we conduct the analysis with a different set of individuals. Supplementary analytical results thus add support for the inference that there is a small but detectable causal relationship between LaPHIE and reengagement in care. Specifically, we conclude based on the preponderance of evidence reviewed for this study that when an individual is eligible to be flagged by LaPHIE and the system is active, that individual will be more likely to reengage in care than a similar individual who is not exposed to the system.

Exploratory Analysis

Since this is an exploratory study and we believe there is practical value in a more complete understanding of how the effect of LaPHIE manifests itself, we examine specifications beyond the average treatment effect.[13] The effect that is estimated in the causal analysis says nothing about the pattern of the LaPHIE impact over time, only that on average, individuals in the treatment group are (marginally significantly) more likely to reengage in care than individuals who are in the comparison group.[14] We use a discrete-time hazard model to estimate these impacts without time-varying predictors,

[10] The difference in predicted probabilities in this model peaks at .09 in the first time period.
[11] The estimated treatment effect in a common standardized "effect size" metric is d_{cox}=.021. By convention, effect sizes below .10 are considered small.
[12] A power analysis conducted with *Optimal Design,* using standard assumptions of alpha = .05 and power = .80, and a sample size of 30,337 observations estimates that we are sufficiently powered to detect a minimal detectable effect size of .032 for a difference of means. To be adequately powered to detect the effect size estimated by our benchmark model, we estimate that the sample would need to be twice the size – or approximately 55,000 person-period observations.
[13] Note that because we are no longer estimating an average treatment effect – an average difference between the balanced treatment and comparison groups, the findings in the exploratory section do not have a causal interpretation.
[14] Even though we estimate and chart a variable baseline "risk" of linking to care over time the impact estimate itself is not time varying. The effect is in fact a constant (logit hazard) effect for all time periods, regardless of the time period itself. See, for example Singer, J. and Willet, J. (2003). *Applied Longitudinal Data Analysis*. New York: Oxford University Press. p.374.

and as such the treatment effect is modeled as an identical impact (in logit hazard terms) across all time periods. But several factors argue for the investigation of alternative patterns of impact over time.[15]

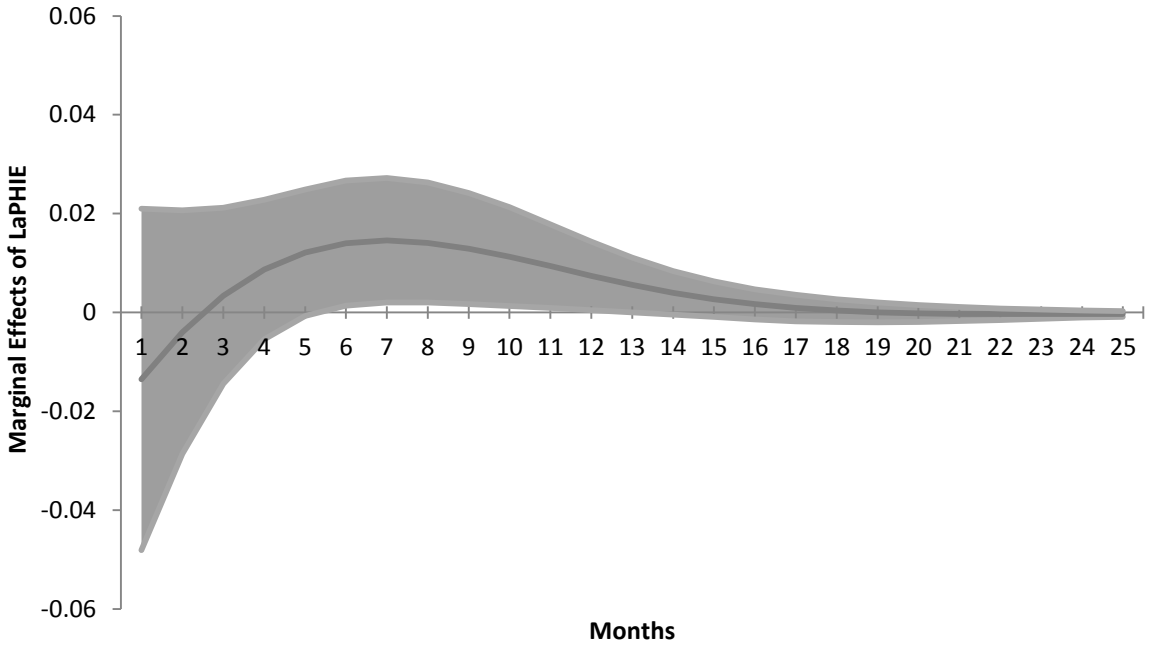
First, as is demonstrated in Figure 1, the baseline likelihood of reengaging in care itself is not constant – or even linear – over time. According to our modeling of the data, the probability of reengaging in care decreases – rapidly at first and then more gradually – as time spent out of care increases. Second, statistics presented in Appendix B that describe characteristics of how the treatment group is identified or flagged by LaPHIE are not suggestive of an immediate or linear effect with respect to time. The typical duration of time before an individual is flagged by LaPHIE (for those who are flagged) is 154 days – or five months after being eligible for that flag (by transitioning to an out-of-care status). The patterns are moreover not constant, as is illustrated Figures B.1 and B.2. But once an individual is flagged they tend to get in to care sooner rather than later – a plurality of those who are flagged (44%) get back into care (by receiving a CD4 or viral load test) the same day they are flagged.

What this suggests – to us – is that LaPHIE should have a limited initial impact on the full population who have become out of care. People who will be flagged do not tend to show up to the hospital on the first day that they are eligible. It takes a number of months for it to become increasingly likely for an out-of-care individual to (go to the hospital and) be flagged. But then we also expect this propensity to peak and then to gradually diminish, as those who are likely to go to a hospital do so – and in effect select themselves out of a remaining group of individuals who are less likely to go to the hospital (and be flagged) for whatever reason. Of those who are directly identified by the system, the pattern of influence (i.e. effect on reengaging in care) appears to be immediate with a gradual taper. As figure B.2 in Appendix B illustrates – the modal group links to care immediately and the remainder take time. The anticipated treatment effect – expressed as likelihood of reengaging in care contingent on time spent out of care – could therefore be expected to resemble an upwards curve that crests sometime after 4 to 5 months (as the probability of a flag increases) and then slowly diminishes. We test this idea with a model that specifies treatment as a quadratic polynomial interaction with time. We present estimates produced by this model in Appendix F.[16] We present a graphical representation of these marginal effect estimates, along with their confidence intervals, over time in Figure 2 below.

[15] Although it was not specified in our analysis plan, Singer and Willett (2003) and Gelman, A. and Hill J. (2006) encourage this sort of exploratory investigation with interaction terms. See also, Gelman, A. and Hill J. (2006). *Data Analysis Using Regression and Multilevel/Hierarchical Models*. New York: Cambridge University Press.

[16] Since the effects of these interactions are not linear and vary over each time period the coefficients (and their associated hypothesis test statistics) are not substantively meaningful. Instead, researchers are encouraged to present marginal effects – the estimated differences in predicted likelihood for the treatment and comparison groups across all meaningful values of time – along with their associated confidence intervals – as a means of testing hypotheses and inferring effects. See Brambor, T., Clark, W.R. and Golder, M. (2006). Understanding Interaction Models: Improving Empirical Analyses. *Political Analysis*, 14 (1): 63-82.

Figure 2: Change in the Probability of Reengaging in Care over Time, When LaPHIE is Active

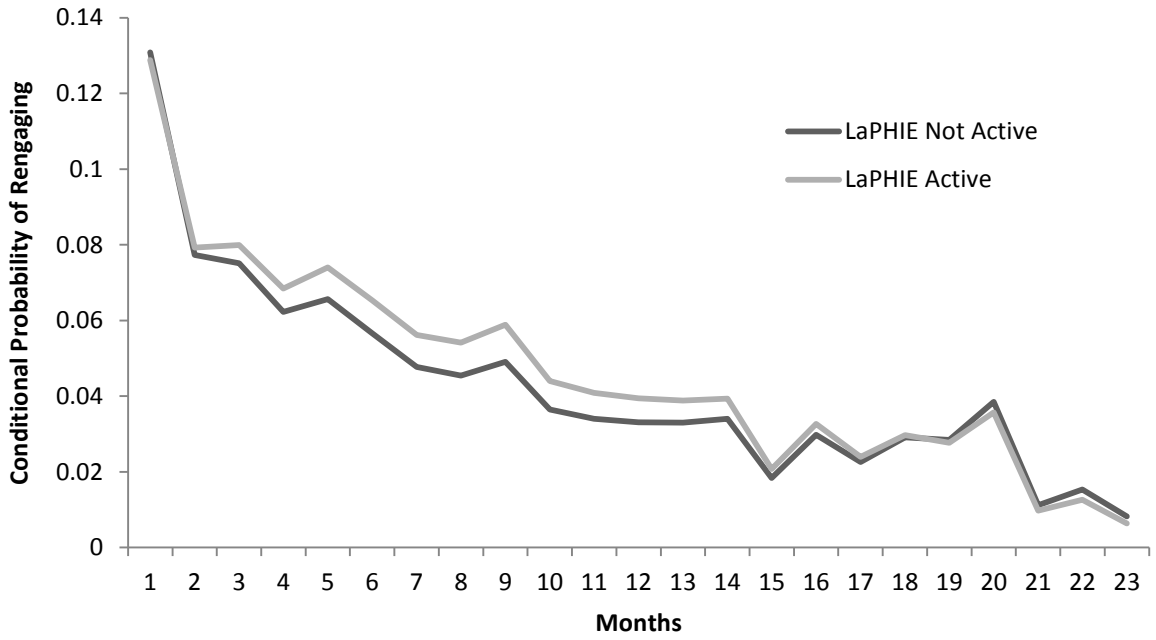


The center curved line in Figure 2 describes the estimated conditional difference in probabilities of the treatment and the comparison groups reengaging in care at each time point. The outer lines mark 95% confidence intervals of this difference and test the hypothesis that the marginal effect of LaPHIE at that time is different from no LaPHIE. In other words, when the lines do not overlap the zero line, our model estimates that the LaPHIE effect is significantly different from the comparison group (at the $p < .05$ level). The graphic thus suggests that if newly out-of-care individuals are eligible to be identified by the LaPHIE system they are significantly more likely to reengage in care between the 6th and 13th month of being out of care than individuals who have no potential exposure. The magnitude of that difference is slightly larger than the average difference estimated by the causal model.[17]

Note that Figure 2 plots the difference in probabilities and not the predicted probabilities themselves. When we graph the probabilities, as we did in Figure 1, but transpose the treatment effect to the interaction with time, as we did in Figure 2, the result is the lines pictured in Figure 3.

[17] The difference in probability estimated by the benchmark causal model peaks at .009; the difference in probability estimated by the exploratory time-interaction model peaks at .014.

Figure 3: Estimated Conditional Probability of Reengaging in Care – Exploratory Model



In this graphic, the non-linear impact of LaPHIE is grafted on top of the general baseline probability of reengaging in care.[18] The baseline "risk" (the probability of reengaging in care that is experienced by the comparison group) has a similar – though not identical – pattern to that in Figure 1 above. The obvious difference here is that the treatment group is predicted to have a more pronounced probability of reengaging in care between the 2nd and 14th month.

Discussion, Limitations, and Further Investigation

The statistical findings we discuss in this report do not lend themselves to a categorical interpretation; however, the evidence gathered suggests – on the basis of preponderance rather than a single hypothesis test – that turning LaPHIE on has resulted in a slight increase in the likelihood that an out-of-care individual will reengage in care within two years. The effect that we have endeavored to estimate is the true causal impact of the LaPHIE system itself and not something that is biased by the motivational differences between those who select into using the hospital system (and become identified by LaPHIE) and those who do not. Baseline diagnostics suggest that the assignment mechanism does appear to have been beyond the influence of participants and we have at least in part succeeded in identifying treatment and comparison groups that are equivalent.

An artifact of this approach is that we estimate an effect for the entire community of newly-out-of care individuals – not just those who have been directly identified by LaPHIE. Aside from being more valid, we contend that this information is also at least as policy relevant as the alternative narrower estimate because it applies to the full (and known) population that could potentially benefit from the intervention. After the fact, we may know how many have been flagged, but it seems important to offer an impact

[18] The predicted probabilities in Figure 3 and the estimated differences in probabilities in Figure 2 incorporate different baseline risk models. The former is based on the third order polynomial, which is desirable for its parsimony – and why we use it in this instance. The latter incorporates the general model which does not constrain the baseline risk to any functional form (e.g. linear) and is desirable for its conceptual simplicity, which is why we selected it as our benchmark approach. As is demonstrated by model estimates in Tables E.2 and E.3, both produce the same substantive results. The only difference is that the polynomial baseline model estimates significant differences between the 6th and 13th month and the general model estimates significant differences between the 6th and 12th month.

estimate that is applicable to the full set of individuals who may benefit and one that is known beforehand. And the effect we estimate is small but meaningful. Turning the LaPHIE system on increases the probability that a newly out of care individual will reengage in care by a factor of approximately .01.[19]

While the principal aim of this study is to test the hypothesis that LaPHIE is having a significant causal impact on reengagement in care, we are able to extrapolate an approximation of the influence of a flag. And while the exact numbers here are sensitive to some of our assumptions and external validity is limited to the regions and times selected, the projections are illustrative and of some explanatory value. Our causal estimate suggests that at its highest potency, LaPHIE results in a predicted 1% increase in probability that an out-of-care individual will reengage in care. From the data produced in Table B.1 in Appendix B, we know that 6.3% (n=113) of our treatment group of 1781 newly-out-of-care individuals were flagged by LaPHIE and virtually all of these individuals ended up reengaging in care (n=104). While the latter fact would seem to suggest that LaPHIE resulted in slightly less than 6% of our sample reengaging in care – the causal analysis indicates that we can attribute an effect to LaPHIE that is only (approximately) 1/6 this magnitude. In other words, the LaPHIE system caused one of every six people who were flagged by the system (and subsequently linked to care) to reengage in care. The remaining five, we may infer, would have linked to care anyway, motivated by other causal factors.

Our findings are limited by a number of assumptions, the data that are available to us, and the design of the study itself. The magnitude of the impact estimate is sensitive to a few decisions we have made to construct our analytic sample. As with any study, it is dependent on the rules of inclusion and exclusion. We believe that the scope of the hospital catchment zones or HSAs that we created – principally the concentration of flagged individuals – could increase or decrease the magnitude of the impact.[20] We demonstrate that it is sensitive to the definition of when the treatment period begins and presumably the robustness of the intervention. We made the decision to use the treatment period on the principle that it was the one we identified prior to collecting any data in the analysis plan.

We make the assertion that our findings permit causal inferences but this is based on our contention that the treatment assignment is not influenced by the participants. The balance appears convincing, but the diagnostics are based on a few variables of dubious theoretical value. This means a lot of what we find is not empirically verifiable with the data we have collected. Future work is needed to verify findings with alternative approaches. Different outcomes should be investigated with the same balanced samples. Alternative methodological approaches – such as a Granger causality study – that approach the question of impact from a completely different perspective could offer useful corroboration.

Much has been made of the internal validity of the study, but external validity has largely been ignored. The fact is that the estimates and inferences we base on them are not generalizable beyond the regions and times investigated. The purpose was to test the hypothesis that the program had an impact in the regions where it was first implemented. Future evaluative work will investigate the relative impact of changes undertaken to increase the programs efficacy as part of the HRSA Special Topics of National Significance

[19] This approximation is based on the predicted probability produced by the causal analysis, which peaks at .009.

[20] During our study period, LaPHIE was not operational statewide, and therefore it did not seem reasonable to assume that all persons in the state would have the same potential to be exposed to the system (i.e., receive care from a hospital implementing likelihood). We reasoned that including persons with a low likelihood of using a LaPHIE hospital would diminish the magnitude of any observed LaPHIE effect. Therefore, we defined hospital catchment areas or HSAs that identify zip codes from which a certain proportion (.5%) of hospital admissions are attributed. Our methods of constructing the HSAs were informed by the following: Makuc DM et al. 1991. Health Service Areas for the United States. National Center for Health Statistics. Vital Health and Statistics (2)112. Retrieved March 15, 2014 from: http://www.cdc.gov/nchs/data/series/sr_02/sr02_112.pdf; Wennberg, J. E. and Cooper, M. M. (eds). 1996. Dartmouth Atlas of Health Care: Appendix on the Geography of Health Care in the United States. Retrieved March 15, 2014 from: http://www.dartmouthatlas.org/downloads/methods/geogappdx.pdf; Gilmour, J.S. 2010. Identification of Hospital Catchment Areas Using Clustering: An Example from the NHS. Health Services Research 45(2):497-513. Retrieved March 15, 2014 from: http://www.ncbi.nlm.nih.gov/pmc/articles/PMC2838157/

(SPNS) grant. This should improve our understanding of the impact of the system as well as the comparative magnitude of those effects.

Exploratory findings suggest that LaPHIE's impact is conditional on time. The increase in likelihood of reengaging in care is not predicated on out-of-care status alone, but rather being out-of-care for a certain length of time. This impact would appear to be bounded by an individuals' propensity to visit a hospital, and before or after a set period of time we would not expect LaPHIE to have any impact. It would also stand to reason, that there are other contingencies that could better explain who is more likely to benefit from LaPHIE. That is, is the LaPHIE effect more pronounced for some and muted for others? And although we have a limited number of variables to explore this, future evaluative efforts could investigate the predictive value of these characteristics. This might help in the design of alternative strategies, or in efforts to augment or complement the current efficacies of the program.

Appendix A: Samples and Baseline Equivalence

To investigate the impact of LaPHIE on reengagement to care we employ two different analytical datasets. The first, referred to as the "April Dataset" is composed of individuals who became out of care during the two years preceding (comparison group) and following (treatment group) the implementation of LaPHIE. As can be seen in Figure A.1, the actual beginning (and close) of the study window varies based on when LaPHIE became operational at the hospitals included in study. We refer to this as our April Sample or April Dataset because we consider LaPHIE to have been fully operational for the first time in April of 2009. This dataset is the basis for our benchmark analysis. The second, which we employ to conduct sensitivity tests of the benchmark findings, is referred to as our "August dataset." This sample is composed of individuals who became out of care prior to the issuance of the first LaPHIE flag (comparison group) and following that event (treatment group). This occurs in August of 2009. As can be seen in Figure A.4, there is no site-level variation in the study window – it opens in August 1, 2007 and ends in July 31, 2011 for persons in all HSAs.

In this appendix, we present details on the analytic samples that we create from these datasets. Our study design is based on the argument that the external agency of LaPHIE being "turned on" (either in April or August) will generate treatment and comparison groups that are equivalent. As partial verification of this claim, we produce baseline balance diagnostics in the form of standardized differences. Researchers are encouraged to assess baseline equivalence with standardized difference statistics rather than hypothesis tests, such as a t-test.[21] Although there is no consensus on what value denotes balance, a difference that is less than .10 is usually understood to signify a balanced sample.[22] Others promote a more staged set of criteria where if the standardized difference for a variable is less than .05, the sample is considered balanced with respects to that variable, with no need for adjustment. Values that range between .05 and .25 are acceptable, but variables with standardized differences in this range must be statistically adjusted for in analysis (i.e., included in the regression model as a covariate). Values over .25 are considered problematic, and indicative of imbalance in the samples.[23] In any case, note that the reported balance is achieved without matching or statistical adjustment. Therefore it is conceivable (though not testable) that the unmeasured variables are similarly balanced as a randomized sample would be**.** Balance measures are presented as Hedges' g for continuous variables and the Cox index for dichotomous variables.

In addition to presenting balance diagnostics for each of the analytic samples, we also present life tables and hazard and survivor function graphics for our the April sample because it is the one we use in our benchmark analysis. This information is useful because it describes the basic characteristics of our analytic sample (and treatment and comparison samples separately) in terms of composition, by aggregating by time period entry and exit, and engagement in care.

April Dataset

Figure A.1 presents the treatment study window for each HSA, which are based on the gradual rollout of LaPHIE. According to OPH staff, initial implementation started in Interim LSU Hospital in New Orleans in February 2009 (though technical difficulties in start-up meant that the system was not fully functional until April 2009), Earl K. Long in Baton Rouge in May 2009, and the remaining hospitals in August

[21] Austin, P. C. (2009). Balance diagnostics for comparing the distribution of baseline covariates between treatment groups in propensity-score matched samples. *Statistics in Medicine*, 28, 3083–3107. Stuart, E.A. (2010). Matching Methods for Causal Inference: A Review and a Look Forward. *Statistical Science*, Volume 25, Number 1 (2010), 1-21.
[22] See Austin (2009).
[23] Guidelines for determining baseline equivalence using standardized differences are taken from the What Works Clearinghouse *Procedures and Standards Handbook, Version 3.0.*

2009.[24] The comparison window is not illustrated but it is prior to and contiguous with the treatment period and exactly the same length of time.

Table A.1, presents the number of persons in our April analytic samples. The samples are slightly different in composition as a result of missing covariate data, which has the effect of reducing the sample size for our sensitivity analyses. The benchmark analysis includes no covariates; consequently the sample is slightly larger than the "full covariate" model.

Since the treatment and comparison groups are not identical we present baseline diagnostics for both samples (in Tables A.2 and A.3). Baseline characteristics (in means or proportions) are presented for both treatment and comparison groups as well as the standardized differences between those groups. Table A.2 lists these statistics for our sample used in our benchmark analysis (model includes only time and treatment status as substantive predictors) and Table A.3 lists the statistics for our sample used in the sensitivity analysis (model includes time and treatment effect along with all observed individual and place-based covariates).[25] Results in these tables illustrate that on observed characteristics, our treatment and comparison groups are quite similar in both the benchmark (treatment and time only) and full covariate samples.

The average age at diagnosis is between 32 and 33 years, and the average age at the time the person fell out of care (i.e., entered the study) is between 41 and 42. The large majority of both treatment and comparison group members (~70%) are identified as Black or African American and a small percentage are identified as Hispanic or Latino (between 2 and 3%). Over two-thirds of both treatment and comparison groups are male and are categorized as having CDC defined AIDS. Nearly 70% of both groups were last known to be living in Regions 1 and 2, and roughly 80% lived in the Interim LSU and Earl K. Long HSAs at the time of the study. In addition, on average persons lived in places (zip codes) where one-quarter of the population lived in poverty, one-fifth did not have health insurance, and slightly over one-tenth were unemployed. The largest difference observed between groups is for location of HIV diagnosis. Approximately four to five percent more individuals in the comparison group received their diagnosis at an HIV Clinic/Counseling and Testing Site, as compared to persons in the treatment group.

The only characteristic that exhibits a difference greater than .10 standardized units is the *HIV Clinic/Counseling and Testing Site* indicator. This suggests some historical variation in the availability (or use) of testing at non-HIV specific sites over the course of the study period. The difference is not substantively important, but it does suggest that proportionately more of our comparison group received their HIV diagnosis in a counseling-and-testing clinic than did our treatment sample.

Tables A.4 and A.5 are life tables that describe the sample in terms of the risk set, number censored, and number who reengage in care in each discrete time period. Table A.4 provides statistics for the full sample used in our benchmark analysis, while Table A.5 breaks down results by treatment and comparison group. In each table we present, by time (i.e., month) the risk set (i.e. number of persons at "risk" of reengaging in care), hazard functions (the conditional probability that persons in the risk set link into care), survivor functions (i.e., the proportion of the sample that is still at "risk" of reengaging in care at the end of each time period, or month), and median lifetime (the point at which half of the sample is still at "risk: of reengaging in care). Figures A.2, and A.3, present a graphical representation of the hazard

[24] According to OPH staff, LaPHIE was first operational in each of the sites' emergency rooms. Expansion to the inpatient and outpatient facilities lagged the emergency room implementation at each site between eight to ten months. Although this certainly means that fewer people would potentially be flagged at each site during the ER-only implementation, we have made the decision to include the ER-only phase-in as part of the treatment period. While this may produce slightly lower estimates of program impact, there are fewer assumptions necessary when the treatment and comparison conditions are contiguous.

[25] It should be noted that the sample size varies across statistics presented in Table A.2., for our benchmark sample because all persons are included in this sample, but not all individuals have observations for all characteristics (this is why they are dropped in the sensitivity analysis). This means that we cannot fully diagnose baseline balance of this sample.

and survival functions of the (benchmark analysis) sample over the course of the treatment period. As can be seen in these tables and figures, the "risk" of reengaging in care is highest when a person first becomes defined as out-of-care, but then decreases over time.

The information in Table A.4 and Figures A.2., and A.3 suggest that the conditional probability of reengaging in care is highest in the first month after falling out of care. They also demonstrate that the conditional probability steadily decreases over time; by the end of the study window (24 months), none of the persons who still remained in the sample linked backed into care and roughly one-third were still in need of reengagement. The median lifetime indicates that half of persons in the full sample had linked to or re-engaged in care by 9.7 months.

Table A.5 presents this same information comparatively for the treatment and comparison groups. While the same overall trend is apparent in both comparison and treatment groups (there is a general decline in the conditional probability of reengaging in care over time), the median lifetimes indicate that the treatment group appears to be reengaging in care faster than the comparison group. Half of the treatment group has linked to care by the end of month eight, whereas it takes the comparison group two additional months to achieve the same milestone.

Figure A.1: Timeline for Treatment Periods included in the April Sample, by Hospital Service Area

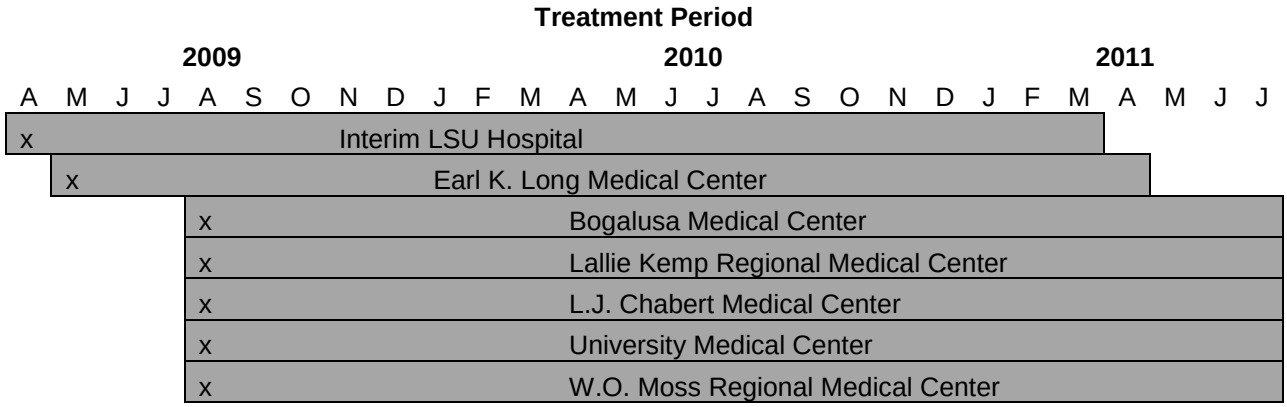


Table A.1: Overview of April Analytic Samples

Model	Sample Size	Number in Comparison	Number in Treatment
Benchmark	3681	1900	1781
Full covariate	3441	1773	1668

Table A.2: Baseline Equivalence of Comparison and Treatment Groups, April Benchmark Sample

	Comparison	Treatment	Standardized Difference
Age	**(*n* = 1900)**	**(*n* = 1781)**	
Age at HIV diagnosis	32.61	32.71	-0.01
Age at study entry	40.75	41.58	-0.07
Ethnicity	**(*n* = 1858)**	**(*n* = 1742)**	
Hispanic or Latino	2.9%	3.1%	-0.01
Race	**(*n* = 1877)**	**(*n* = 1760)**	
Black/ African American	70.5%	69.4%	0.03
White	28.4%	29.4%	-0.02
Other	1.1%	1.2%	-0.01
Gender	**(*n* = 1900)**	**(*n* = 1781)**	
Male	67.6%	67.5%	0.00
Female	32.1%	31.7%	0.01
Transgender	0.3%	0.7%	-0.06
HIV Status	**(*n* = 1900)**	**(*n* = 1781)**	
HIV Positive (not AIDS)	29.8%	33.3%	-0.08
CDC Defined AIDS	70.2%	66.7%	0.08
Location of HIV Diagnosis	**(*n* = 1900)**	**(*n* = 1781)**	
Blood Bank	2.2%	2.6%	-0.031
Family Planning/OBGYN Clinic	0.4%	0.4%	-0.004
HIV Clinic/Counseling and Testing Site	20.4%	16.0%	0.113
Emergency Room	0.9%	1.7%	-0.065
Correctional Facility	4.6%	4.2%	0.018
Drug Treatment Center	0.5%	0.7%	-0.026
Inpatient Facility/Hospital	26.1%	27.0%	-0.020
Unknown (out-of-state)	12.5%	14.1%	-0.048
Outpatient Facility/Clinic	23.8%	24.3%	-0.011
Infectious Disease /STD Clinic	8.2%	8.4%	-0.006
Other	0.4%	0.6%	-0.020
Public Health Region of Residence	**(*n* = 1900)**	**(*n* = 1781)**	
One	37.8%	38.7%	-0.02
Two	29.2%	31.4%	-0.05
Three	3.8%	2.8%	0.06
Four	8.2%	7.2%	0.04
Five	5.5%	5.0%	0.02
Six	5.6%	4.1%	0.07
Seven	1.2%	1.5%	-0.03
Eight	3.2%	2.6%	0.03
Nine	5.6%	6.7%	-0.05

	Comparison	Treatment	Standardized Difference
Health Service Area of Residence	**(*n* = 1900)**	**(*n* = 1781)**	
Interim	39.4%	40.0%	-0.01
Earl K Long	37.2%	38.2%	-0.02
Bogalusa	3.1%	2.9%	0.01
Lallie Kemp	2.2%	3.3%	-0.07
L.J. Chabert	3.3%	2.5%	0.05
University Medical Center	9.1%	8.0%	0.04
W.O. Moss	5.8%	5.2%	0.02
Mean Percent of Individuals in Zip Code[26]	**(*n* = 1838)**	**(*n* = 1728)**	
Living in poverty	24.6%	24.7%	-0.01
Without health insurance	19.7%	19.4%	0.04
Unemployed	10.7%	10.5%	0.03
With at least a high school education	80.6%	81.3%	-0.09
Who take public transportation or walk to work	7.6%	7.5%	0.01

The following are WWC Standards for establishing baseline equivalence according to standardized differences: <=.05 equivalence established; >.05 <=.25 equivalence established, with statistical adjustment; >.25 equivalence not established.

[26] The sample size is not uniform across variables. The sample size is one fewer in the comparison group (*n* = 1837) for the variables *percent of individuals who are unemployed* and *percent of individuals who take public transportation or walk to work*. The sample size is one fewer in the treatment group (*n* = 1727) for the variables *percent of individuals in poverty* and *percent of individuals with at least a high school education.*

Table A.3: Baseline Equivalence of Comparison and Treatment Groups, April Full Covariate Model

	Comparison (*n =1773*)	Treatment (*n =1668*)	Standardized Difference
Age			
Age at HIV diagnosis	32.53	32.63	-0.009
Age at study entry	40.68	41.49	-0.073
Ethnicity			
Hispanic or Latino	1.6%	2.0%	-0.030
Race			
Black/ African American	70.5%	69.2%	0.029
White	28.5%	29.8%	-0.028
Other	1.0%	1.0%	-0.006
Gender			
Male	67.6%	67.4%	0.004
Female	32.0%	31.8%	0.004
Transgender	0.3%	0.7%	-0.053
HIV Status			
HIV Positive (not AIDS)	29.7%	33.2%	-0.076
CDC Defined AIDS	70.3%	66.8%	0.076
Location of HIV Diagnosis			
Blood Bank	2.1%	2.6%	-0.036
Family Planning/OBGYN Clinic	0.4%	0.4%	-0.004
HIV Clinic/Counseling and Testing Site	20.5%	15.9%	0.119
Emergency Room	0.9%	1.8%	-0.078
Correctional Facility	4.6%	4.4%	0.012
Drug Treatment Center	0.6%	0.7%	-0.012
Inpatient Facility/Hospital	26.5%	26.6%	-0.002
Unknown (out-of-state)	12.7%	14.1%	-0.043
Outpatient Facility/Clinic	23.4%	24.6%	-0.027
Infectious Disease /STD Clinic	7.8%	8.2%	-0.014
Other	0.5%	0.6%	-0.021
Public Health Region of Residence			
One	37.2%	38.7%	-0.032
Two	29.2%	31.5%	-0.050
Three	3.8%	2.8%	0.060
Four	8.2%	7.2%	0.037
Five	5.6%	4.9%	0.030
Six	5.7%	4.1%	0.075
Seven	1.2%	1.5%	-0.022
Eight	3.3%	2.8%	0.026
Nine	5.8%	6.5%	-0.028

	Comparison (*n =1773*)	Treatment (*n =1668*)	Standardized Difference
Health Service Area of Residence			
Interim	38.9%	40.0%	-0.024
Earl K Long	37.5%	38.5%	-0.021
Bogalusa	3.1%	2.8%	0.017
Lallie Kemp	2.3%	3.1%	-0.053
L.J. Chabert	3.3%	2.4%	0.056
University Medical Center	9.1%	7.9%	0.042
W.O. Moss	5.9%	5.2%	0.031
Percent of Individuals in Zip Code			
Living in poverty	24.47	24.65	-0.016
Without health insurance	19.65	19.43	0.037
Unemployed	10.64	10.52	0.028
With at least a high school education	80.59	81.27	-0.095
Who take public transportation or walk to work	7.52	7.53	-0.001

The following are WWC Standards for establishing baseline equivalence according to standardized differences: <=.05 equivalence established; >.05 <=.25 equivalence established, with statistical adjustment; >.25 equivalence not established.

Table A.4: Life Table Describing Number of Months Out of Care, April Benchmark Study

		Number		Proportion	
Month	**Fell out of care**	**Reengaged in care**	**Censored**	**Reengaged in care**	**Persons still out of care**
1	3681	478	213	0.13	0.87
2	2990	234	113	0.08	0.80
3	2643	205	82	0.08	0.74
4	2356	154	70	0.07	0.69
5	2132	149	64	0.07	0.64
6	1919	117	70	0.06	0.60
7	1732	90	53	0.05	0.57
8	1589	79	64	0.05	0.54
9	**1446**	**78**	**50**	**0.05**	**0.51**
10	1318	53	34	0.04	0.49
11	1231	46	53	0.04	0.48
12	1132	41	62	0.04	0.46
13	1029	37	66	0.04	0.44
14	926	34	73	0.04	0.43
15	819	16	66	0.02	0.42
16	737	23	70	0.03	0.40
17	644	15	85	0.02	0.39
18	544	16	65	0.03	0.38
19	463	13	74	0.03	0.37
20	376	14	78	0.04	0.36
21	284	3	70	0.01	0.35
22	211	3	73	0.01	0.35
23	135	1	74	0.01	0.35
24	60	0	60	0.00	0.35

Median Lifetime

Risk

Hazard function

Survivor function

Figure A.2: Estimated Hazard Probability, April Benchmark Study

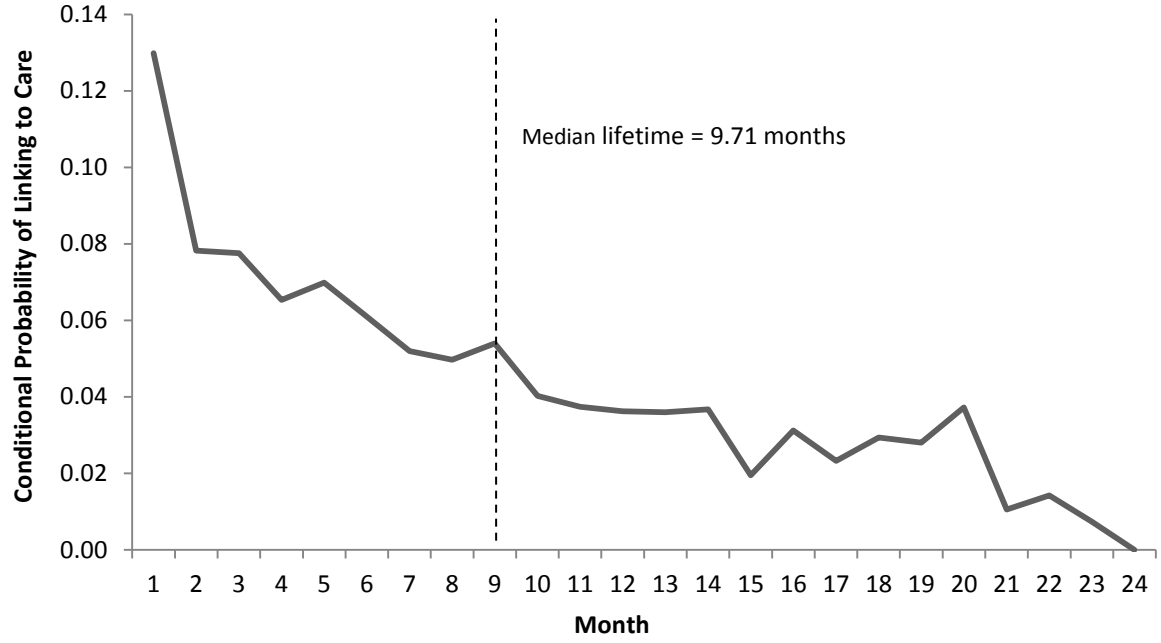


Figure A.3: Estimated Survival Probability, April Benchmark Study

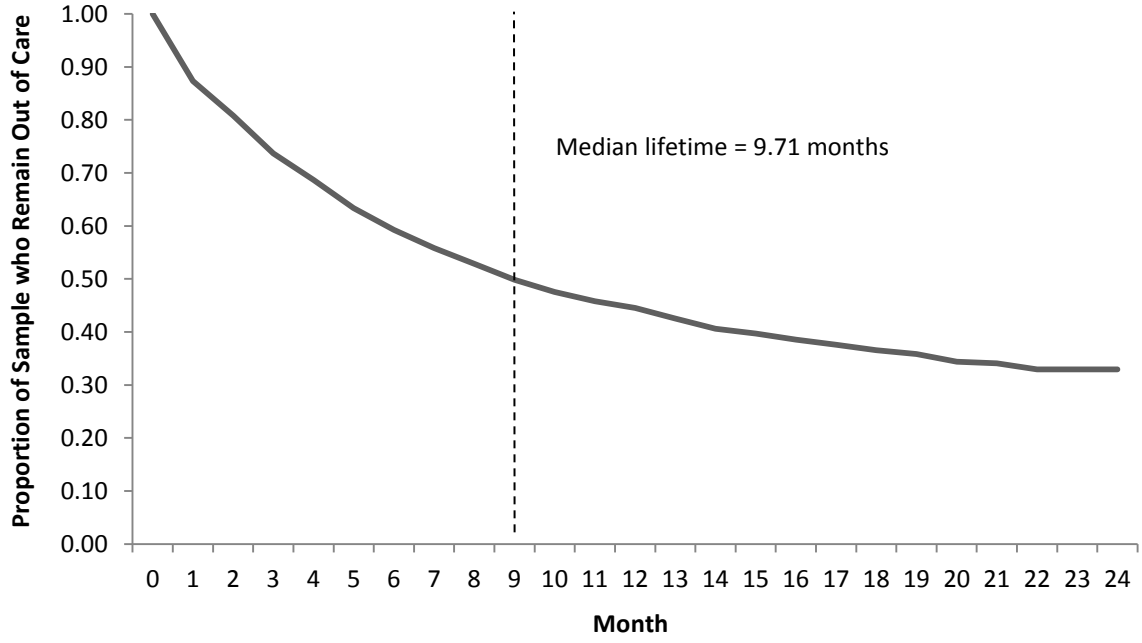


Table A.5: Life Table Describing Months Out of Care by Treatment Status, April Benchmark Study

	Comparison (LaPHIE Not Active)			Treatment (LaPHIE Active)		
Month	**Fell out of care**	**Proportion reengaged in care**	**Proportion still out of care**	**Fell out of care**	**Proportion reengaged in care**	**Proportion still out of care**
1	1900	0.13	0.87	1781	0.13	0.87
2	1502	0.08	0.80	1488	0.07	0.81
3	1304	0.07	0.74	1339	0.09	0.74
4	1165	0.06	0.70	1191	0.07	0.69
5	1059	0.06	0.65	1073	0.08	0.63
6	956	0.06	0.62	963	0.06	0.59
7	864	0.05	0.59	868	0.06	0.56
8	803	0.05	0.56	786	0.05	0.53
9	732	0.05	0.53	714	0.06	0.50
10	666	0.03	0.51	652	0.05	0.48
11	628	0.04	0.49	603	0.04	0.46
12	572	0.04	0.47	560	0.03	0.45
13	513	0.03	0.46	516	0.04	0.43
14	462	0.03	0.45	464	0.05	0.41
15	415	0.02	0.44	404	0.02	0.40
16	381	0.03	0.42	356	0.03	0.39
17	334	0.02	0.41	310	0.03	0.38
18	289	0.03	0.40	255	0.03	0.37
19	250	0.04	0.39	213	0.02	0.36
20	205	0.03	0.37	171	0.04	0.34
21	161	0.01	0.37	123	0.01	0.34
22	124	0.00	0.37	87	0.03	0.33
23	74	0.01	0.36	61	0.00	0.33
24	36	0.00	0.36	24	0.00	0.33

Note: Month 8 contains the median lifetime (8.95) for the treatment group; month 10 contains the median lifetime (10.68) for the comparison group.

August Dataset

Figure A.4 presents a graphical depiction of the treatment study window for each HSA included in the August dataset. This window is constructed to commence in the month of the first flag issued by the LaPHIE system – August 2009. Unlike the April dataset, the treatment periods for each HSA starts and ends at the same time. Again, the comparison window is not illustrated but it is prior to and contiguous with the treatment period and exactly the same length of time.

Table A.6, presents the number of persons in our August analytic samples. Again, the samples are slightly different in composition as a result of missing covariate data, which has the effect of reducing the size of the sample for our sensitivity analysis that includes these as predictors. As we did for the April sample, we present baseline diagnostics for both samples. Tables A.7 and A.8 present the baseline characteristics (in means or proportions) for both treatment and comparison groups as well as the standardized differences that exist between them. Table A.7 lists these statistics for the slightly larger sample that is available when we model likelihood of reengagement as a function of time and treatment status. Table A.8 does the same for the slightly restricted sample that have complete data when we include all observed individual and place-based covariates in addition to time and treatment status.

In terms of balance diagnostics, the statistics in Tables A.7 and A.8 demonstrate that the samples in our August study are balanced and nearly identical to those in our April study. In this case, however, the largest standardized difference is for HIV status. Over two-thirds of the treatment and comparison groups reportedly have CDC defined AIDS, but there is a noticeably larger proportion in the comparison group who have this status (roughly 5% more).

Figure A.4: Timeline for Treatment Periods included in the August Study, by Hospital Service Areas

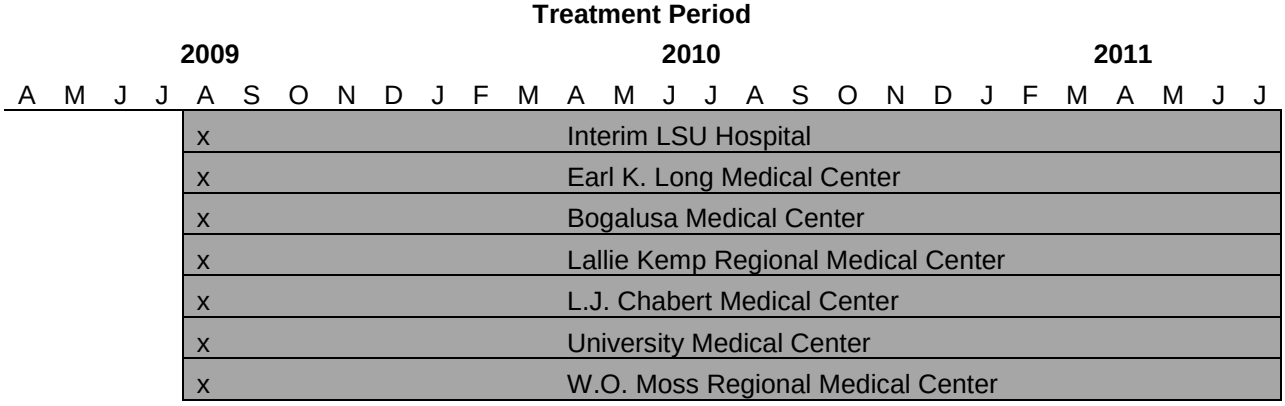


Table A.6: Overview of August Study Samples

Model	Sample Size	Number in Comparison	Number in Treatment
Treatment only	3489	1839	1650
Full covariate	3259	1715	1544

Table A.7: Baseline Equivalence of Comparison and Treatment Groups, August Treatment Only Model

	Comparison	Treatment	Standardized Difference
Age	(*n* = 1839)	(*n* = 1650)	
Age at HIV diagnosis	32.71	32.67	0.00
Age at study entry	40.94	41.67	-0.06
Ethnicity	(*n* = 1803)	(*n* = 1615)	
Hispanic or Latino	2.8%	3.3%	-0.03
Race	(*n* = 1818)	(*n* = 1630)	
Black/ African American	71.2%	69.0%	0.05
White	27.7%	29.8%	-0.05
Other	1.2%	1.2%	-0.01
Gender	(*n* = 1839)	(*n* = 1650)	
Male	67.6%	67.1%	0.01
Female	32.0%	32.1%	0.00
Transgender	0.4%	0.8%	-0.05
HIV Status	(*n* = 1839)	(*n* = 1650)	
HIV Positive (not AIDS)	29.5%	34.4%	-0.11
CDC Defined AIDS	70.5%	65.6%	0.11
Location of HIV Diagnosis	(*n* = 1839)	(*n* = 1650)	
Blood Bank	2.6%	2.3%	0.02
Family Planning/OBGYN Clinic	0.3%	0.6%	-0.04
HIV Clinic/Counseling and Testing Site	19.1%	16.5%	0.07
Emergency Room	1.1%	1.8%	-0.05
Correctional Facility	4.5%	3.9%	0.03
Drug Treatment Center	0.5%	0.7%	-0.02
Inpatient Facility/Hospital	26.4%	27.0%	-0.01
Unknown (out-of-state)	13.0%	14.7%	-0.05
Outpatient Facility/Clinic	23.6%	23.8%	0.00
Infectious Disease /STD Clinic	8.2%	8.2%	0.00
Other	0.5%	0.5%	0.01
Public Health Region of Residence	(*n* = 1839)	(*n* = 1650)	
One	35.1%	38.5%	-0.07
Two	30.9%	30.3%	0.01
Three	4.1%	2.8%	0.07
Four	8.5%	7.8%	0.02
Five	5.7%	5.4%	0.01
Six	5.7%	4.1%	0.07
Seven	1.2%	1.5%	-0.03
Eight	3.1%	2.6%	0.03
Nine	5.8%	7.0%	-0.05

	Comparison	Treatment	Standardized Difference
Health Service Area of Residence	(*n* = 1839)	(*n* = 1650)	
Interim	36.8%	39.8%	-0.06
Earl K Long	39.0%	36.7%	0.05
Bogalusa	3.2%	3.1%	0.00
Lallie Kemp	2.2%	3.5%	-0.08
L.J. Chabert	3.4%	2.7%	0.04
University Medical Center	9.4%	8.7%	0.03
W.O. Moss	6.0%	5.6%	0.01
Mean Percent of Individuals in Zip Code[27]	(*n* = 1772)	(*n* = 1599)	
Living in poverty	24.47	24.39	0.01
Without health insurance	19.57	19.26	0.05
Unemployed	10.66	10.42	0.05
With at least a high school education	80.51	81.24	-0.10
Who take public transportation or walk to work	7.41	7.34	0.01

The following are WWC Standards for establishing baseline equivalence according to standardized differences: <=.05 equivalence established; >.05 <=.25 equivalence established, with statistical adjustment; >.25 equivalence not established.

[27] The sample size is not uniform across variables. The sample size is one fewer in the comparison group (*n* = 1837) for the variables *percent of individuals who are unemployed* and *percent of individuals who take public transportation or walk to work*. The sample size is one fewer in the treatment group (*n* = 1727) for the variables *percent of individuals in poverty* and *percent of individuals with at least a high school education.*

Table A.8: Baseline Equivalence of Comparison and Treatment Groups, August Full Covariate Model

	Comparison (*n* =1715)	Treatment (*n* =1544)	Standardized Difference
Age			
Age at HIV diagnosis	32.66	32.62	0.00
Age at study entry	40.87	41.62	-0.07
Ethnicity			
Hispanic or Latino	1.5%	2.2%	-0.06
Race			
Black/ African American	71.5%	68.7%	0.06
White	27.5%	30.2%	-0.06
Other	1.0%	1.1%	-0.01
Gender			
Male	67.5%	67.0%	0.01
Female	32.1%	32.2%	0.00
Transgender	0.4%	0.8%	-0.05
HIV Status			
HIV Positive (not AIDS)	29.3%	34.3%	-0.11
CDC Defined AIDS	70.7%	65.7%	0.11
Location of HIV Diagnosis			
Blood Bank	34.5%	38.4%	-0.08
Family Planning/OBGYN Clinic	30.9%	30.6%	0.01
HIV Clinic/Counseling and Testing Site	4.1%	2.8%	0.07
Emergency Room	8.5%	7.8%	0.03
Correctional Facility	5.8%	5.3%	0.02
Drug Treatment Center	5.8%	4.0%	0.08
Inpatient Facility/Hospital	1.3%	1.5%	-0.02
Unknown (out-of-state)	3.2%	2.8%	0.02
Outpatient Facility/Clinic	5.9%	6.9%	-0.04
Infectious Disease /STD Clinic	2.6%	2.3%	0.02
Other	0.3%	0.6%	-0.04
Public Health Region of Residence			
One			
Two	19.4%	16.4%	0.08
Three	1.1%	1.9%	-0.06
Four	4.5%	4.1%	0.02
Five	0.6%	0.6%	0.00
Six	26.9%	26.7%	0.00
Seven	13.1%	14.8%	-0.05
Eight	23.2%	24.0%	-0.02
Nine	7.7%	8.0%	-0.01

	Comparison (*n* =1715)	Treatment (*n* =1544)	Standardized Difference
Health Service Area of Residence	0.6%	0.5%	0.01
Interim			
Earl K Long	36.3%	39.7%	-0.07
Bogalusa	39.3%	37.2%	0.04
Lallie Kemp	3.2%	3.0%	0.01
L.J. Chabert	2.3%	3.4%	-0.06
University Medical Center	3.4%	2.6%	0.05
W.O. Moss	9.4%	8.5%	0.03
Percent of Individuals in Zip Code	6.1%	5.6%	0.02
Living in poverty			
Without health insurance	24.38	24.37	0.00
Unemployed	19.55	19.24	0.05
With at least a high school education	10.66	10.43	0.05
Who take public transportation or walk to work	80.54	81.26	-0.10

The following are WWC Standards for establishing baseline equivalence according to standardized differences: <=.05 equivalence established; >.05 <=.25 equivalence established, with statistical adjustment; >.25 equivalence not established.

Appendix B: Exposure to LaPHIE Flag in Treatment Sample

In this appendix, we describe the occurrence of LaPHIE flags in our treatment samples. It should be noted that while the analytic samples may include individuals who were flagged after they linked to care, we do not count them as flagged in the descriptive tables and graphics below or in our sensitivity study that includes a flag indicator.[28] We have reasoned that if the flag occurs after an individual links to care there is no way that it could be causally relevant.

Tables B.1 and B.2 report descriptive statistics on the incidence of flags for the April and August samples, respectively. The statistics show that that the August treatment sample is slightly smaller, but both have a similar proportion of individuals who were flagged during the study period. On average, flags occurred roughly five months after individuals fell out of care and the vast majority (over 92%) subsequently engaged in care during the study window.

The average time from a LaPHIE flag to reengaging in care is much shorter than the receipt of a flag. On average, flagged individuals linked to care within two months of their initial flag.

Figure B.1 and B.2 present information on the length of time to the receipt of a flag graphically for both samples and Figures B.2 and B.4 present information on the length of time from flag to reengaging in care. The graphics show clearly that while it typically takes a while to receive a flag, once it has been received reengagement typically follows quickly. More than any other length of time, individuals who have been flagged reengage in care on that same day.

April Dataset

Table B.1: Overview of LaPHIE Flags in Treatment Group, April Benchmark Model

	Statistic
Number of PLWH in treatment group	1781
Number of treatment group flagged	113
Percent of treatment group flagged	6.3%
Average time from falling out of care to LaPHIE flag	153.9 days
Number of flagged individuals who engage in care	104
Average time from LaPHIE flag to engaging in care	49.6 days

[28] This has no impact on our causal analysis other than in our sensitivity study that explicitly models the receipt of a flag. OHP SHP staff report that these cases are most likely a result of a lag in lab reporting, so that persons who had already linked to care were included in the LaPHIE out-of-care dataset because their lab results had not yet been reported to OPH SHP.

Figure B.1: Days from Falling Out of Care to First LaPHIE Flag, April Benchmark Model (*n* = 113)

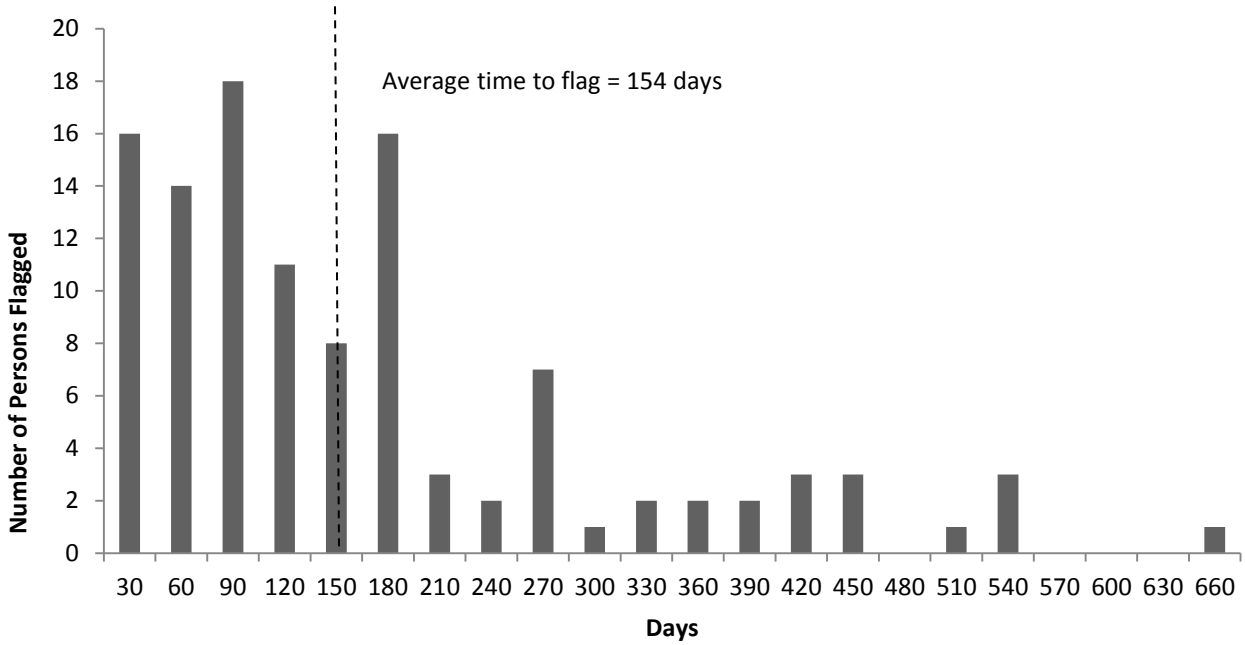
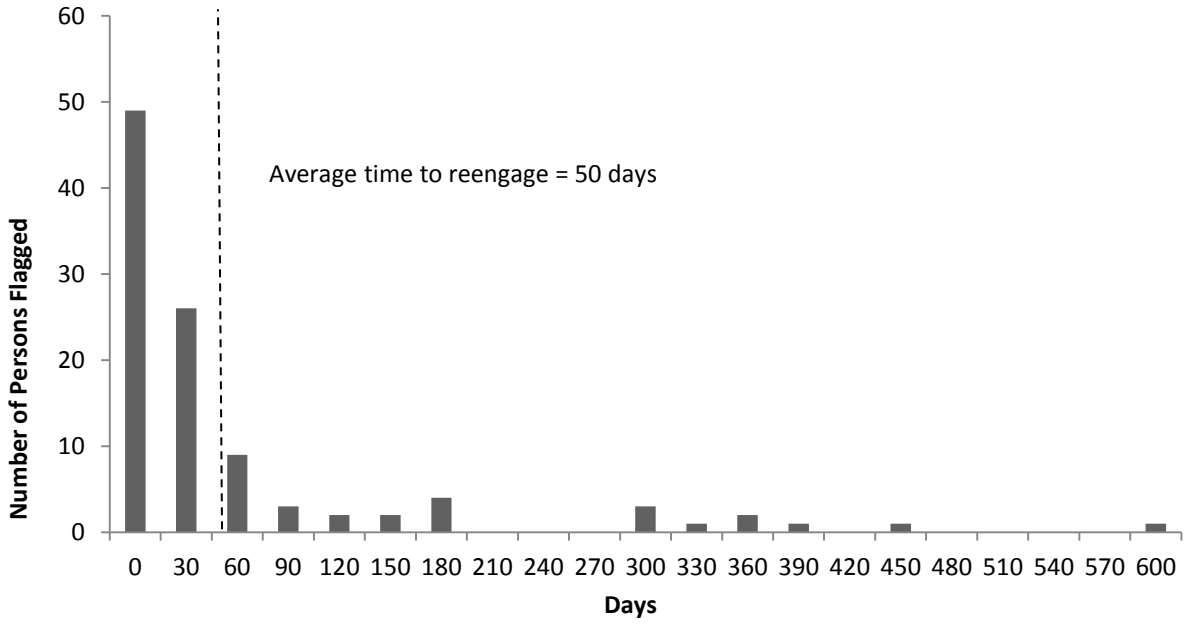


Figure B.2: Days from LaPHIE Flag to Reengaging in Care, April Benchmark Model (*n* = 104)



August Dataset

Table B.2: Overview of LaPHIE Flags in Treatment Group, August treatment and time only model

	Statistic
Number of PLWH in treatment group	1650
Number of treatment group flagged	106
Percent of treatment group flagged	6.4%
Average time from falling out of care to LaPHIE flag	143.1 days
Number of flagged individuals who reengage in care	96
Average time from LaPHIE flag to reengaging in care	44.9 days

Figure B.3: Days from Falling Out of Care to First LaPHIE Flag, August Treatment Only Model (*n* = 106)

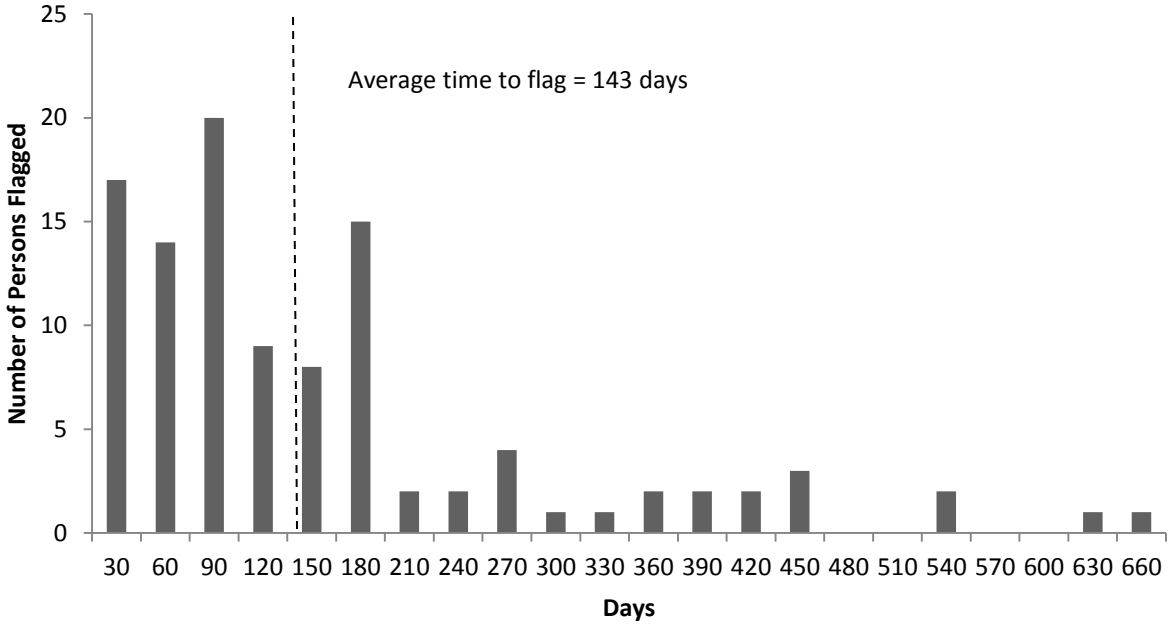
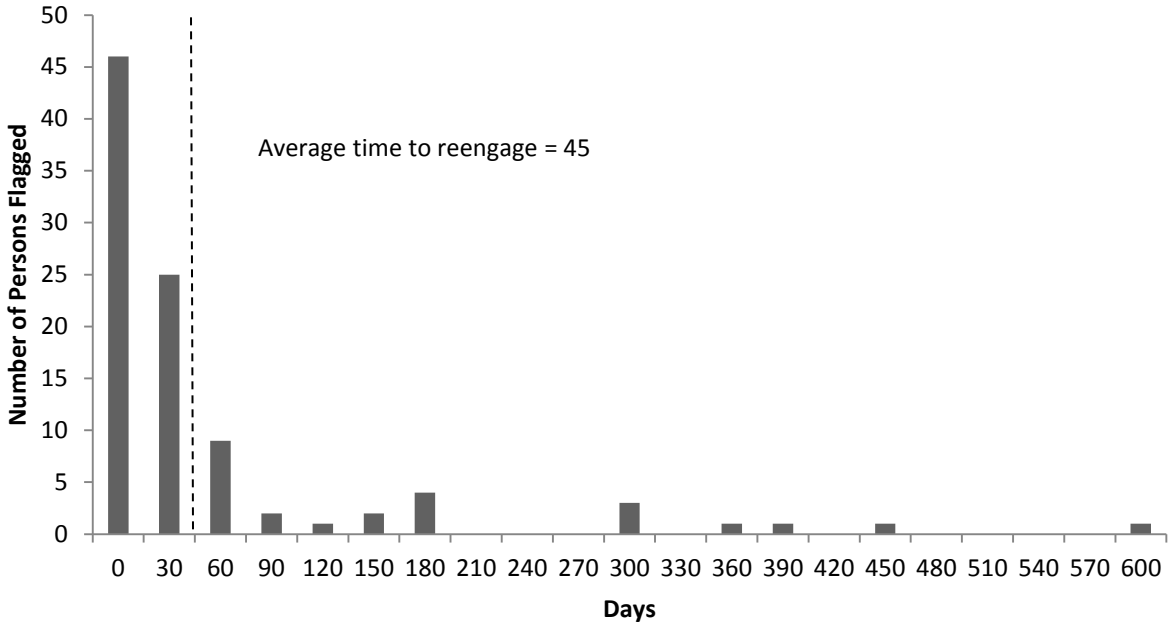


Figure B.4: Days from LaPHIE Flag to Reengaging in Care, August Treatment Only Model (*n* = 96)



Appendix C: Benchmark Analytic Model

In this appendix we present a discussion of and specifications for the benchmark analytical model, including a brief discussion of alternative options for the functional form of time, as well as the model used in sensitivity analyses. We decided to include only one substantive predictor (treatment) in our benchmark approach for two reasons. First, we had very limited covariate data that were available to us and the ones that we had were not that theoretically relevant to the outcome (reengagement in care) or the hypothesized mechanism by which the treatment could increase incidence of that outcome (health seeking behaviors). Second, unlike matching procedures, our "arguably" exogenous assignment mechanism is anticipated to produce balance in observed and unobserved characteristics. Upon review of the baseline balance diagnostics we concluded that our treatment and comparison groups were reasonably equivalent and we did not want to insinuate bias into our estimates by including what amounted to a relatively arbitrary set of covariates. We empirically test this decision with a sensitivity analysis (see Sensitivity Test 2, Appendix E). Model specifications are presented below.

We selected a general specification of time as the benchmark approach because it is conceptually the simplest model to explain. The other leading candidate was a 5^{th} order polynomial, which demonstrated decent goodness-of-fit statistics, but we decided against this because we felt it would have been more difficult to translate. We present goodness-of-fit statistics in table C.1. The selection of functional form is largely academic in the sense that both provide virtually the same estimates across a variety of samples and model specifications (see Appendix E).

Although we planned to use weeks as the discrete time period, we found in initial analyses that there were multiple periods when the hazard was zero (no one linked to care), which drops cases and can insinuate bias. For this and other reasons, we decided to use months as the discrete time periods. Slicing time as months is actually preferable because it reduces the number of variables in model and produces estimates of equal value, since the shape or detail of the baseline risk is not central to our research question on the effect of the LaPHIE system. While the final month (24) itself has no incidents of people linking to care (therefore no risk and the same problem as the weekly time period) we test the results of this model against the cubic transformation of time (compare, for instance, treatment effect estimates for Table E.2 and E.3) and the results are virtually the same.

In Table C.2, we provide details on the operationalization of our analytic variables.

Benchmark Model

This model includes a treatment indicator and time modeled as a series of dummy variables:

$$Logit\left(t_{ij}\right) = \left[\alpha_j D_j\right] + \beta_1 T_i$$

Where:

$\left(t_{ij}\right)$ = the discrete-time hazard for individual *i* at time *j*. In the estimating model, the dependent variable is the indicator (0 = no; 1 = yes) of reengagement in care for individual *i* at time *j*.

D_{ij} = a series of *j* dummy variable that indicates each time period in the study in which the event may happen. We specify this as a vector of 24 monthly indicator variables. (The alternative specification of time that we note above is a simple polynomial that includes a constant term, a month counter – from 1 to 24, a squared transformation and a cubic transformation of the counter variable).

α_j= the parameter estimate of the logit hazard for individuals in the "baseline" or comparison group at time period *j*. This represents the "risk" of reengaging in care for the comparison group at time *j*.

T_i = a dummy variable that indicates whether an individual *i* is in the treatment group (1) or in the comparison group (0).

β_1= the substantive parameter estimate of interest. This represents the difference in the logit hazard between the comparison (quantified at each month by α) and the treatment group.

Model for Sensitivity Test 2

This model includes a treatment indicator, time modeled as a series of dummy variables, and a series of individual-level covariates and regional/site controls:

$$Logit\left(t_{ij}\right) = \left[\alpha_j D_j\right] + \beta_1 T_i + \beta_p X_{pi}$$

Where:

$\left(t_{ij}\right)$ = the discrete-time hazard for individual *i* at time *j*. In the estimating model, the dependent variable is the indicator (0 = no; 1 = yes) of reengagement in care for individual *i* at time *j*.

D_{ij}= a series of *j* dummy variable that indicates each time period in the study in which the event may happen. We specify this as a vector of 24 monthly indicator variables.

α_j= the parameter estimate of the logit hazard for individuals in the "baseline" or comparison group at time period *j*. This represents the "risk" of reengaging in care for the comparison group at time *j*.

T_i = a dummy variable that indicates whether an individual *i* is in the treatment group (1) or in the comparison group (0).

β_1= the substantive parameter estimate of interest. This represents the difference in the logit hazard between the comparison (quantified at each month by α) and the treatment group.

X_{pi} = a p vector of X covariates for individual *i*. May be time variant or time invariant. For a description of each of these variables see Table C.2 below.

β_p = the effect of the covariate on the logit hazard for a one-unit change in covariate.

Specification of Time

In a discrete-time hazard model, time can be specified as a set of dichotomous variables – one for each discrete time unit included in analysis (general model) or it can be specified as a continuous variable with values that identify the discrete time units (e.g., linear or polynomial model). Therefore, prior to analysis, we conducted exploratory analyses of the baseline hazard model (i.e., time is the only predictor, no covariates are included in the model) in order to ascertain which specification of time is most well suited for our data. Table C.1 presents goodness of fit statistics for a number of alternate specifications of time.

Beginning with the linear model (which is compared to the constant model) likelihood ratio test statistics (presented in columns four and five) compare each representation of time to the less complex specification of time that was modeled previously. If the test statistic is significant, the more complex model is interpreted as the better fit model. In addition Akaike and Bayesian information criterion (AIC and BIC) measures that take into account both goodness of fit and complexity of models are presented for each model; models with smaller AIC/BIC values are considered better. Data presented in Table C.1 suggest that the fifth order model is the best specification of time. A likelihood ratio test results show that the fifth order is a better fit than the fourth order model and it has the best AIC statistic (which does not account for number of variables). Though these tests suggest a polynomial of the fifth order is the best fit for our data, because interpretation of a fifth order polynomial is difficult, we follow the advice of Willet and Singer (2003), and for our benchmark analysis use the general specification of time – which the table shows is better than all specifications other than the fifth order model. Because goodness of fit estimates suggest the cubic polynomial is a better fit than the quadratic, has the lowest BIC statistic, and is conceptually easier to understand, we use the cubic transformation in sensitivity analyses.

Table C.1: Measures of Fit for Models with Different Representations for Main Effect of Time

Representation of Time	n parameters	Deviance	Difference in Deviance in comparison to		AIC	BIC
			Previous Model	General Model		
Constant	1	14208.73	--	**476.56** (22)	14210.73	14219.05
Linear	2	13798.72	**410.01** (1)	**66.55** (21)	13802.72	13819.36
Quadratic	3	13783.61	**15.11** (1)	**51.44** (20)	13789.61	13814.58
Cubic	4	13771.3	**12.31** (1)	**39.13** (19)	13779.3	13812.59
Fourth Order	5	13768.7	2.61 (1)	**36.53** (18)	13778.7	13820.31
Fifth Order	6	13751.64	**17.05** (1)	19.47 (17)	13763.64	13813.58
General	23	13732.17	--	--	13778.17	13969.53

Note: Likelihood ratio test results are presented in the fourth and fifth columns; significant results (at n<.05) are in bold, and degrees of freedom are in parentheses.

Table C.2: Analytic Variable Operationalization

Outcome	Description of outcome and operationalization
Reengaged in care	Reengaged in care is operationalized as having at least one CD4 or viral load test during the two-year study window after transitioning to out-of-care status during the same study period. Individuals become out-of-care when they fail to record a CD4 or viral load test in the preceding 12 months. Reengaged in care is coded as a time-variant dummy indicator. Each individual who transitions to out-of-care status at any of the 24 discrete time intervals in the two-year study window (i.e. treatment or comparison) is coded 0.Any individual who has fallen out of care (i.e. entered the analytic sample and coded 0) but then receives at least one CD4 or viral load test will be coded as 1.

Covariate	Description and operationalization	Rationale for inclusion
Age:		Studies have shown that compared with younger patients, older patients are more likely to delay entry into HIV medical care (Bamford et al 2010; The Natural History Project Working Group for COHERE 2014; Reed et al 2009; U.S. Department of Health and Human Services Health Resources and Services Administration HIV/AIDS Bureau 2011).
at diagnosis	The variable is operationalized as the individual's age in years at time of HIV diagnosis.	
at time of study	The variable is operationalized as the individual's age in years at time of inclusion in study.	
Hispanic ethnicity	The variable is operationalized as a dummy variable where Hispanic or Latino individuals are coded as 1 and all others are coded as 0.	Research has shown Hispanic ethnicity to be significantly related to timing of entry into primary care (Reed et al 2009; Bamford et al 2010).
Race	The variable is operationalized as a set of 3-1 = 2 dummy variables that include: White; Black/African American; Other (American Indian or Alaska Native, Asian, Native Hawaiian or Pacific Islander, Multiracial).	Surveillance data indicate that African Americans, as compared to other groups, are less likely to link to or be retained in care (CDC 2014).
Location of HIV diagnosis	The variable is operationalized as a set of 11-1 = 10 dummy variables that include: Blood bank, Family Planning Prenatal, HIV Care Site, ER, Correctional Facility, Drug Treatment Center. Inpatient facility, Outpatient Facility, STD Infectious Disease Clinic , Other	Research indicates that individuals are less likely to be engaged in care if they were diagnosed in public facilities or in non-medical environments (Pollini et al., 2011, Torian & Wiewel, 2011; Hall et al., 2012; Hu et al., 2012; Jenness et al., 2012; Yehia et al., 2012).
Hospital service areas	The variable is operationalized as a set of 7-1 dummy variables that represent the hospital service areas, or catchment populations, of each of the LaPHIE hospitals included in analysis	A person's potential to be exposed to LaPHIE is largely dependent on whether the hospital a person is most likely to use has implemented the system. As such, we have used patient flow methods, similar to those recommended by the CDC and Dartmouth Medical School, to define service areas for each LaPHIE

Covariate	Description and operationalization	Rationale for inclusion
		hospital. In brief, zip codes were assigned to a service area if data provided by LSU indicated they accounted for more than .5% of the hospital's admissions during the time of the study (CDC 1991; Dartmouth Medical School 1999; Gilmour 2010).
Region of residence	The variable is operationalized as a set of 9-1= 8 dummy variables that represent the Louisiana public health regions.	Regional differences in public polices and health systems have been shown to greatly affect access to health care and individual health outcomes (Adler and Newman 2002; Mugavero 2011; Murray et al 2006)
Percent of persons in each zip code:[29]		
Living in poverty	The variable is operationalized as the percent of persons in the zip code living below the poverty line.	Socio-economic factors are known to be important predictors of engagement in medical care and health inequalities. In the absence of sufficient individual level data, and as individual and regional well-being haven been shown to be inextricably linked, we use place-based (aggregate, zip-code level) variables as proxies for socio-economic factors (Adler and Newmand 2002; Craw et al 2012; Mugavero 2011; Murray et al 2006; Pollini 2011).
Without health insurance	The variable is operationalized as the percent of persons in the zip code without insurance.	
Unemployed	The variable is operationalized as the percent of persons in the zip code who are unemployed, but not outside the workforce.	
With at least a high school education	The variable is operationalized as the percent of persons in the zip code who have obtained the equivalent of a high school degree.	
Who take public transportation or walk to work	The variable is operationalized as the percent of persons in the zip code who take public transportation or walk to work.	

[29] Data used to construct these measures were obtained from the U.S. Census Bureau's 2008-2012 5-year American Community Survey estimates.

Appendix D: Benchmark Model Estimates

In this appendix we present model estimates for the benchmark analysis. For a description of the model used to produce these estimates see Appendix C. The estimate of substantive interest is the Treatment effect at the bottom of Table D.1. Results indicate that the effect is significant at the p <.10 level. The coefficients (β) themselves are in logit (hazard) terms, which are difficult to conceptualize. Figure 1 in the body of the report graphs these results in terms of predicted probabilities, which are easier to comprehend.

Table D.1: General Discrete-Time Hazard Model Regression Results

Variable	β	SE
Time (Month)		
1	-1.94***	0.86
2	-2.51***	0.86
3	-2.52***	0.86
4	-2.70***	0.86
5	-2.63***	0.86
6	-2.78***	0.86
7	-2.95***	0.86
8	-2.99***	0.86
9	-2.91***	0.86
10	-3.21***	0.87
11	-3.29***	0.87
12	-3.32***	0.87
13	-3.33***	0.87
14	-3.31***	0.87
15	-3.96***	0.89
16	-3.48***	0.88
17	-3.78***	0.9
18	-3.54***	0.9
19	-3.58***	0.9
20	-3.29***	0.9
21	-4.58***	1.03
22	-4.27***	1.03
23	-4.94***	1.32
Treatment Effect[30]	0.08~	0.05

*** p<0.001, ** p<0.01, * p<0.05, ~ p<0.10

[30] *Treatment Effect* refers to the effect associated with being a member of the treatment group as compared to the comparison group. That is, the difference in the log likelihood of linking to care associated with becoming out of care while LaPHIE was first active as compared to becoming out of care when LaPHIE was not active.

Appendix E: Sensitivity Studies

In this appendix we present a discussion of the sensitivity studies we employ to test the robustness and validity of our benchmark model and its estimates. We first discuss the findings of each sensitivity study and then present the estimates for each in Tables E.1 through E.7.

Sensitivity Test 1: Statistically Removing the Flag Effect from Treatment Impact
This analysis tests the validity of the effect we seek to measure. What we aim to measure is the effect of LaPHIE being active – that is the potential exposure to a LaPHIE flag and not the flag itself. We are able to test whether our estimate has identified this potential exposure – and not some other historical effect coincides with the treatment period – by statistically removing individuals from the treatment group after they have been flagged. This is achieved by including a variable that indicates all person periods after an individual has been flagged. The rationale here is that if we separately account for a direct flag effect, we should observe no LaPHIE treatment effect because the mechanism for reengaging in care (the flag itself) has been directly included in the model. We would thus expect to see the resulting estimate of a treatment effect lose significance and magnitude. Results in Table E.1 confirm these expectations. With a flag variable included in the model, the treatment (LaPHIE) effect falls out entirely. Results thus offer corroboratory evidence that our point estimate is a valid measure of LaPHIE being active.

Sensitivity Test 2: Inclusion of Covariates and Other Controls
Our second test of our benchmark analysis seeks to determine if the impact estimates are robust to the inclusion of other covariate and regional control variables. By including additional variables into the model, we are testing whether the treatment effect we observe is reduced by the inclusion of other possible explanatory factors – namely the individual and regional characteristics that are available in the dataset provided by OPH. (For a full description of the variables included in the full covariate model, see Appendix C, above.) This is the equivalent of the regression adjustment that is used in observational studies, whereby background differences in the individuals in the analytic sample are controlled for statistically.

One limitation to and complication of this approach here is that there are few truly theoretically relevant variables in the data we have been provided. We have basic background HIV-characteristic and demographic variables as well as a number of fairly coarse regional indicators. This is complicated by the fact that our basic model is the most appropriate one (i.e. it should yield unbiased estimates) for a sample that is balanced in both measured and unmeasured background characteristics. If our sample is in fact truly balanced – as opposed to being apparently or putatively so – the inclusion of selective variables in the analytic model could bias our impact estimates (for e.g., by insinuating omitted variables bias) rather than making those estimates more precise. The point we are making here is that the model with additional variables is not necessarily the preferred model. It may in fact produce estimates that are less precise and more biased. Results in any case again corroborate our basic, benchmark model. Estimates presented in the final two columns of Table E.2 (i.e. Model 3) demonstrate that the treatment effect is substantively and statistically equivalent to that produced by the benchmark model (Model 2). Findings therefore, again add support to our benchmark results.

Sensitivity Test 3: Conduct Same Analysis with Different Sample
The third test is to conduct the same analysis on a different analytic sample to determine if the observed treatment effect persists – and therefore corroborates our benchmark results – or if it is an artifact of the sample itself. Our initial idea for this sensitivity test was more particular – we intended to distill the initial sample to one that had more flags so that we could test the hypothesis whether the effect we observe was greater in magnitude and significance. For a description of that sample see August Datasets section of

Appendix A.[31] We expected this to produce more flags because an analysis of flags produced by the LaPHIE system demonstrated that even though the system was nominally active, LaPHIE did not identify individuals in our sample until August.

In the end, findings do support the hypotheses (estimates are presented in Table E.4). Results (in the final row) indicate that potential exposure to LaPHIE demonstrates a greater and this time statistically significant increase in likelihood of reengaging in care.

The complication is that the sample that has been created does not include more flags per treatment participant as intended.[32] While this does not necessarily undermine the inference of LaPHIE impact, it does call into question the hypothesized dynamics that produced the results. Our reasoning at present is that it may be the quality of programming that improved over time rather than the quantity. The hypothesis could thus be modified to be that more consistent and capable health-care staff response and action as a result of the flags would similarly result in an increased LaPHIE impact. At this point, however, this lies beyond the scope of the data. We do not have measures of the quality of staff response; therefore we can only postulate that this is what is producing the results.[33]

[31] The resulting analytic sample is similarly balanced; baseline diagnostics produced in Table A.7 and A.8 demonstrate treatment and comparison equivalence.
[32] See Appendix B, Table B.1 and Table B.2 for a description of the number of flags in each sample. In fact the proportion of flags per treatment sample member is virtually the same as our initial sample. The reason for this appears to be that there is this latency period between entering the sample (becoming newly out-of-care) and being flagged.
[33] We also test these estimates by including the full set of covariates and controls as we do with the benchmark analysis. Findings are similarly consistent although the treatment effect becomes just barely not significant at the $p<.05$ level with the full set of covariates and regional controls added. See Table E.6 in Appendix E.

April Samples

Table E.1: General Discrete-Time Hazard Model Results including LaPHIE Flag, April

Variable	β	SE
Time (Month)		
1	-1.94***	0.05
2	-2.51***	0.07
3	-2.52***	0.08
4	-2.70***	0.09
5	-2.63***	0.09
6	-2.78***	0.1
7	-2.95***	0.11
8	-2.99***	0.12
9	-2.90***	0.12
10	-3.21***	0.14
11	-3.28***	0.15
12	-3.32***	0.16
13	-3.32***	0.17
14	-3.30***	0.18
15	-3.94***	0.25
16	-3.46***	0.21
17	-3.77***	0.26
18	-3.52***	0.25
19	-3.57***	0.28
20	-3.28***	0.27
21	-4.57***	0.58
22	-4.26***	0.58
23	-4.92***	1
Treatment Effect	0.03	0.05
LaPHIE Flag (Reference = Not flagged)		
Flagged by LaPHIE	0.70***	0.11

*** p<0.001, ** p<0.01, * p<0.05, ~ p<0.1

Table E.2: General Discrete-Time Hazard Model Results Hazard Regression Results, April

	Model 1		Model 2		Model 3	
	β	**SE**	**β**	**SE**	**β**	**SE**
Time[34]	< -1.8***	<1.1	< -1.9***	<1.1	< -1.8***	<1.4
Treatment Effect			0.08~	0.05	0.09~	0.05
Race (Reference = White)						0.00
Black/ African American					0.06	0.06
Other					-0.33	0.3
Ethnicity						
Hispanic or Latino					-0.21	0.22
Gender (Reference = Female)						
Male					-0.10~	0.06
Transgender					-0.06	0.33
Age						
Age at study entry					-0.02***	0.01
Age at HIV diagnosis					0.02***	
HIV Status (Reference = HIV positive (not AIDS))						
CDC defined AIDS					0.20***	0.06
Location of HIV Diagnosis (Reference = Other)						
Blood bank					0.1	0.39
Family planning/OBGYN clinic					0.2	0.53
HIV clinic/counseling and testing Site					0.09	0.35
Emergency room					0.62	0.4
Correctional facility					-0.31	0.37
Drug treatment center					-0.21	0.49
Inpatient facility/hospital					0.09	0.35
Unknown (out-of-state)					-0.44	0.36
Outpatient facility/clinic					0.1	0.35
Infectious disease /STD clinic					-0.1	0.36
Percent of Individuals in Zip Code:						
Living in poverty					-0.01	0.00
Without health insurance					0	0.01
Unemployed					-0.01	0.01
With at least a high school education					0	0.01
Who take public transportation					0	0.00

[34] For the sake of parsimony, we do not present findings for each discrete time variable, instead we present the range of coefficient and standard error values.

or walk to work				
Public Health Regions (Reference = Region One)				
Two			-0.48	0.38
Three			0.09	0.26
Four			0.06	0.39
Five			0.19	0.37
Six			-0.15	0.56
Seven			0.1	0.28
Eight			0.06	0.36
Nine			0.27	0.28
Health Service Areas (Reference = Interim)				
Earl K Long			0.31	0.54
Bogalusa			-0.1	0.49
Lallie Kemp			0.08	0.57
L.J. Chabert			0.18	0.57
University Medical Center			-0.25	0.54
W.O. Moss			-0.24	0.53
Goodness of Fit				
AIC	13778.2	13777.3	12903.4	
BIC	13969.6	13977.0	13406.8	
Likelihood-Ratio Test				
Treatment vs Constant		2.88		
Full vs Treatment			947.89	

*** p<0.001, ** p<0.01, * p<0.05, ~ p<0.1

Table E.3: Cubic Baseline Hazard Regression Results, April

	Model 1		Model 2		Model 3	
	β	**SE**	**β**	**SE**	**β**	**SE**
Constant	-1.76***	0.07	-1.80***	.07	-1.76*	0.86
Time						
Linear (time)	-0.28***	0.04	-0.28***	0.04	-0.28***	0.04
Quadriatic (time$^{2)}$	0.02***	0.00	0.02***	0.00	0.02***	
Cubic (time$^{3)}$	-0.00***	0.00	-0.00***	0.00	-0.00***	
Treatment Effect			0.08~	0.05	0.09~	0.05
Race (Reference = White)						
Black/ African American					0.06	0.06
Other					-0.33	0.3
Ethnicity						
Hispanic or Latino					-0.21	0.22
Gender (Reference = Female)						
Male					-0.10~	0.06
Transgender					-0.06	0.33
Age						
Age at study entry					-0.02***	0.01
Age at HIV diagnosis					0.02***	0.00
HIV Status (Reference = HIV positive (not AIDS))						
CDC defined AIDS					0.20***	0.06
Location of HIV Diagnosis (Reference = Other)						
Blood bank					0.09	0.39
Family planning/OBGYN clinic					0.19	0.53
HIV clinic/counseling and testing Site					0.09	0.35
Emergency room					0.62	0.4
Correctional facility					-0.31	0.37
Drug treatment center					-0.21	0.49
Inpatient facility/hospital					0.09	0.35
Unknown (out-of-state)					-0.44	0.36
Outpatient facility/clinic					0.1	0.35
Infectious disease /STD clinic					-0.1	0.36
Percent of Individuals in Zip Code:						
Living in poverty					-0.01	0.00
Without health insurance					0	0.01
Unemployed					-0.01	0.01
With at least a high school education					0	0.01

Who take public transportation or walk to work			0	0.00
Public Health Regions (Reference = Region One)				
Two			-0.48	0.38
Three			0.09	0.26
Four			0.06	0.39
Five			0.18	0.37
Six			-0.15	0.56
Seven			0.1	0.28
Eight			0.06	0.36
Nine			0.27	0.28
Health Service Areas (Reference = Interim)				
Earl K Long			0.3	0.53
Bogalusa			-0.1	0.49
Lallie Kemp			0.08	0.57
L.J. Chabert			0.18	0.56
University Medical Center			-0.26	0.54
W.O. Moss			-0.24	0.53
AIC	13779.3	13778.6	12899.7	
BIC	13812.6	13820.2	13246.3	
Likelihood-Ratio Test				
Treatment vs Constant		2.74		
Full vs Treatment			952.87	

*** p<0.001, ** p<0.01, * p<0.05, ~ p<0.1

August Samples

Table E.4: General Discrete-Time Hazard Model Results, Treatment Only August

Variable	β	SE
Time (Month)		
1	-1.93***	0.06
2	-2.50***	0.07
3	-2.52***	0.08
4	-2.74***	0.09
5	-2.68***	0.09
6	-2.68***	0.1
7	-2.74***	0.11
8	-3.01***	0.13
9	-2.90***	0.13
10	-3.29***	0.16
11	-3.25***	0.16
12	-3.12***	0.16
13	-3.48***	0.2
14	-3.51***	0.21
15	-4.13***	0.29
16	-3.69***	0.25
17	-3.57***	0.25
18	-3.59***	0.27
19	-3.78***	0.32
20	-3.32***	0.28
21	-3.54***	0.36
22	-4.20***	0.58
23	-4.12***	0.71
24	-3.86***	1.01
Treatment Effect	0.10*	0.05

*** p<0.001, ** p<0.01, * p<0.05, ~ p<0.1

Table E.5: General Discrete-Time Hazard Model Results, including LaPHIE Flag August

Variable	β	SE
Time (Month)		
1	-1.93***	0.06
2	-2.51***	0.07
3	-2.52***	0.08
4	-2.74***	0.09
5	-2.69***	0.09
6	-2.69***	0.1
7	-2.74***	0.11
8	-3.01***	0.13
9	-2.90***	0.13
10	-3.28***	0.16
11	-3.25***	0.16
12	-3.12***	0.16
13	-3.48***	0.2
14	-3.50***	0.21
15	-4.12***	0.29
16	-3.67***	0.25
17	-3.55***	0.25
18	-3.57***	0.27
19	-3.76***	0.32
20	-3.30***	0.28
21	-3.52***	0.36
22	-4.18***	0.58
23	-4.09***	0.71
24	-3.83***	1.01
Treatment Effect	0.05	0.05
LaPHIE Flag (Reference = Not Flagged)		
Flagged by LaPHIE	0.72***	0.12

*** p<0.001, ** p<0.01, * p<0.05, ~ p<0.1

Table E.6: General Discrete-Time Hazard Model Results, August

	Model 1		Model 2		Model 3	
	β	**SE**	**β**	**SE**	**β**	**SE**
Time	< -1.8***	<1.1	< -1.9***	<1.1	< -1.8***	<1.4
Treatment Effect			0.10*	0.05	0.11*	0.05
Race (Reference = White)						
Black/ African American					0.01	0.06
Other					-0.29	0.29
Ethnicity						
Hispanic or Latino					-0.47~	0.25
Gender (Reference = Female)						
Male					-0.14*	0.06
Transgender					-0.2	0.33
Age						
Age at study entry					-0.02**	0.01
Age at HIV diagnosis					0.02***	0.01
HIV Status (Reference = HIV positive (not AIDS))						
CDC defined AIDS					0.21***	0.06
Location of HIV Diagnosis (Reference = Other)						
Blood bank					0.46	0.45
Family planning/OBGYN clinic					0.34	0.58
HIV clinic/counseling and testing Site					0.49	0.43
Emergency room					1.05*	0.46
Correctional facility					0.12	0.44
Drug treatment center					0.12	0.56
Inpatient facility/hospital					0.48	0.42
Unknown (out-of-state)					-0.12	0.43
Outpatient facility/clinic					0.49	0.42
Infectious disease /STD clinic					0.4	0.43
Percent of Individuals in Zip Code:						
Living in poverty					-0.01~	0.00
Without health insurance					0.01	0.01
Unemployed					-0.01	0.01
With at least a high school education					0	0.01
Who take public transportation or walk to work					0.01	0.01
Public Health Regions (Reference						

= Region One)				
Two			-0.42	0.38
Three			0.09	0.27
Four			0.13	0.39
Five			0.2	0.38
Six			-0.14	0.57
Seven			0.15	0.29
Eight			0.03	0.37
Nine			0.37	0.29
Health Service Areas (Reference = Interim)				
Earl K Long			0.26	0.53
Bogalusa			-0.14	0.49
Lallie Kemp			0.08	0.57
L.J. Chabert			0.19	0.57
University Medical Center			-0.3	0.54
W.O. Moss			-0.24	0.53
Goodness of Fit				
AIC	12659.3	12657.2	11801.2	
BIC	12856.4	12862.6	12305.8	
Likelihood-Ratio Test				
Treatment vs Constant		4.09		
Full vs Treatment			930	

*** p<0.001, ** p<0.01, * p<0.05, ~ p<0.1

Table E7: Cubic Discrete-Time Hazard Model Results, August

	Model 1		Model 2		Model 3	
	β	**SE**	**β**	**SE**	**β**	**SE**
Constant	-1.80***	0.07	-1.85***	0.08	-2.33*	0.91
Time						
Linear (time)	-0.23***	0.04	-0.23***	0.04	-0.23***	0.04
Quadratic (time$^{2)}$	0.01**	0.00	0.01**	0.00	0.01**	0.00
Cubic (time$^{3)}$	-0.00~	0.00	-0.00~	0.00	-0.00~	0.00
Treatment Effect			0.10*	0.05	0.11*	0.05
Race (Reference = White)						
Black/ African American					0.01	0.06
Other					-0.3	0.29
Ethnicity						
Hispanic or Latino					-0.47~	0.25
Gender (Reference = Female)						
Male					-0.14*	0.06
Transgender					-0.19	0.33
Age						
Age at study entry					-0.02**	0.01
Age at HIV diagnosis					0.02***	0.01
HIV Status (Reference = HIV positive (not AIDS))						
CDC defined AIDS					0.21***	0.06
Location of HIV Diagnosis (Reference = Other)						
Blood bank					0.46	0.45
Family planning/OBGYN clinic					0.34	0.58
HIV clinic/counseling and testing Site					0.49	0.43
Emergency room					1.04*	0.46
Correctional facility					0.11	0.44
Drug treatment center					0.12	0.56
Inpatient facility/hospital					0.47	0.42
Unknown (out-of-state)					-0.13	0.43
Outpatient facility/clinic					0.49	0.42
Infectious disease /STD clinic					0.39	0.43
Percent of Individuals in Zip Code:						
Living in poverty					-0.01~	0.00
Without health insurance					0.01	0.01
Unemployed					-0.01	0.01
With at least a high school education					0	0.01

Who take public transportation or walk to work			0.01	0.01
Public Health Regions (Reference = Region One)				
Two			-0.42	0.38
Three			0.09	0.27
Four			0.13	0.39
Five			0.2	0.38
Six			-0.14	0.57
Seven			0.15	0.29
Eight			0.03	0.37
Nine			0.36	0.29
Health Service Areas (Reference = Interim)				
Earl K Long			0.26	0.53
Bogalusa			-0.14	0.49
Lallie Kemp			0.08	0.57
L.J. Chabert			0.19	0.57
University Medical Center			-0.3	0.54
W.O. Moss			-0.25	0.53
AIC	12665.1	12663.1	11803.5	
BIC	12698.0	12704.1	12145.3	
Likelihood-Ratio Test				
Treatment vs Constant		4.04		
Full vs Treatment			933.5	

*** p<0.001, ** p<0.01, * p<0.05, ~ p<0.1

Appendix F: Exploratory Findings

The exploratory analysis does not estimate an average treatment effect because we are no longer comparing all treatment individuals with all comparison individuals, but rather a subset of treatment with comparison individuals. Consequently these findings do not provide us with the same "causal" inference. They are still valuable in helping us discover the nature of the impact and whether – as this study hypothesizes – the effect is not just contingent on eligibility of exposure but also time spent in that status. It is worth emphasizing that the baseline risk portion of the model we use to produce Figure 2 in the text of the report is different from the one used in our benchmark model. For reasons of analytical ease we use the 3rd-order polynomial transformation of time. There are no covariates included in this model; again this is for ease of interpretation. However, additional analyses confirm that the results are substantively the same for all variations. The model itself is specified as:

$$Logit\left(t_{ij}\right) = \left[\alpha_0 C \;+\; \alpha_1 Month_j + \alpha_2 Month_j{}^2 + \alpha_3 Month_j{}^3\right] + \left[\beta_1 T_j + \beta_2 T * Month_j \;+\; \alpha\beta_3 T * Month_j{}^2\right]$$

The first set of brackets identifies the component of the model that is the predicted risk associated with discrete time. Instead of being specified as series of 24 dummy variables, as it is in the benchmark analysis, we have specified it as a cubic polynomial. This allows us to estimate the interactions in the second part of the model with more ease. As we show elsewhere, the cubic baseline produces substantively similar estimates to the general specification of time. The second set of brackets identifies the component that interacts time (months spent out of care) with treatment status and thus permits the estimated magnitude of the effect to change over time in the shape of a concave curve. Our decision to model the treatment interaction with time as a quadratic effect is based on hypotheses alone. Model goodness-of-fit statistics favor the fifth-order polynomial but that is difficult to conceptualize. Model estimates are presented below (Table F.1) followed by Table F.2, which presents the marginal effects – the predicted difference in probability between the treatment and comparison groups at each time point – and confidence intervals for these estimates at each time point. When the confidence interval is greater than zero the estimated effect is considered statistically significant. The p-value for time period 13 suggests that the confidence interval excludes zero but at a magnitude that is less than .000. Figure 2 in the text plots the marginal effects and confidence values that appear in Table F.2 graphically.

In Figure 3 (in text of report), we produce predicted probabilities for treatment and comparison groups with a slightly different model. This is for the sake of consistency. This model is simply the exploratory model (specifications above), with the first (baseline or time) component specified as it is in the benchmark model. Time periods, in other words are modeled as a set of 24 dummy variables.

Table F.1: Cubic Discrete-Time Hazard Model Results for Exploratory Analyses, April Sample

Variable	β	SE
Constant	-1.72***	0.09
Time		
Linear (time)	-0.31***	0.04
Quadratic (time$^{2)}$	0.02***	0.00
Cubic (time$^{3)}$	-0.00***	0.00
Treatment	-0.08	0.10
Treatment * Time	0.06~	0.03
Treatment * Time * Time	0.00	0.00

Table F.2: The Marginal Effects of LAPHIE on the Probability of Reengaging in care Over Time

Time (Month)	Marginal Effect	Standard Error	p-value	95% Confidence Interval	
1	-0.014	0.018	0.442	-0.048	0.021
2	-0.004	0.013	0.752	-0.029	0.021
3	0.003	0.009	0.708	-0.014	0.021
4	0.009	0.007	0.226	-0.005	0.023
5	0.012	0.007	0.063	-0.001	0.025
6	0.014	0.007	0.030	0.001	0.027
7	0.015	0.006	0.023	0.002	0.027
8	0.014	0.006	0.023	0.002	0.026
9	0.013	0.006	0.024	0.002	0.024
10	0.011	0.005	0.027	0.001	0.021
11	0.009	0.004	0.031	0.001	0.018
12	0.007	0.004	0.036	0.001	0.014
13	0.006	0.003	0.048	0.000	0.011
14	0.004	0.002	0.077	0.000	0.008
15	0.003	0.002	0.147	-0.001	0.006
16	0.002	0.002	0.289	-0.001	0.005
17	0.001	0.001	0.506	-0.002	0.004
18	0.000	0.001	0.749	-0.002	0.003
19	0.000	0.001	0.973	-0.002	0.002
20	0.000	0.001	0.840	-0.002	0.002
21	0.000	0.001	0.689	-0.002	0.001
22	0.000	0.001	0.567	-0.002	0.001
23	0.000	0.001	0.468	-0.001	0.001
24	0.000	0.000	0.386	-0.001	0.000